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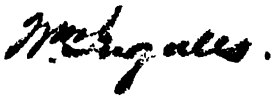

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THE

MEDICO-CHIRURGICAL REVIEW.

JULY, 1846.

LECTURES ILLUSTRATIVE OF VARIOUS SUBJECTS IN PATHOLOGY AND SURGERY. By Sir *Benjamin C. Brodie*, Bart., F.R.S., Serjeant-Surgeon to the Queen; Surgeon to H. R. H. Prince Albert; Foreign Correspondent of the Institute of France. Octavo, pp. 411. London: Longmans, 1846.

IT has often been noticed with regret that the eminent members of our profession who are engaged in extensive practice too frequently neglect the great opportunities they possess of enriching the annals of medical science and practice. This cannot be said of the distinguished author of the volume before us, who shows a due sense of the responsibilities of his position by occasionally recording, for the benefit of his brethren, the results of a large experience; and all must admit that his writings fully justify the great reputation which he enjoys, both in the profession and with the public. To the School of St. George's Hospital, which Sir Benjamin Brodie has always acknowledged himself indebted to for the opportunities of cultivating Pathology and Surgery, he has certainly never been wanting, and since he ceased to give a complete and systematic course of surgical lectures to the students of that institution, he has annually addressed to them a limited number of lectures on the same subjects, which have been continued since he resigned the office of surgeon to the hospital. We learn from the Preface that the present volume contains several of these discourses, which have already appeared in the weekly medical journals, but have undergone various corrections and received such additions as the author's later experience and more mature reflection have enabled him to furnish. Writings of the character of these Lectures are well adapted as a medium for the development of the opinions of an experienced observer, and they are more agreeable to read, and perhaps more profitable to practitioners, than discourses of a more systematic description. The novelty and interest of these Lectures are however much diminished by their previous publication. It sometimes happens that the writings of an author of no note when published in a weekly periodical, though displaying talent, are at the time little read and are soon forgotten, and their disinterment after the writer has acquired some celebrity may be desirable to establish his reputation. But the productions of Sir Benjamin Brodie are little likely to be thus neglected. We know that many of the principles of surgical practice inculcated in the work before us have been served up in

Druit's Vade Mecum, and quoted and taught in every medical school of the metropolis. We may instance the excellent lectures on Varicose Veins and Ulcers of the Legs, on Corns and Bunions, and on Mortification, which we will venture to say have already been more read in the journals than many works of greater pretension. We do not, however, make this remark as an objection to their publication in a collected form, for on the contrary, their popularity is the best proof that, in a corrected and enlarged form, they cannot fail to be acceptable to the profession. Besides these discourses originally addressed to the Students of St. George's Hospital, Sir Benjamin Brodie has also included two lectures, forming part of a course which he delivered as Professor of Anatomy and Surgery to the Royal College of Surgeons of England more than twenty years ago, and which had not been previously published.

The two first lectures are introductory addresses to the students of the Medical School of St. George's Hospital. The first was delivered in 1838, and the second in 1843. These discourses are admirable. They convey in language terse, clear and precise, valuable advice, seasonable hints and useful directions—in fact, just such hints and just such directions as are wanted by the majority of young men on commencing their educational career. They place before the medical student the proper objects and true ends of his professional education, and whilst representing the real difficulties of his course, they offer every encouragement and every incentive which should stimulate a virtuous and honorable ambition. Sir B. Brodie knew well the rock on which the fortunes of many men of ardent minds are shipwrecked, when penning the following passage:—

"Let it always be borne in mind that this last (whatever relates to Medical and Surgical treatment) is the real object which you have in view. I address you as future medical practitioners. If, taking another course, you choose to study anatomy and physiology, merely as interesting branches of human knowledge, you are at liberty to do so, and you will be as well rewarded for your labours as if you had applied yourselves to geology, optics, or astronomy. In like manner, if any one apply himself, as a philosopher, altogether to the study of pathology, he will find much in it that may interest himself, and that may be useful afterwards to those who carry their researches further. But as medical practitioners, you must not stop at either one or the other of these points; and, never losing sight of the ultimate object of all your investigations, you must estimate the value of whatever other knowledge you acquire by the degree in which you find it to be directly or indirectly applicable to the healing art." P. 4.

There is not a school in London which cannot furnish examples of poor victims of science, who, little heeding this friendly warning, have blasted their prospects and disappointed the hopes of their friends by acquiring habits of study and tastes for pursuits which have thoroughly unfitted them for the business of life as practitioners.

The second of these two addresses, on the duties and conduct of medical students and practitioners, is far superior to the first. When every page, indeed every sentence, in this Lecture contains some judicious counsel or valuable suggestion we feel at a loss to select passages to convey to our readers an adequate idea of its merits. We may quote the following observations on the disadvantages resulting from the neglect of education in early life:—

"One business of education is to impart knowledge; but another, and still

more important one, is to train the intellectual faculties. To acquire the habit of fixing the attention on the object before you; of observing for yourselves; of thinking and reasoning accurately; of distinguishing at once that which is important from that which is trivial; all this must be accomplished in the early part of life, or it will not be accomplished at all. Nor is the same remark less applicable to qualities of another order; integrity and generosity of character; the disposition to sympathize with others; the power of commanding your own temper; of resisting your selfish instincts; and that self-respect, so important in every profession, but especially so in our own profession, which would prevent you from doing in secret what you would not do before all the world; these things are rarely acquired, except by those who have been careful to scrutinize and regulate their own conduct in the very outset of their career." P. 31.

How encouraging, yet how just, are the author's reflections on the differences in the intellectual powers of competitors in the race of life.

"With respect, however, to the last-mentioned subject, I have no doubt that the difference is not so great as you, or the world generally, may suppose it to be. There are few persons who have not some talent, which, if properly cultivated, may be turned to good account, and he who is deficient in one kind of talent may excel in another. But the greatest talents may be wasted. They may be blighted by indolence; they may be used for base or improper purposes; or they may be directed to too great a variety of objects. It is well indeed for you to have some diversity of study, so as to keep all your mental faculties in wholesome exercise; so that you may not be without some sympathies with those around you, and that you may avoid the evils of narrow-mindedness and prejudice; still, whoever would be really useful in the world, and be distinguished in it, must act to a great extent on the principle of concentration, keeping one object especially in view, and making his other pursuits subservient to it. And let no one sit down in despair and say, 'I have not the abilities of my neighbours, and it is needless for me to exert myself in competition with them.' If you would know what your own powers are you must try to use them. Industry is necessary to their development; and the faculties of the mind, like those of the body, go on improving by cultivation. It is impossible for you to form a right estimate of yourselves in early life, nor can you be rightly estimated by others. The self-sufficient, who do not keep before their eyes an ideal standard of perfection, who compare themselves only with those who are below them, will have an advantage with inexperienced and superficial observers; but I must say that I have never known any one to do any real good in the world, or obtain ultimately a bright reputation for himself, who did not begin life with a certain portion of humility. The greatest men are humble. Humility leads to the highest distinction, for it leads to self-improvement. It is the only foundation of a just self-confidence. Study your own characters; endeavour to learn, and to supply your own deficiencies; never assume to yourselves qualities which you do not possess; combine all this with energy and activity, and you cannot predicate of yourselves, nor can others predicate of you, at what point you may arrive at last." P. 35.

Let the slave to the opinions of others, as well as the hunter after posthumous fame, remember—

"There is no profession in which it is more essential that those engaged in it should cultivate the talent of observing, thinking, and reasoning for themselves, than it is in ours. The best part of every man's knowledge is that which he has acquired for himself, and which he can only to a limited extent communicate to others. You will spend your lives in endeavouring to add to your stores of information; you will, from day to day, obtain a clearer and deeper insight into

the phenomena of disease; you will die at last, and three-fourths of your knowledge will die with you; and then others will run the same course." P. 39.

The young practitioner is told—

"He who has not a full sense of the responsibilities which it involves, is unfit for our profession; and the anxieties of a professional life are but a wholesome stimulus to diligence and exertion. I say this, supposing them to be kept within reasonable bounds. You may allow your thoughts to dwell on subjects of anxiety until an entirely opposite effect is produced, and life is rendered miserable, and the mind enervated. Such a morbid sensibility is as mischievous on the one hand, as a want of just sensibility is on the other. You must be careful to train the mind so that it may not fall into either of these extremes. Make every exertion to obtain knowledge, and to use it properly; and then keep it in your recollection that there are bounds to human knowledge, and to human powers; and that, in the exercise of our art, we cannot do all that is required of us; for, if we could, pain and misery would be banished from the world, man would be immortal, and the order of the universe would be disturbed. Do not begin life with expecting too much of it. No one can avoid his share of its anxieties and difficulties." P. 43.

We cannot deny ourselves the pleasure of giving one more passage, the spirit and tendency of which exhibit the good sense and good feeling of the writer.

"There are some employments which bring those who are engaged in them in contact more especially with the bad qualities of mankind, their pride, their arrogance, their selfishness, their want of principle. It is not so with your profession. All varieties of character will be thrown open to your view; but nevertheless, you will see on the whole the better side of human nature; much indeed of its weakness, much of its failings, much of what is wrong, but more of what is good, in it. Communicating, as you will probably do, with persons of all conditions, you will be led to estimate others according to their intrinsic qualities, and not according to those circumstances which are external to themselves. You will learn, that of the various classes of which society is composed, no one is pre-eminently good or pre-eminently bad; and that the difference is merely this, that the vices and virtues of one class are not exactly the vices and virtues of another. You will have little sympathy with those prejudices which separate different classes from each other; which cause the poor to look with suspicion on the rich, and the rich to look down upon the poor; and while you cannot fail to perceive the great advantages which education gives, you will acknowledge, that, to be well educated, is not the necessary result of having the opportunity of education; that a bad education is worse than none at all; and that what is called the uneducated classes present many examples, not only of the highest religious and moral principles, but of superior intellect, and of minds stored with valuable knowledge." P. 52.

We need scarcely add that we strongly recommend the perusal of these addresses, especially of the second, to all medical students and young practitioners.

The third Lecture is on the effects of Strangulation. Sir B. Brodie states—

"The mode of death from strangulation or hanging is sufficiently obvious. 1. The trachea is obstructed, so that air cannot enter the lungs. 2. The blood passing through the lungs does not undergo that change which respiration produces, and which is necessary to life. 3. Dark-coloured blood, which has not been purified by exposure to air, is transmitted to the left side of the heart, and

from thence to the brain and other organs. 4. The heart continues to act, circulating dark-coloured blood, but its actions gradually become weaker, and, in the course of a very few minutes, cease altogether." P. 61.

The author's observations on the immediate cause of the cessation of the heart's action, and on the length of time the heart continues to act so as to circulate blood, after respiration has ceased, are full of interest. The description given of the symptoms which arise in the different stages of strangulation, though founded chiefly on experiments made on the lower animals, is no doubt a faithful representation of the phenomena occurring in the human body. Under the head of treatment to be had recourse to in cases of strangulation, excellent directions are given for making artificial respiration, and as there are many occasions besides those of strangulation in which practitioners may be unexpectedly called upon to conduct this process, and as erroneous notions prevail as to the best means of doing it, we may briefly notice Sir B. Brodie's views on the subject. We believe him to be quite right in stating that the only effectual method of supplying the want of natural respiration is that of inflating the lungs, by a pair of bellows, or a syringe, or some similar contrivance. The operator is repeatedly cautioned against the danger of too forcible distension of the air-cells and the impulsion of air into the blood-vessels, through the thin tunics of the capillary vessels of the lungs. It is stated, "under ordinary circumstances, there are three inspirations for one pulsation of the heart and arteries." This is an error, of course accidental, which is too obvious to mislead. Sir B. Brodie no doubt intended to write *there is one inspiration to three pulsations of the heart and arteries*. In cases of obstruction from inflammation and swelling of the mucous membrane of the pharynx and larynx, the lungs can be inflated only by means of a tube introduced into the trachea, and the simplest mode of proceeding is to make an opening with a double-edged scalpel in the space between the cricoid and thyroid cartilages, this situation being more convenient (where time is valuable) than one lower down, on account of the smaller quantity of dissection which is required for the exposure of the trachea in the former case, and the greater liability to hæmorrhage in the latter. We are glad to find that our author does not recommend the curved trochar and canula, which is now sold by most instrument-makers, and is used by some of the surgeons to the Borough Hospitals. We are convinced that this is a very dangerous instrument, its use being attended with considerable risk of wounding the posterior part of the larynx or trachea, and of making an opening into the œsophagus. In cases of strangulation it is not necessary to introduce a tube into the trachea. A tube introduced into one nostril will answer every purpose, and it may be passed without delay. "A short ivory tube, of the size of the anterior aperture of the nostril, and with the projecting rim, to prevent it slipping in beyond your reach, is very convenient for the purpose; but this may not be at hand; and you will find a large elastic gum catheter; a piece of card rolled into a cylinder; or the nozzle of a pair of bellows, to answer the purpose well enough, and at all events any one of these may be used until you are provided with a better apparatus." But bellows may not be at hand, the surgeon must then be content, in the first instance, to inflate the lungs by his breath, or

by that of another person, with the aid of a gum tube, or any other tube that can be procured. The upper part of the patient's person should be exposed, and the operator will then know, by observing a gentle elevation of the chest and abdomen, that the lungs are sufficiently inflated. It is needless to give directions as to the exact admeasurement of the air. However good this may be in theory it is practically impossible. A portion of the air escapes by the other nostril and the mouth, and these apertures must not be closed, as they form a safety-valve which will do more towards preventing the over-distension of the lungs than the most ingenious apparatus. The operator must bear in mind that the quantity of air which is required at each artificial inspiration is not only not greater, but that it may even be less, than in natural breathing. An assistant should press the thyroid and cricoid cartilages against the vertebræ, so as to close the upper extremity of the œsophagus, otherwise a portion of air will, at each closure of the bellows, find its way into the stomach, gradually distending it until it prevents the descent of the diaphragm and the entrance of the air into the lungs altogether. Voltaic electricity, which has been recommended in these cases, is shown to be quite inapplicable in surgical practice. A pair of bellows may be obtained anywhere; but a voltaic battery is not to be had at the moment, and requires time for being brought into operation. Sir B. Brodie remarks:—

"It cannot be too strongly impressed on your minds that in these cases for the most part there is no time to be lost. If the natural efforts to respire have actually ceased, the cessation of the heart's action will take place in the course of a very few minutes. If the patient recover from the first effects of the strangulation, but lies with stertorous respiration, and other symptoms of apoplexy, he may cease to breathe altogether at any moment: *and if that action of the heart by which the circulation is maintained should cease, as a consequence of the suspension of respiration, it can never be restored. This I positively assert, after having made it the subject of a very careful investigation.* If others have held a different opinion, it is because they have confounded those feeble and irregular contractions of the heart, which may last for a long time, but which mean nothing, with those regular and powerful movements which are necessary to propel the blood through the system. The most probable means of restoring the action of the heart would seem to be the application of voltaic electricity. But Bichat distinctly states that it has no influence whatever over the involuntary muscles; and without venturing either absolutely to confirm, or absolutely to deny this assertion, I am bound to say, after having made many experiments on the subject, that when the mode of death is that to which the name of asphyxia has been commonly but (according to its etymology) most absurdly and improperly applied, the application of electricity in any form to the heart is altogether useless." P. 82.

Sir B. Brodie is not favourable to venesection in the treatment of strangulation, and is also of opinion that as much benefit may be obtained from keeping the patient in an atmosphere of a moderately warm temperature as from placing him in a warm bath.

In regard to the mode of death from drowning, which is treated of in the fourth Lecture, it is stated to be similar to death the consequence of strangulation; and the want of the due oxygenation or decarbonization of the blood is the sole cause of the animal's destruction. The following is

a good account of the phenomena of drowning, as observed on immersing a small animal in water.

"There is first a deep expiration, by which bubbles of air are expelled from the lungs. There is then an effort to inspire; but the effort is ineffectual, there being no air which can be received into the lungs; and a spasm of the muscles seems to prevent the admission of water in any considerable quantity into the trachea. The attempts to breathe are repeated several times; and after each attempt a small quantity of air is expelled from the mouth and nostrils, until the air-cells of the lungs are almost completely emptied. Then the animal becomes insensible, and convulsive actions of the muscles mark the instant when the brain begins to suffer from the influx of the dark-coloured blood. After these convulsions the animal is motionless, and gives no signs of life; but if the hand be applied to the thorax the pulsation of the heart, gradually becoming fainter and fainter, indicates that some remains of vitality still linger in the system. Before the circulation ceases altogether, the muscles of respiration resume their action, and some ineffectual efforts are again made to breathe. It is a remarkable circumstance that the diaphragm continues to exert itself nearly as long as the heart itself, so that the interval between the cessation of the attempts to breathe and the cessation of the motions of the heart, short as it is in animals that die of strangulation, is shorter still in those that perish from drowning. These phenomena follow each other in rapid succession, and the whole scene is closed, and the living animal is converted into a lifeless corpse, in the brief space of a few minutes." P. 86.

Sir B. Brodie believes that the heart is very rarely, if ever, capable of maintaining the circulation for a longer period than five minutes after complete submersion. After remarking that a spasm of the muscles of the glottis seems to prevent the admission of air into the windpipe, he notices some experiments made on animals, and which prove that a very small quantity of water obtains admittance, and states that there is no manifest reason why this should be very injurious.

In his experiments Sir B. Brodie found the action of the heart to be generally more feeble in animals which are drowned than in those which are strangled; and he knows not to what this difference can be attributed, unless it be the additional shock which the former species of death occasions to the nervous system, in consequence of the immersion of the animal in a cold medium which rapidly carries off the animal heat. He agrees with all sound physiologists in totally disbelieving the tales related by travellers of divers who have been capable of remaining under water for twenty minutes, or even for a longer period. Dr. Davy, who resided for a considerable time in Ceylon, found on inquiry that the average time of diving is less than a minute. Similar errors prevail respecting the time that persons have been restored to life after submersion. We are informed in a note that Mr. Woolley, the surgical attendant at the Receiving House of the Royal Humane Society in Hyde Park, believes that very few lives are preserved after four minutes of complete submersion. Two instances, however, of persons who have recovered after having been five minutes under water, have come under his observation.

As the mode of death from drowning is the same as from strangulation, so there must be a great similarity of treatment. We find, however, no suggestions on this subject but what are well known to the profession. Sir B. Brodie confesses, however, "on the whole it must be acknowledged

that in both orders of cases the resources of art are limited, and that of those who recover from a state approaching to dissolution a greater number will owe their recovery to unassisted nature, than to the most judicious treatment."

With reference to the mode in which death is produced by a stroke of lightning, Sir B. Brodie remarks, that the facts which he has been able to collect relating to this subject, lead him to the conclusion, that the influence of lightning, or of a powerful shock of electricity, in the majority of cases, is expended chiefly in disturbing, or destroying, the functions of the brain.

It has given us pleasure to call attention to these two lectures, delivered at the Royal College of Surgeons, because Sir B. Brodie's later labours in practical surgery have somewhat eclipsed his former, but scarcely less important, labours in the field of physiology.

In Lecture V. a short account is given of some cases of cysts containing watery fluid, apparently connected with the liver. Two cases are related of fluctuating tumours occurring on the right side, and lifting up the ribs. They were tapped and a watery fluid evacuated. In each instance the wound closed by the first intention, and there was no return of the fluid. It is supposed that these cysts were connected with the liver, and that they were analogous to the cysts occasionally developed in the neck, and also in the testes and spermatic cord in encysted hydrocele. A third case of the kind, in which after puncture the cyst suppurated, and burst into the colon, is also detailed.

In Lecture VI., which treats of ununited fractures, Sir B. Brodie describes the process of union of a fractured bone, but makes no addition to what has long been known on this subject. He next considers the circumstances under which fractures do not unite, noticing most of the causes usually described by surgical writers as preventing a bony union. He remarks—

"In most instances, I cannot doubt that the want of union is to be traced to a peculiar state of the constitution. A gentleman was growing fat, and not liking to do so, he placed himself on a very spare diet, though accustomed to good living previously. After six months of starvation, he broke his arm, and the bone would not unite. I saw him many months afterwards, and there was scarcely any union, even by soft substance. Another patient about whom I was consulted, a lady, also was growing corpulent, and she also thought that she might prevent it, by pursuing a similar system of diet. Some months afterwards she broke her fore-arm, and union did not take place. A young man had been for many months living very low on account of a complaint under which he laboured, and under these circumstances broke both bones of his fore-arm. At the end of several months there was no union. Cases of ununited fracture are not very common, yet here are three among those which have fallen under my observation, in which the want of union seems clearly to be traced to the bad state of the constitution, produced by abstinence from food." P. 125.

He thinks that, in some cases, too tight bandaging by interfering with the supply of blood to the limb, may prevent the union of a fracture. This is very probable. In some experiments on rabbits and other animals he found, after fracturing the femur, and then tying the femoral artery, that

until the end of the seventh day there was no commencement of the process of union. But after this period union goes on as usual, owing, it is supposed, to the anastomosing branches having become sufficiently dilated to make up for the obliteration of the femoral artery. Mr. Travers, however, in his work on Constitutional Irritation, has recorded a case of fractured femur, accompanied with injury to the popliteal artery, in which, although the femoral artery was tied, the fracture was soundly united in six weeks. Sir B. Brodie does not notice the effects of one end of the fractured bone being cut off from its supply of blood from its nutrient artery, as interfering with the union in some cases, a circumstance pointed out, if we recollect rightly, in a paper published a few years back in the Transactions of the Medico-Chirurgical Society.

Sir B. Brodie reviews the various means which have been employed for the purpose of procuring a union of the fracture. He gives no sanction to the operation of cutting down on the broken ends of the bones, and sawing off a portion of each of them, and supposes that no modern surgeon, having a moderate share of prudence, would undertake it. In reference to the introduction of the seton in these cases, he remarks that the result of the practice in this country appears to be, that sometimes it has succeeded in the upper extremities, but that where it has been performed on the lower extremities, as far as he knows, it has only succeeded in a single instance. The operation is uncertain, and the result tedious. Sir B. Brodie speaks favourably of the treatment by pressure proposed by Mr. Amesbury, and states that it succeeded perfectly in three cases which were attended with him. The success, in one of these at least, was not so complete as is represented, since it appears that there was so much yielding motion between the upper and lower portions of the fractured femur, "that it was plain that the union could be merely ligamentous." In this mode of treatment the pressure must be considerable, so as to cause some inconvenience to the patient, both from pain and from swelling of the limb below. But the inconvenience is only temporary.

"The principle of Mr. Amesbury's practice is simply that of keeping the ends of the bones in perfect repose, and at the same time applying pressure, particularly on the broken surfaces, so as to keep them in the closest possible contact with each other. Of course no general rule can be laid down as to the mode of attaining this object. In a case of transverse fracture, one kind of apparatus must be employed, in one of oblique fracture another, and in one of comminuted fracture a third. The apparatas will also differ accordingly as it is a fracture of the arm, the fore-arm, the leg, or the thigh. In a case of oblique fracture a very simple apparatus will do all that is required. Secure the limb by fastening it to a single rather broad wooden splint. Apply a pad of thick leather on each side of the fracture, and then a tourniquet, by which the two opposite surfaces of bone may be kept firmly squeezed against each other. By means of the tourniquet the pressure may be easily regulated, and increased or diminished as the patient can bear it. The best kind of tourniquet is not the common one, known under the name of Petit's, but one which occupies a smaller space, invented by the late Mr. Savigny, and sold by Philip and Whicker in St. James's Street.

"I do not say, however, that this method always succeeds. I had tried it in the case of the little boy whose case I have already mentioned, (on whose leg I afterwards used the seton,) and without advantage. There was another patient in this hospital on whom it was tried for a considerable time under Mr. Amesbury's observation, and no union was effected: and it appears that Mr. Ames-

bury has met with some cases in his own private practice, in which he has adopted it, and no doubt done ample justice to it, but in which it has failed. Still it has proved a very successful method on the whole, and certainly very much more successful than any other." P. 135.

The subject of Lecture VII. is sero-cystic tumours of the female breast. This is the most complete account of the disease that we have met with. This affection was improperly termed by Sir A. Cooper "hydatid disease of the breast," its pathological character being quite distinct from true hydatid formations, which occasionally occur in the female mamma. The first perceptible indication of the disease is a globular tumour imbedded in the glandular structure of the breast, and to a certain extent moveable underneath the skin. Sometimes there is only one such tumour; at other times there are two or three, or many more. In most instances the disease is confined to one breast. There seems to be little doubt that the cysts are originally formed by a dilatation of the lactiferous tubes. The diagnosis is generally easy, but occasionally the tumour is so deeply seated, that even a very experienced person may not at once recognise its nature in the first instance, and may be led to suppose that it is a medullary tumour or a chronic abscess, or any thing else, rather than what it really is. It occurs at the middle period of life, and is more common in single than in married women. The pathological history of the disease is thus described:

"*First:* A greater or less number of membranous cysts are generated in the breast, containing serum. The latter is at first of a light yellow colour, and transparent, but afterwards becomes of a darker colour, and opaque. There is reason to believe that these cysts are formed by a dilatation of portions of some of the lactiferous tubes.

"*Secondly:* Morbid growths or excrescences are generated from the inner surface of one or more of these cysts, projecting into their cavities. These excrescences seem to consist of albumen or fibrine, which, after some time (if not immediately,) becomes organized. They are covered by a thin delicate membrane, which is reflected over them from the inner surface of the cyst; but whether they are originally formed between two layers of the membrane of the cyst, or whether they are at first mere deposits of fibrine or albumen on the inner surface of the cyst, a thin membrane being formed on their surface afterwards, remains to be determined by future observations.

"*Thirdly:* There is some reason for believing that a similar growth of fibrinous substance may take place from the external surface of the cysts, connecting different cysts with each other; but this point also may, perhaps, require to be illustrated by further investigations.

"*Fourthly:* Under certain circumstances the cysts become completely filled up by the morbid growths, so that their cavities are obliterated, the tumour being thus converted into a solid mass, in which, however, the remains of the cysts are perceptible; and this is the prelude to a still further change, in which the greater part of the cysts have wholly disappeared, a solid mass of an indistinctly laminated texture occupying their place.

"*Fifthly:* If one of the membranous cysts be artificially laid open, or if it burst from over-distension with serum, the fibrinous excrescence, from its inner surface being no longer restrained by the pressure of the skin, increases in size, and protrudes externally in the form of a fungus, giving to the tumour a new and more formidable character.

"In this last stage of the disease, it is evident that spreading ulceration, sloughing, and hæmorrhage, the usual results of an ulcer occurring in a diseased

structure, must ensue, for which our art furnishes no other means of cure than the removal of the affected parts by a surgical operation." P. 149.

In the early stage of this disease, the treatment recommended, is the application of a stimulating embrocation to the skin. In the majority of cases thus treated, the tumour or tumours have entirely disappeared; in others, without disappearing altogether, they have become very much reduced in size; and, in only a few instances, in which the treatment was not very rigidly pursued, it was productive of no manifest advantage. The application generally employed is the following:—

"℞. *spiritûs camphorati, spiritûs tenuioris*, ãã. ℥ iijss; *liquoris plumbi diacetatis*, ℥ j.; *fiat embrocatio.*

"I have directed the patient to soak a piece of flannel, once folded, in this embrocation, and to apply it so as to cover that part of the breast in which the tumour is situated, renewing the application six or eight times in the day and night, until the skin becomes inflamed; then to omit the application for two or three days, but to resume the use of it as soon as the inflammation has subsided. The period of time during which it is necessary to pursue this method of treatment varies in different cases. In some, all that can be desired is accomplished in the course of three or four weeks; in others, it must be continued, with occasional intermissions, for some months. Other stimulating applications may be occasionally substituted for that which I have just mentioned. Several blisters may be applied in succession; each of them being kept open for a few days with the savine cerate; or a solution of ℥ j. of iodine in ℥ j. of alcohol may be applied to the skin once or twice daily, by means of a large camel's hair brush. On the whole, however, I am led to believe that the embrocation is more efficient than any thing else." P. 153.

But this simple treatment is wholly inefficient after the growth of solid substance has commenced. In this more advanced period of the disease, no good is to be expected, except from the removal of the entire breast; and such an operation may be had recourse to with every prospect of success, the disease being entirely local. Sir B. Brodie states that it undoubtedly is not malignant. It may go on to inflammation and ulceration, and the ulcer may spread and slough and bleed, but it does not contaminate the constitution. He adds, however, "still, I am not prepared to say that it may not, under certain circumstances, and in peculiar constitutions, assume a malignant character; this being no more than may happen to almost any morbid growth." That it may in an advanced stage acquire a true carcinomatous character we have no doubt whatever; but we believe that this is only an occasional, and not a common, result of the disease.

In Lectures VIII. and IX. Sir B. Brodie treats of Varicose Veins and Ulcers of the Legs, the formation and pathological causes of which, he describes with his usual accuracy. We find nothing, however, but what is well known to practical surgeons. In speaking of the application of plaster in these cases he recommends the soap plaster spread on the *fungus* or *amadon* used for the lighting of cigars. Being to a certain degree elastic, it admits of being applied in a single piece and makes a very uniform pressure. This plaster is sold by Mr. Weatherfield, in Henrietta-street, Covent-garden. Sir B. Brodie states that, in private practice, he frequently recommends a bandage which is made of stocking web, which, we quite agree with him, is very convenient, being easily applied, and making a very

equal pressure: indeed, for comfort and efficiency, we believe this bandage to be far preferable to the best made elastic stockings. He adds, that it cannot well be used by the poorer classes of society, being more expensive in the first instance, and being also good for nothing after it has been washed a few times. This is a mistake. A coarser kind of bandage well adapted for poor persons may be purchased for a shilling, and it will last six months, and bear washing many times. Sir B. Brodie gives no countenance to operations on the vena saphena, and mentions cases in which, after division of, or after tying this vein, the patients have died from venous inflammation. He remarks—

"There are indeed no circumstances here to justify the performance of a dangerous operation. You may perform such operations to get rid of a disease still more dangerous, but you have no right to perform an operation attended with such a degree of danger as can be appreciated, in order to get rid of a disease which is not dangerous; and no one can say that varicose veins belong to the class of dangerous diseases. But still there is another reason against having recourse to this operation. I do not believe, from what I have formerly seen, that it permanently benefits the patients. It is true that they appeared to go out of the hospital much relieved; but where I had the opportunity of seeing them one or two years afterwards, I always found them as bad as ever. Indeed I am by no means certain that the benefit which the patients appeared to derive, in the first instance, was the result of the operation; and I am more inclined to believe that it arose from their having been necessarily kept for some time in bed in the horizontal posture. Patients with varicose veins always seem to improve under these circumstances. But I may observe further, that there appears to be no reason why, in ordinary cases of varicose veins, the obliteration of the *saphena* should do any good, and that there are better grounds for believing that it will do harm. I have already explained to you that pressure on large venous trunks causes an obstruction of the blood in passing through them, and that this is one common cause of varices." P. 186.

Lecture X. contains some interesting observations "on the Cases of Scirrhous Tumours of the Breast which require an Operation." Sir B. Brodie notices the difference of opinion that has existed amongst surgeons of eminence as to the propriety of operating in scirrhous disease of the mamma, and after insisting on the importance of removal of the entire mamma in cases in which an operation is considered desirable, he remarks, "you may divide scirrhous tumours of the breast into two classes: one, where there is a conversion of the gland of the breast itself into the scirrhous structure, there being no well-defined margin to it; the other, where there is a scirrhous tumour imbedded in what appears to be otherwise a healthy breast, as if it were altogether a new growth, there being a well-defined boundary to it." In the first order of cases, the operation not only never succeeds in making a permanent cure, but it rather hastens the progress of the disease. The patient dies within two or three years, and probably much sooner, from an effusion of fluid into the cavity of the pleura. In another order of cases, where the skin is contaminated, there is no chance of the operation making an ultimate or permanent cure. The retraction of the nipple and tucking in of the skin are also regarded as unfavourable symptoms, and forming objections to the operation. The presence of indurated glands in the axilla, the adhesion of the scirrhous tumour to the pectoral muscle or ribs, indications of malignant disease in

other organs, are all circumstances sufficient to forbid an operation with a view to an ultimate cure. Having taken away these cases, there are very few left in which it is right to propose an operation as affording a chance of permanent cure. An operation is proper in cases free from the objections here mentioned. It may be performed, too, with a still better prospect of success in cases of scirrhous tumour unconnected with the breast though attached to it, and also in cases of scirrhous tumour of the nipple. Our author remarks that there may be circumstances which justify the surgeon in performing the operation for the removal of a scirrhous tumour of the breast, not in the expectation of a permanent cure, but with the view to afford the patient a respite and relief from present suffering, and he mentions several cases in which life appeared to have been prolonged by an operation.

"Of course you are here called upon to exercise no small degree of discrimination; and especially you should reject altogether those cases in which the skin is distinctly contaminated by the disease, whether it be that there are scirrhous tubercles in it, or that it be converted into the brawny structure which I have formerly described. In neither of these cases will the patient obtain even a respite by submitting to an operation." P. 206.

Sir B. Brodie observes that the operation is not free from danger, and admits that he has lost patients after it. He adds, however, "But where the breast is small; where the patient is otherwise healthy, and not much advanced in life; and where you do not starve the patient either before the operation or after it; and are also careful that there shall be as little loss of blood as possible;—there the danger of the operation is comparatively trifling."

Lecture XI. is on Corns and Bunions, a subject which Sir B. Brodie has not disdained to treat of—and though these affections have been described by fashionable chiropodists in handsome quarto works, we believe that the brief observations of Her Majesty's Serjeant-Surgeon contains the best description of them, and the best directions for their relief and cure which have hitherto been published. Those of our readers who may suffer from corns or bunions, or who may wish for information as to their causes and treatment, cannot do better than read this lecture.

The subject considered in Lecture XII. is one of considerable interest—the administration of mercury in cases of syphilis. We are glad to perceive that Sir B. Brodie rejects the notion of treating this disease without mercury, and of mercury aggravating the disease instead of curing it. Experience proves to him that we have hitherto found no remedy having the same power of extinguishing the venereal poison as mercury. In answer to the objection that it may do great harm, he justly remarks, "In this there is nothing at all remarkable, for (with the exception, perhaps, of sarsaparilla,) I do not know any medicine capable of doing great good, that may not, under certain circumstances, operate as a poison. I saw a gentleman very nearly killed by an over-dose of quinine: others have died in consequence of the imprudent exhibition of the iodide of potassium; and others have been killed by arsenic. A remedy that is strong enough to do good is almost invariably strong enough to do harm, if it be not used

properly." He states that mercury is not to be given as a matter of course in all cases of syphilis; but the general rule is, that it should be given. We think this remark might have been qualified by the addition of "in the primary and secondary stages of the disease," for, in tertiary syphilis, we quite agree with Ricord, that mercury is generally inapplicable. Sir B. Brodie next describes several conditions in which it would not be right to give this remedy. In reference to the best mode of administering it, he, in common with most good practical surgeons, gives the preference to inunction in cases where the symptoms of syphilis are not of the very mildest character. He seems to think that we have rather gone back than advanced in the treatment of this disease, and that the mercurial treatment as employed by the late Mr. Pearson during the greater part of his life, was as nearly perfect as possible, and that it was much more successful than the less careful treatment of modern practitioners. There is certainly much force in these remarks. If mercury be necessary, it should be given so to affect the system as to eradicate the disease, or it will do more harm than good. If the constitution cannot bear this action of the remedy, then do not give it at all. No advantage is gained in attempting to get rid of the disease or to keep the symptoms under by small doses of mercury. They become more confirmed and more difficult of cure when the disease is treated in this way, and there are other remedies which are more efficacious as palliatives, and at the same time do less injury to the system than mercury. Our author's observations on the treatment of syphilis in infants are deserving of attention.

"Children are sometimes born with syphilis, the father or mother having been affected with it. The child looks thin, and is of small size; and, instead of thriving, becomes thinner and thinner. At the end of three weeks it is covered by a red scaly eruption; there are aphthæ in the mouth, with chaps about the lips and the anus. The symptoms are well marked, and tell you at once the nature of the disease. I have tried various ways of treating such cases. I have given the gray powder internally to the child, or some kind of mercury to the wet nurse. But the mercury given to the infant by the mouth gripes and purges severely; that given to the wet nurse cannot be depended on; and at all events the latter is a very cruel and scarcely justifiable practice. The mode in which I have treated such cases for some years past is this: I have provided a flannel roller, on one end of which I have spread some mercurial ointment,—say a drachm, or more; and I have applied the roller, thus prepared, not very tight, round the knee; repeating the application daily. The motions of the child produce the necessary friction: and the cuticle being thin the mercury easily enters the system. This causes neither griping nor purging; in a child it does not even in general cause soreness of the gums; but it cures the disease. Very few of those children ultimately recover in whom the mercury has been given internally; but I have not seen a single case in which this other method of treatment has failed." P. 245.

As mercurial fumigations have been recommended by a modern writer on Syphilis, our readers will like to know the opinion of this method of treatment entertained by Sir B. Brodie. He states, "I have used the mercurial vapour-bath with success in several cases where it was my object at once to affect the system; but I have found that Mr. Pearson's objection to it is very well founded; namely, that it is difficult in this way to regulate the mercurial action. You may affect the system too much, or

too little; and you may be taken unawares by the patient's gums becoming all at once excessively sore." In the review of Mr. Parker's work on Syphilis, in our preceding number (p. 456,) we expressed a similar opinion.

The following remarks, with which this Lecture is concluded, are so true and practical that we cannot forbear quoting them :—

"I have spoken of the necessity of administering mercury, not only till the symptoms are relieved, but for a considerable time afterwards. But you may ask whether a long course of mercury be not more likely to injure the constitution than a short one? Undoubtedly it is; and that is the very reason why you should prefer a long course. If the course be a short one, the disease is sure to return; you have then to repeat it, and again the disease reappears. Thus you have repeated courses; and not only is the system weakened by the mercury, but the disease, whenever it does return, assumes a more formidable character than before. But if, on the other hand, you put the patient through a long course in the first instance, such a frequent recurrence to the use of mercury will be unnecessary. A patient who takes mercury for a chancre, for a month or five weeks, may probably never want it again; but if he take it only for a fortnight, he has secondary symptoms, and then it will be required for at least six weeks, perhaps for ten; so that that which is a short course at first becomes a long one in the end." P. 249.

Tic Douloureux, or facial neuralgia, forms the subject of Lecture XIII. The symptoms are briefly but graphically described. It is worthy of notice that the disease effects only one side of the face. Sir B. Brodie has never met with a case in which it affected both sides. Diseased teeth have been supposed to give rise to this painful affection, but he never knew a case in which a patient was relieved of a genuine *tic douloureux* by the extraction of a tooth. Cases in which it appeared to depend on diseased bone are mentioned, but these are rare occurrences. "It is a great mistake to suppose that diseased or dead bone is an ordinary cause of this disease." Various other sources to which the disease may be traced are noticed, as hysteria, disorders of the digestive organs, and organic disease in the brain. Sir B. Brodie observes :—

"There are many cases in which you cannot trace the *tic douloureux* to its real source. There is something or another, somewhere or another, in the system, which acts as a source of irritation to the nerves of the face; but where that something is, and what it is, we cannot discover. Indeed, generally speaking, I should say that to trace local nervous affections to their origin is one of the greatest difficulties that we meet with in the practice of our art. The disease may be in one part of the body, and the pain or spasm which it produces may be in another. I have known a patient have neuralgia of the foot, which depended on a stricture of the urethra, and which, whenever it occurred, was invariably relieved by the use of a bougie. I have known another patient have neuralgia of the foot depending on internal piles, which came on when the piles were protruded through the anus, and went away when they were reduced. I have known a spasmodic wry neck, or a nervous pain in the back, to alternate with insanity." P. 259.

He adds, that there is good reason to believe that the seat of those nervous communications on which such sympathies depend is for the most part not in the nerves themselves, but in a higher place—in the brain, or in the spinal cord.

In the treatment of this painful affection, Sir B. Brodie strongly objects

to operations for dividing the trunks of the nerves; observing, that the irritating cause, whatever it may be, manifestly acts not on the extremity of the nerve, but on its origin; and both reason and experience prove that the division of the nerves below the origin is of no service. It is altogether an unscientific operation. After making some excellent practical observations on the treatment of neuralgia in cases where the disease may be referred to some obvious causes, and in those in which the disease cannot be traced to its real source, the question is asked—

"But having tried all ordinary means without benefit, are you to go on *ad infinitum* tormenting the patient with medicine? The first rule of our art is to do no harm, and if you have had recourse to all reasonable expedients without benefit, it is not advisable for you to go on making further experiments. *No one can be dosed constantly with medicine without the health being ultimately injured by it; and if you have not some reasonable grounds for giving medicine, you have no right to run the risk of doing harm by its continued exhibition.* It is much more wise, as well as much more honest, when you do not know what to do, to do nothing, and to advise your patient to wait and take the chance of the pain subsiding of itself, as in fact it does in a great many instances." P. 265.

Sir B. Brodie has no great faith in the veratrine ointment; and the extravagant manner in which this application has been lauded as a remedy for neuralgia, leads him to conclude this Lecture with the following seasonable observations:—

"The statement of its effects which had been published promised a great deal too much, and I should have expected more if it had promised less. I shall take this opportunity of observing, that I am not disposed to try indiscriminately all the new remedies which in these days are being constantly brought before society; nor can I think well of this modern fashion of resorting on all occasions to novel methods of treatment. I advise you, if you wish to succeed in your profession, and to be useful to society, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and, as a general rule, have recourse to them in the first instance. If the old remedies fail, and you are at a loss as to what you should do, then, and not till then, have recourse to the new ones. If you always begin with new remedies, you throw away the valuable results, not only of your own experience, but of the experience of those who have gone before you. You have to begin, as it were, *de novo;* and the first consequence of this will be, that you will not cure your patients; and the second, that you will have none to cure. I should be very sorry to see the march of science impeded by an unjust apprehension of experiments and innovations; but, surely, there is a broad enough line between a discreet and prudent use of new remedies, and that indiscreet and hasty use of them which we find to prevail at present in the practice of our profession, and especially in that of its junior members." P. 268.

Lecture XIV. consists of practical observations on fatty or adipose tumours. Mention is made of a kind of fatty tumour which occurs occasionally, but which has not been described by surgical writers.

"In the cases to which I allude, the tumour is not well defined; in fact there is no distinct boundary to it, and you cannot say where the natural adipose structure ends and the morbid growth begins. I will relate to you the history of one of several cases of this kind that I have met with, and this will explain as much as I know of the matter. A man came to this hospital several years ago having a very grotesque appearance; there being an enormous double chin

(as it is called) hanging nearly down to the sternum, and an immense swelling also on the back of his neck formed by two large masses, one behind each ear, as large as an orange, and connected by a smaller mass between them. He said that the enlargement had begun to show itself three or four years before, and had been increasing ever since. They gave him no pain; nevertheless they made him miserable, and in fact had ruined him. The poor fellow was by occupation a gentleman's servant, and having so strange an appearance no one would take him into his service. I gave him half a drachm of the *liquor potassæ* three times daily, and gradually increased the dose to a drachm, dissolved in small beer. When he had taken the medicine for about a month the tumours were sensibly diminished in size. He went on taking the alkali, and the tumours continued to decrease. It was just then that iodine began to have a reputation, much indeed beyond experience has proved it to deserve, for the cure of morbid growths, and I left off the *liquor potassæ*, and prescribed the tincture of iodine instead. The effect of this change of treatment was remarkable. The patient lost flesh, while the tumours increased in size. Of course I omitted the iodine, and prescribed the *liquor potassæ* a second time. Altogether he took a very large quantity of the latter medicine, and left the hospital very much improved, with directions that he should continue to take it with occasional intermissions. I had lost sight of him for some time, when it happened that I was requested to visit a patient in Mortimer Street. I did not observe the servant who opened the door, but as I was leaving the house he stopped me, saying that he wished to thank me for what I had done for him. It was this very patient. He was so much improved in appearance that he was enabled to obtain a situation as a footman. There were still some remains of the tumours, but nothing that was very remarkable. I have seen some other cases of the same kind, in which the exhibition of very large doses of the *liquor potassæ* appeared to be of great service. But I have not had the opportunity of trying it, or of knowing the results in every case; and I am informed that in some cases it has been given to a considerable extent without manifest advantage." P. 276.

These tumours feel like fat; but they may be distinguished from common fatty tumours by their having no well-defined boundary, and by their being less soft and elastic. Such deposits may probably take place in any part of the body, but our author has seen them more frequently in the neck than anywhere else. Sir B. Brodie describes another kind of tumours, which also, as far as he knows, are not described by surgical writers. "They are situated in the sub-cutaneous adeps, give no pain, and are not tender to the touch; they have a well-defined margin, and are of a somewhat firmer consistence than common fatty tumours, to which, in other respects, they bear a great resemblance. They grow to a certain point, then remain stationary, while others show themselves elsewhere; until, at last, there is no part of the trunk or extremities in which they are not to be met with, varying in size from that of a pea to that of a small walnut." If any of these tumours attained a large size they might be removed with the knife, but as they are numerous it would be absurd to think of removing the whole in this manner. In these cases he has given the *liquor potassæ* in large doses with service. In one case, the tumours nearly or quite disappeared under its use. He thinks it probable that, in cases of fatty deposits, the *liquor potassæ* operates in the following manner: the oily part of the tumour combines with the alkali, is taken into the circulation and thus carried off. He gives as much as a drachm and a half, in a large quantity of fresh small beer. It seems to act better in this than when given in any other liquid, and the beer does not disagree with the

stomach because the alkali neutralizes every drop of vinegar which it contains. Sir B. Brodie agrees in the opinion of Sir Astley Cooper, that a fatty tumour will sometimes take on the action of a malignant disease, and become a malignant tumour. The case, however, related in confirmation of this opinion, does not seem to us a satisfactory example.

In the next six Lectures the subject of Mortification is very fully treated of. They contain the best account of this process, and of its causes and treatment, that we know of. Our notice of the volume has been so full, that we have only space for a few remarks and extracts of an important practical character, or that may be new to our readers. In reference to the use of ammonia in mortification Sir B. Brodie remarks, "It is a temporary stimulus; but alcohol, prudently administered, is much better; and my observations lead me to suspect that large doses of ammonia, if persevered in for a considerable time, tend to depress the vital powers, and lessen the chances of recovery." Whenever putrid matter is pent up round a slough of the cellular membrane, the system is poisoned by it. The sulphuretted and carburetted hydrogen gas evolved during the decomposition of dead animal matter, seem to pass, in part at least, into the circulation, producing the most dangerous symptoms. The incisions which relieve the tension of the skin allow these noxious gases to escape, and the relief which this affords to the patient is very great. The following is a remarkable example:—

"I was called, some few years since, to see a gentleman, who appeared to be actually on the point of death. His extremities were cold; his pulse barely perceptible. It was doubtful whether he was sensible or not. He made, on being roused, several imperfect attempts to speak, but could say nothing intelligible. Below the right hypochondrium there was a considerable tumour, the skin being of a dark red colour, on the verge of mortification. On examination with the fingers, I perceived a sort of emphysematous crackling, and an imperfect fluctuation. Having made a free incision, I discovered, underneath the discoloured skin, what might be called a quagmire of slough. A small quantity of putrid matter escaped; but there escaped also such a quantity of noisome and offensive gas, apparently sulphuretted hydrogen, that I could scarcely bear to remain in the room. The stench pervaded the whole house, and even could be perceived in the garden round it. Within two minutes after the performance of this operation, so trifling in appearance, but so important in reality, the patient looked up, and said, quite distinctly, "What is that you have done which has made so great a difference in my feelings?" At the same time the pulse returned at the wrist; and from this moment he recovered, without any further unfavourable symptoms. After a few days sloughs came away, probably of muscle, cellular membrane, and peritoneum, in a confused mass, and, with them, a gallstone of moderate size; explaining, to a certain extent, at least, the origin of the disease." P. 299.

In Lecture XVII. there are some valuable observations on the use of the different kinds of Caustics. In bites by rabid dogs, the depth of the wounds being uncertain, Sir B. Brodie observes:—

"The best caustic, I apprehend, for you to use on these occasions is the caustic potash; and for this reason: that it dissolves the parts with which it comes in contact, and that afterwards the dissolved caustic penetrates still further beyond the part to which it has been actually applied. If the tooth penetrate to the cellular membrane, by the time that you are consulted some of the saliva may

have reached the cells beyond, and if you apply the nitrate of silver, or the nitric acid, these will coagulate the fluids and harden the solids, while the caustic potash becaming diffused will follow the course of the saliva. A convenient way of applying the caustic on these and on some other occasions is this: melt it in a silver or platina spoon, and, when melted, dip into it the blunt end of a probe. It will come out with a varnish of the caustic upon it; dip it in again until the button of caustic has attained a sufficient size. By means of a probe thus armed you may carry the caustic even into a very narrow wound, so that you are sure it will penetrate wherever the dog's tooth has penetrated; after which, from the particular nature of the caustic (as I have just explained) you may be certain that it will penetrate still further, and as far as the poison can have reached." P. 325.

Caustics, it is remarked, may often be used very advantageously for the purpose of destroying diseased lymphatic glands, forming the base of ill-conditioned ulcers in the groin, which are indisposed to heal. A kind of caustic is required which will lie in the substance of the diseased glands, and destroy their internal structure, as well as their outer surface. The following caustic, used by the late Mr. Pearson, answers this purpose. "It consists of one ounce of crumb of bread, two drachms of the bichloride of mercury, and one drachm of red oxide of lead. These are to be mixed together, kneaded with the fingers, and formed into a sort of paste. The paste should be rolled into little conical troches, and these, if left to dry, become hard, like what are called bread seals. These troches may be stuck into the enlarged gland like pins into a pin-cushion. In the course of a little time they begin to act, as the patient knows from the pain produced. The pain lasts for some hours, and if a sufficient number of the troches be employed, the whole of the gland is at once destroyed. If any portion remains, it is easy to destroy it by repeating the process." In the removal of small nævi and small morbid growths of different kinds, Sir B. Brodie prefers the caustic to the use of the knife. He states, "a wound always heals much more readily after the application of caustic, than after the use of the knife. Take two cases: if you destroy one tumour of a given size by the knife, and the other, supposed to be of the same size, by caustic, in spite of the time occupied by the separation of the slough, the sore in the latter case will be healed sooner than that in the former." It is less formidable to the patient than the knife, and is likewise attended with less risk. For example, an attack of erysipelas seldom follows the use of caustic; certainly much less frequently than after the use of the knife. The application of caustic to tumours on the scalp must be made with great caution, as appears from the following case:—

"A surgeon applied the caustic potash to the scalp, with the view to make an issue in a man's head, who was labouring under a head-ache and nothing else. When the slough had separated a piece of the occiput was exposed, as large as half-a-crown, or larger. The patient was soon seized with a set of strange symptoms, and died. It was found that the dura mater had become detached from the inside of the bone, just opposite the part where the pericranium had been destroyed on the outside; and it was clear that the sloughing of the dura mater was the cause of the man's death." P. 337.

When caustics are used, it is prudent to have some counter-agent at hand to stop their action on the sound parts around. "Acids may be neutralized by alkalies; caustic potash may be neutralized by vinegar, or

by a solution of the diacetate of lead. If you are afraid of nitrate of silver burning the neighbouring parts, its action may be neutralized by common olive oil. A solution of the bicarbonate of potash will decompose chloride of zinc; and so with other caustics."

Sir B. Brodie does not agree with Dupuytren, who described the gangrene that occurs in old age as the result of arterial inflammation. He has examined the bodies of a great many old persons who have died with mortification of the toes, and has always found some morbid condition of the arteries of the affected limb. In the very great majority of cases there is extensive ossification of the arteries of the thigh and leg; in many cases they are not only ossified, but some of them are contracted and obliterated. This, we believe to be the view entertained by all the best English pathologists, whilst the majority of the French adhere to the erroneous views of Dupuytren. It has been said that mortification of the toes in old persons is often the result of disease in the heart itself. This does not correspond with the results of our author's experience. It is true that he has known persons who had disease in the heart to die of mortification of the toes; but then there was always enough in the condition of the arteries of the limb to account for the mortification independently of the other disease. Sir B. Brodie recommends neither depleting nor a decidedly stimulating treatment, but a nutritious diet and such stimulants, suited to the previous habits of the patient, as do not occasion heat of skin nor raise the pulse. He advises the limb to be wrapped in carded wool, and we are not surprised to find that he strongly objects to amputation even when the mortification is arrested. "If you apply your knife to living parts, you will probably bring on a fresh attack of mortification. Leave the separation altogether to the natural process, which will do all that is required."

Lecture XXI., the last in the volume, is on Chronic Abscess of the Tibia. A case is related of extremely painful enlargement of the lower end of the tibia which led to amputation of the limb. A cavity, as large as a small chesnut and containing pus, was discovered in the diseased bone. This occurred in 1824. It was clear that, if an opening in the tibia had been made with a trephine the limb might have been saved. In a case of painful enlargement of the tibia, in which Sir B. Brodie was consulted about two years after the occurrence of the preceding case, he made an opening into the cancellous structure of the bone with a trephine, and having given vent to some pus, the patient became cured. Four other cases, three of enlargement of the lower end of the tibia, and one of the upper, are related. They were all trephined with a successful result in giving exit to pus and curing the patient of a most painful affection. The operation has also been twice successfully performed by Mr. Liston. The following important questions present themselves. What are the circumstances that would lead the surgeon to suspect the existence of abscess in the tibia? And, supposing it to be probable that such an abscess exists, what are the exact steps of the operation to be performed for its relief?

"When the tibia is enlarged from a deposit of bone externally—when there is excessive pain, such as may be supposed to depend on extreme tension, the pain being aggravated at intervals, and these symptoms continue and become still further aggravated, not yielding to medicines, or other treatment that may be had

recourse to, then you may reasonably suspect the existence of abscess in the centre of the bone. You are not to suppose that there is no abscess because the pain is not constant; on the contrary, it very often comes on only at intervals, and in one of the cases which I have related there was, as I then mentioned, an actual intermission of seven or eight months. After the disease has existed a certain number of years, indeed, the pain never entirely subsides, but still it varies, and there are always periods of abatement and of exacerbation. The combination of circumstances which I have described will fully justify you in making an opening into the bone with a trephine. But how will it be if you are mistaken? This will not often occur; but if it should, the taking out a circle of bone can be of no consequence; no injury follows the operation: it is unattended with danger. The operation itself is very simple. You expose the surface of the bone, and make a circular opening with a trephine at that part where there seems to be some tenderness and some pain on pressure. One principal thing to be attended to is that you have a proper trephine. You do not want so large a one as for the cranium, and it must be somewhat differently constructed. Those which lie on the table are made for the purpose. One is of very small diameter, but, generally, it is quite sufficient. The common trephine is made with a projecting rim or shoulder, and if there be much enlargement of the bone, it will not penetrate deep enough to reach the abscess. It is true that you may break away the bone afterwards, by means of a chisel, but the operation may be more easily performed with a trephine having no projection, which will at once penetrate to the abscess, however deep it may be, and render the chisel unnecessary." P. 405.

After the operation, the bone soon granulates, the space is filled up by a sort of fibrous substance, and the wound cicatrizes. Sir B. Brodie states that, if the operation were not performed, the patient may continue in torture for a great number of years, losing all the best part of his life; or a worse event than that may take place. The abscess may lead to disease in a neighbouring joint and in this way prove destructive to life. Cases are given in which abscesses in the head of the tibia had caused serious disease of the knee-joint. These cases "show that it is not safe to leave an abscess in the extremity of the tibia beyond a certain time; that the joint is always in danger, and that the perforation of the bone is the only remedy. Even if you were mistaken in your diagnosis, no harm can arise from the operation."

A case is mentioned of a young gentleman who had a painful enlargement of the middle of the humerus. It was trephined freely, but no matter escaped. The bone was very hard and compact, and the trephine was with difficulty made to penetrate through it. The wound healed, the relief was complete, and the patient continued quite well. Sir B. Brodie supposes "that this was a case of chronic inflammation of the humerus, and that taking out the piece of bone from the centre, probably, partly by relieving the tension, and partly by a discharge of matter from the bone unloading the vessels, accounted for the relief which the patient obtained from the operation."

Notwithstanding the result of this case, it cannot be questioned that the greatest care and caution are required in determining the necessity for an operation for the evacuation of a supposed abscess in the substance of a bone. We have been informed that, not long since, a limb was amputated by a hospital surgeon, in consequence of a suspected abscess in the head of the tibia of a painful character, and implicating the knee-joint. On

examination of the limb after removal, the joint and tibia were found to be quite healthy, and the complaint proved to be hysterical.

We here terminate our notice of these interesting Lectures. We have not attempted more than to furnish an outline of the subjects treated of, and a condensed account of the more important contents of the volume, giving such extracts as may appear likely to be useful to the majority of our readers. Sir B. Brodie's position in the profession and great experience are such that his practical writings must be considered as beyond the reach of criticism, at least by those of more limited observation. Time will be the best test of the value of the principles inculcated and of the suggestions for the treatment of disease. We strongly recommend our friends not to rest satisfied with this notice, but to read the volume itself, and we can promise them that, by doing so, they will obtain a clearer insight into many obscure subjects of surgical pathology, and become better armed to overcome the difficulties of practice. It now only remains for us to return our most hearty thanks to the author for this valuable contribution to surgical literature, and to express a cordial hope that he may have health and leisure to fulfil the expectations which he has held out of publishing a second Series of his Lectures.

ON THE ALTERNATION OF GENERATIONS; OR, THE PROPAGATION AND DEVELOPMENT OF ANIMALS THROUGH ALTERNATE GENERATIONS: A PECULIAR FORM OF FOSTERING THE YOUNG IN THE LOWER CLASSES OF ANIMALS. By *Joh. Japètus Sm. Steenstrup*, Lecturer in the Academy of Söro. Translated from the German Version of *C. H. Lorenzen* by *George Busk*. London: printed for the Ray Society. 1845.

THIS work is calculated to excite much interesting speculation, as exhibiting some new phases in the process of generation, and also as throwing some light upon one of the most obscure points of physiology, the reproductive process of Entozoa. The title, extended as it is, will hardly convey to those unacquainted with the views of the author any very clear notion of the peculiar mode of propagation here described. This cannot, however, be a matter of surprise; for although, as Mr. Busk states, some glimpses of the subject were afforded by the naturalist Chamisso, in his observations on the apparent alternations of generation, so far back as 1819, and by the subsequent researches of Sars and Siebold on the development of the Medusæ, and by those of Lovén on the development of Campanularia, &c., still no one, until the author of the present work, "appears to have entered upon the general question of the existence of an analogous course of development *throughout* the lower classes of animals." —(*Translator's Preface*, p. vi.) As the phenomena described by M. Steenstrup are so novel, we shall probably succeed best in conveying a knowledge of them to our readers, by prefixing a few general remarks illustrative of the relations between them and the ordinary mode of generation.

In all the forms of reproduction with which we are best acquainted, it is seen that the production of the new being is partly effected by powers inherent in the parents, male and female, and partly by powers inherent in the germ or embryo itself; the former effecting the primitive formation of the germ, whether that be accomplished by the male, by the female, or by these conjointly, for all these theories have their advocates; the latter, accomplishing the development or growth of the germ.

It is necessary to bear in mind that the individual modes of generation differ from each other essentially, only by the manner in which this second part of the process (development) is accomplished: it may be oviparous, ovo-viviparous, or viviparous, but in each case the germ is the sole agent of its own growth, however much the mode of its nutrition may vary. This law does not, however, exclude the indirect agency of the parents, principally the mother, in providing some of the necessary conditions—such as a supply of nutritious matter, of heat, of protection, &c. Up to the present time the best known exceptions to these incidental processes being accomplished by the parents, and again we may observe almost exclusively by the mother, are furnished by bees, wasps, ants, and termites, among which interesting animals the curious provision of *nursing* is displayed, that is to say, individuals are met with having no other office than to provide food, heat, and protection for the young; an economy with which all are familiar, who know the proceedings of the bee-hive or ant-hill.

Although, as will afterwards be apparent, this system of nurses is a branch of the propagation of animals by the principle of "alternate generations," we would more specially fix the attention of the reader on the mode of reproduction which takes place among Aphides, because this in reality is a perfect type of the remarkable process elucidated in the present work.

"The propagation of these creatures through a series of generations has been already long known. In the spring, for instance, a generation is produced from the ova, which grows and is metamorphozed, and without previous fertilization, gives birth to a new generation, and this again to a third, and so on, for ten or twelve weeks; so that, in certain species, even as many as nine such preliminary generations will have been observed; but, at last, there always occurs a generation consisting of males and females, the former of which, after their metamorphosis, are usually winged; fertilization and the depositing of eggs takes place, and the long series of generations re-commences in the next year, and in the same order." P. 108.

In order to form a true notion of this most remarkable form of propagation, it is necessary to state that, as no immature or imperfect animal can form germs, the germs of all these generations must have been produced by the perfect and fertile individuals, the several intervening generations, excepting the last, standing simply in the relation of nurses, affording, like the nurse-bees, but *in the interior of their bodies*, nutritious matter, heat, and protection; so that we have here the curious spectacle of animals giving birth to young without being parents, and of young proceeding from individuals of which they are not the offspring.

If this be the true view of the generation of Aphides, it becomes an interesting, and, in connexion with the general laws of ovology, an important question to determine the true sexual nature of the animals forming the

intervening generations, and the nature of their retentive genital organs. The history of social bees and wasps shows that the feeders or nurses are females, whose sexual organs remain in an undeveloped state; and Steenstrup affirms, what from this fact we should necessarily infer, that what he well terms "foster-parents," are among the Aphides also imperfect females; indeed, in every similar case, the multitude of individuals, to whom is committed the perfecting of a later generation, "consist invariably of females, the males being apparently excluded from any participation in the office, on which account the males of all the animals among which the system of 'nursing' or of 'feeding' obtains, constitute a very subordinate number." (P. 111.) This conclusion is greatly strengthened by what next follows.

Among the many interesting facts and generalizations of this philosophic investigator, there are none evincing more acumen than those relating to the nature of the retentive organ. M. Steenstrup well observes—

"That the '*nursing*' should be committed to females alone appears to us very natural, since we are acquainted with an organ in them whose natural function would be to perform that office. The generative organs are, indeed, in perfect (female) individuals, divided as it were into two parts of very distinct natures; the *ovarium* for the preparation of the germ and the production of the egg, and the *oviduct* and *uterus* in which the ova are as it were incubated, and the germ and embryo sufficiently developed to allow of its being born. Now, it is actually the case, that no true *ovary* has been discovered in the '*nursing*' generations; on the contrary, the germs, as soon as they are perceptible, are situated in organs which must be regarded as *oviducts* and *uteri*, as, for instance, in the most perfect '*nurses*' we are acquainted with, the *Aphides*." P. 112.

This is a most valuable and distinguishing fact, and not only throws a clear light upon the whole system of alternating generations, but also reconciles with the law already noticed, that imperfect individuals, namely, cannot form germs, what had hitherto appeared to be the anomaly of imperfect Aphides reproducing. We shall have occasion again to notice this point.

It is thus seen that, in this propagation by the agency of nursing animals, the business of generation, so far as the process of developing the new being is concerned, instead of being, as in ordinary reproduction, accomplished by one individual or mother, (we exclude for the moment those few cases in which the male assists in nourishing the embryo,) is, in this most remarkable process, divided among several different individuals.

We now proceed more particularly to point out the views of the author, and to detail some of the principal facts that have led to the theory he so ably advocates.

"The special subject of this Essay," observes M. Steenstrup, "is the fundamental idea expressed by the word '*Alternation of Generations*,' or the remarkable, and till now inexplicable natural phenomenon of an animal producing an offspring, which at no time resembles its parent, but which, on the other hand, itself brings forth a progeny, which returns in its form and nature to the parent animal, so that the maternal animal does not meet with its resemblance in its own brood, but in its descendants of the second, third, or fourth degree or generation; and this always takes place in the different animals which exhibit the phenomenon, in a *determinate* generation, or with the intervention of a *determinate* number of generations. This remarkable *precedence* of one or more gene-

rations, whose function it is, as it were, to prepare the way for the later, succeeding generation of animals destined to attain a higher degree of perfection, and which are developed into the form of the mother, and propagate the species by means of ova, can, I believe, be demonstrated in not a few instances in the animal kingdom." P. 2.

His investigations concern more especially the phenomena which have been observed in the propagation and development of the Medusæ and of the Claviform Polypes among the Acrita, of the Salpæ among the Mollusca, and of the Trematoda among the Entozoa.

The mode of generation in the Medusæ has for a long time been an enigma among physiologists, and although much of this obscurity was cleared up by the admirable researches of the Norwegian naturalist Sars, and subsequently by those of Siebold, yet it was reserved for the author to trace all the links which connect the ovum with the fully-formed animal. The eggs, after experiencing a certain degree of development first of all in the ovaries, and subsequently in the marsupial vesicles belonging to the oral arms or tentacles, disclose, and bring forth young, not, as might be expected, having the characters of the parent Medusa, but creatures of an oval form, and outwardly resembling the Ciliated Infusoria, such as *Leucophrys* or *Bursaria*. This apparent, but not real, infusorial animalcule soon attaches itself to some fixed object, and by degrees becomes transformed into an animal, having a mouth provided with a number of tentacula amounting ultimately to thirty; in this stage it very much resembles a polype. By a kind of transverse fission this polypiform animal is gradually separated into many portions, which, acquiring independent vitality, are finally separated from each other, and become free swimming animals. Now it is within the abdomen (if such a term may be here applied) of these polypiform Medusæ that the true larvæ of the perfect animals are found; and we must, as far as we at present know, assume that the function of these polypiform individuals is fulfilled when the larvæ have reached a definite development, and that their whole existence has for its object the perfecting of a series of beings of the same species, to which they stand in the relation in what, for want of a better term, may be called "*foster-parents*." (P. 24.)

M. Steenstrup concludes his observations on the propagation of the Medusæ by stating, that—

"Since, according to the observations of both Sars and Siebold, only animals which have the power of affixing themselves proceed from the ova of Medusæ, all of which become in the way so often described, polypiform '*nurses*,' which nourish the Medusæ-larvæ from their bodies, a considerable anatomical and physiological difference clearly exists between them, which are *all* of one sex, and the perfect *Medusæ* which are of two sexes. Here, however, we observe a great natural harmony; since wherever we find the fostering of a brood or fry to be committed to special individuals, these are always of *one* sex, and indeed females in whom the sexual organs of germination remain in an undeveloped state, whilst in consequence of this abortion in the development of these parts, the instinct or *nisus* serving for the preservation of the species takes a peculiar direction." P. 24.

This mode of development has only been seen in two of the most common species of Medusæ (*M. aurita* and *Cyanæa capillata*,) but, in all probability, it occurs throughout the whole family. It is further proper to

remark that, although the author admits with Sars that the "*nursing*" individuals increase by gemmation, every part of the body being capable of throwing out gemmæ or buds, yet he contends that this fact does not affect the truth of his representation; for the question arises—May not an ovum occasionally contain several embryos, one of which becomes developed at the expense of the others, which make good their individuality at a later period?—(*L. c.*) We are not, it is true, in a position at present to solve this question; but it must be apparent to those who are acquainted with the obscurity overhanging the reproductive process among the most simple animals, some of which are believed to establish the doctrine of equivocal generation, whilst others are pronounced at one time to be fissiparous, at another gemmiparous, and at another oviparous, that these researches of Steenstrup, if ultimately established, must render a review of the whole theory of generation indispensably necessary.

The next class investigated, referring to some families of the multitudinous section of *polypes*, throws further light upon this interesting question. We shall select for illustration one of the bell-shaped polypes, *Campanularia geniculata*. On the branched stem of one of these individuals, are placed a number of cells or cups, shaped like inverted cones or bells, which have a determinate position, and are of three kinds, constituting, according to the author, so many distinct generations. Those of the first order occupy the summits of the branches, and elicit afterwards the larger individuals forming the second class, which, like the bud in a plant, are situated in the axillæ of the branches; in the interior of these axillory polypes are placed the individuals of the third kind or generation, which are in fact the perfect, that is, the reproductive females. The mode in which this curious progeny is produced is, according to Steenstrup, as follows:—between the outer and inner membranes of the second order (axillary polypes) and upon the so-called intestinal tube, small vesicular elevations are formed, each of which becomes included in a transparent membrane or sacculus; within this sac, and in connection with the vesicular elevation, two perfectly spherical bodies appear; subsequently, the sacculus with its contents approaches the operculum of the axillary cell or polype, and breaks through it, without, however, losing its connection with the central tube; finally, the apparent sacculus opens, a circlet of about twelve irregularly-toothed tentacula projects around the opening, and it is now seen that the sacculus is in reality a distinct individual. Several such individuals are found within a single axillary polype; and we may now state that they are the completely developed females above noticed, the two spherical bodies being prolific ova, presenting opaque granules (yelk-globules?) and, which is more interesting, the germinal vesicle of Purkinjé. The subsequent steps of the process require to be carefully attended to:—the ova are developed within the parent-body, and when the embryo is matured, it ruptures the delicate membrane of the egg, remains for some time within the parent animal, and then passing out, it enters the water, freely swims about for a time, being provided with vibratile cilia, afterwards attaches itself to some fixed object, and becomes the origin of a new compound polype, on which are formed the three cup-like cells already described. Thus, according to the views here advocated, three distinct and successive generations are en-

gaged in perfecting the fully-developed and fertile female, and are so many sets of nurses or foster-parents.

"Upon reviewing this sketch, it can hardly be considered to present an instance of metamorphosis, since it is not the same *individuals* which in the course of time exhibit various forms, but we see here also, a series of *generations* whose succession in a definite order is necessary to the complete development of the species. The ciliated embryos which, by their adhesion lay the foundation of the polypes, originate only from ova, which are developed in the females. These latter are generated between the inner and outer tunic of the polype in the axillary cells, which polypes may consequently be considered as the '*nurses*' or foster-parents of the female individuals, whilst they are themselves, on the contrary, derived from a previous generation of polypes of a different form, which occupy the extremities of the branches, are of the same shape as the polype of the stem, and must be regarded as of the same kind, although they appear to arise from it by what is called *gemmation*." P. 37.

In the preceding instances, the theory of Alternate Generations has been traced only among the Radiate sub-kingdom, and consequently in reference to the simplest animal forms. In the Third Chapter, however, comprising an account of the Alternate Generation of the Salpæ, the author proceeds to a more important division, that of the Mollusca. It may be as well to premise that the Salpæ belong to that order of the acephalous molluscous animals, called *Acephala nuda*, and which are divided by Cuvier into two families—the solitary and the aggregated—according as the individuals are met with in an isolated or aggregate condition. We may further remark that there are some peculiarities connected with the mode of generation among these naked Acephala, which have for a long time occupied the attention of Naturalists, and have given rise to many conflicting opinions. We will, in the first place, state the facts that have been ascertained, and then give M. Steenstrup's interpretation of them.

It is well known, as regards the Salpæ, that although they are classed by Cuvier in the solitary division, yet they are also met with joined together, forming long chains consisting of from 20 to 40 individuals or more, possessing organs for attachment; these *salpa-chains* are found in the Mediterranean and warmer parts of the ocean, and all the individuals forming them move with great uniformity, so that the whole chain progresses in a serpentine manner below the smooth and tranquil surface of the water. The most interesting facts hitherto known respecting these curious mollusks were observed by Chamisso. That excellent naturalist ascertained that the solitary salpæ, though never themselves forming parts of a chain, yet always contain a progeny resembling those which do, and, further, that each separate embryo of this progeny is linked together like the individuals forming the salpa-chains; whilst in the associated Salpæ, on the contrary, he found young resembling the solitary ones. "Now, as examination of the free swimming individuals, which presented marks of having been disjointed from a chain, proved that these also contained only solitary pedunculated embryos, Chamisso concluded from his observations, *that all solitary Salpæ produced associated ones or chains; and, on the contrary, that all the associated Salpæ were parents of solitary ones, and these again of the associated*, and so on." (P. 39.)

This theory of the alternately solitary and associated generations of the Salpæ was so opposed to all then known of the reproductive process, that

it is not a matter of surprise that it encountered much opposition. Professor Eschricht, of Copenhagen, although one of Chamisso's opponents, admitted his facts, and added one strongly confirmatory of the truth of his deductions, namely, that in the case of the *Salpa Cordiformis*, "each embryo constituting the embryo-chain, while still enclosed in the body of the mother, presented distinct traces of younger solitary embryos." It is not our object to enter into any of the details of this controversy; we will therefore only mention, as showing the fallacy of Professor Eschricht's arguments, that he supposed the Salpæ, whilst in the associated form, and therefore certainly in an immature condition, might give birth to solitary individuals, and subsequently, becoming detached from each other, that they produced a later generation of associated embryos; a supposition which, as Steenstrup justly remarks, is opposed to the fundamental physiological principle, that no animal is able to propogate before it has attained its perfect form and development.

Such, then, being the fact, for we assume as proved the position of Chamisso, that all Salpa-chains are produced from solitary Salpæ, and all solitary Salpæ from the associated individuals, we come to the author's interpretation. After pointing to the resemblance between the phenomena just described and those noticed in the propagation of the Medusæ, and which assuredly cannot escape observation, M. Steenstrup proceeds—

"I have consequently no hesitation in expressing my belief, that the alternate generation of the *Salpæ* from solitary and associated individuals, is to be explained in the same way as that of the *Medusæ*, and that the first generation of these animals serves as a series of foster-parents, whose object is, by their furthering the development of the second generation, to conduce to the perfection of the species. I am, however, prepared to admit, that this kind of explanation does not enlarge our knowledge with regard to the propogation of the *Salpæ*; I only think, that the phenomena already known, will have a clearer light thrown upon them from the analogy. Some doubt even must still remain, on the circumstance whether the solitary or the associated *Salpæ* are the foster-parents. In my opinion, however, it is the solitary form rather than the other which should be assumed to be that of the '*nurses*.'" P. 46.

In this last conclusion we cannot agree with the sagacious author, inasmuch as, according to his own remark, the free moving animal must be regarded as more perfect and developed than those which are attached together, and, consequently, as being the true prolific individuals or parents.

We have now to notice one of the most interesting parts of the whole inquiry, the development of the Trematode Entozoa, and especially of the *fluke* or *Liverworm* (Distoma Hepaticum.) Although some links are still wanting, sufficient proofs are adduced to show that the system of "nurses" obtains here, as in so many other of the lower classes of animals. By a very minute and comprehensive investigation, the author appears to have ascertained that several of the Trematoda when young are not connected with any organ, but enjoy free locomotion in water, externally to the animal, which in their future state they infest—(he particularly has examined the flukes of water-snails, such as *Limnæus stagnalis*, *Planorbis cornea*, &c.)

"In this condition they are provided with a locomotive organ, usually a tail of moderate length, by the waving movement of which the creature propels itself

through the water, like a tadpole, to which in its external form it is not dissimilar, except that it is much smaller, and almost microscopic. In this larval state, the *Trematoda* are known to naturalists under the generic name of *Cercaria*. It was well known that this form was not a permanent one, but we were ignorant of the changes which it underwent. As I have been so fortunate as to trace these changes, I will now detail them." P. 53.

This, then, is the first connecting link in the investigation, namely, that *Cercariæ* are in reality the true larvæ of the Distomata infesting water-snails; but they undergo a curious metamorphosis before attaining their ultimate and mature character. On examining with a sufficient magnifying power a portion of the skin of the snail, to which the Cercariæ adhere by their abdominal sucker, it is seen that they make forcible lashing motions of the tail, which organ at length is thus cast off as a lifeless mass, when "the tailless animal itself assumes so completely the appearance of a *Distoma* or *fluke*, that it could not fail of being recognised as belonging to that genus, in case it were met with in this condition in the viscera of other animals." Subsequently the animal, burying itself in the soft integument of the snail, becomes covered with mucus, (which gradually hardens and becomes a nearly transparent case); it is now in fact a pupa, and remains in that state for a period of from two to nine months, when, quitting its puparium, it becomes a true entozoon, the Distoma Hepaticum. We have here the solution of the enigma which puzzled the celebrated helminthologist, Siebold, who could not ascertain what became of the pupæ of the Cercariæ.

These points having been determined, it is necessary next to state that the Cercariæ, although they are thus proved to become Distomata, are not themselves immediately derived from those trematode animals, but from certain parasitic worms found in snails, and described by Bojanus under the somewhat barbarous name of "*King's-yellow-worms*," and which are therefore, in the language of Steenstrup, "nurses," (*Altrices*.) This derivation would seem sufficiently complex, but the author is satisfied that even these nurses do not proceed from the ova of the fluke, but from an interposed and pre-existing brood (termed *parent-nurses*, *Abaltrices*,) in the interior of whose bodies he plainly distinguished "a progeny consisting of actual nurses in all stages of development."—(P. 69.) The precise origin of these parent-nurses has not been determined, although it is said there is no doubt they are the immediate offspring of the parent Distoma, being derived in the ordinary mode from ova. In the following table we have endeavoured to place this strange genealogy in a clearer point of view:—

1. Distoma Hepaticum—perfect animals, androgynous, reproductive.
2. Parent-nurses (Abaltrices) derived from ova } Imperfect
3. Nurses (Altrices,) King's-yellow-worms } females.
4. Cercariæ, true larvæ of fluke.

The author does not describe with any minuteness one of the most important points in the whole theory, the true nature, namely, of the germ from which each generation springs. He says, "at first the germs, from which the Cercariæ are developed, are nearly spherical, and appear to be formed of numerous vesicular globules or cells, which are pretty clearly seen to be all surrounded by a very delicate common membrane: these spherical germs afterwards increase in length, as they are formed into

embryos"—(P. 64); and a similar obscure account is given of the germs of the "Altrices." The general laws of development, as well as the fact, already stated, that in the similar process among the Campanulate polypes the vesicula Purkinji was detected, lead to the conclusion that each generation springs from true germinal vesicles; and, as imperfect animals are never fertile, these vesicles must all have been derived from the parent distoma; but how the germs of the successive broods are placed as regards each other, we have at present no means of determining. In all cases, however, it is certain that the embryos are lodged when they first appear in the posterior part of the body between the two lateral processes existing there; "it cannot be doubted," adds Steenstrup, "that the germs are always collected in that situation as in a distinct organ, (uterus?)" As to the mode of birth, it would appear that the Cercariæ quit their "nurses" at a particular depression under what is called the collar, where two apertures are supposed to be placed. An examination of the plates (more particularly Tab. 2, fig. 3, a——h, fig. 5, a——h) confirms the conclusion that, however much the process may deviate from ordinary generation, there is no departure from the fundamental laws.

It has always been a difficult point to comprehend how the Entozoa, which have their habitat in the very substance of organs, the *Trichina spiralis* for example in muscle, effect their entrance. Although this general question cannot be resolved by the proceedings of the Cercariæ infesting aquatic animals, still the mode in which those creatures bury themselves in the skin of the snail on assuming the pupa state, enable us at all events to understand, as the author remarks, how they can enter the nobler internal organs. In this instance it is probable that the instrument for effecting the passage, consists of the circlet of spines on the collar around the oral orifice, since these are found on the Cercariæ, even after they have escaped from the pupa-case, but are cast off after the Distoma has gained its nidus. It is further interesting to remark that, as the Cercariæ of water-snails are proved to undergo their metamorphosis in the interior of the abdominal cavity as well as on the outer surface, there is no difficulty in perceiving that, in higher animals, as mammalia, altrices, or their progeny of larvæ, might be introduced into the alimentary canal, and thence, to borrow a common expression, work their way into the parenchyma of the organs.

The observations of M. Steenstrup respecting the generation and development of Entozoa in general, are of such deep interest that no apology is required for laying them before our readers.

"When a metamorphosis occurs so extensively, or is even universal in one division of the entozoa, the question naturally arises, whether, with respect to the other divisions of that class, it is probable that a similar transformation is effected by a single metamorphosis, or whether even they probably do not attain their development through a series of alternate generations, and exist at first externally to the organisms with which their life is afterwards inseparably connected. Although I cannot commit myself to a precise reply to this query, which is rather beside the especial object of these pages, yet I cannot help giving a few hints on the subject. The *Nematoidea*, which in their adult state often pass from one individual to another, also probably penetrate from without and as embryos, the organism they infest; they do not appear to undergo a true metamorphosis, but to change their skin; I am also unacquainted with any observation which would

justify the supposition, that there is in this group any fostering of the young by precedent generations, unless the genus *Sphærularia*, a parasite of the *Hymenoptera*, which Siebold refers to this division of entozoa, be a '*nurse*;' at least it much resembles one, appears to be nearly powerless, to perform spontaneous movements, and contains a numerous smooth-skinned progeny, which move about very actively within their parent, but bear no resemblance to it. The *cystic* entozoa, on the contrary, betray in many ways that they are a '*nursing*' generation, and especially in the singular circumstance of their being frequently inclosed like boxes one within the other. Probably the full-grown animal of this division is quite unknown; and it is not unlikely that, in course of time, it may happen with them as it has with the whole division of the '*asexual*' Trematoda of Siebold, viz., *Cercaria*, *Leucochloridium*, &c., that they must be rejected from the system as being earlier forms of development, or earlier generations of other animals. The *Echinorhynchi* present several phenomena which are interesting with regard to our object, viz., the remarkable incubation or breeding which takes place between the skin and the viscera, and the inclusion and incipient development of the ova in the so-called '*loose ovaries*,' during the continued growth of the latter. I must confess, that I look upon these oval bodies much rather as individuals which will never quit the parent animal, than as '*ovaries*;' and till their true nature is known, I shall regard them as such, and consequently consider most of the *Echinorhynchi* hitherto known, as '*nurses*.' It is confessedly uncertain whether the *Echinorhynchi* spend part of their life externally to the organism which they inhabit as full-grown animals, or not; it is however very probable they do so, as the embryo attains no real development in the ova, so long as these are in the *Echinorhynchus*; and the ova are met with in the mucus of the stomach and in the excrements, by thousands, in the same condition, so that the development of the young in the ova and their escape from them, certainly occurs very long after the ova have reached the water." P. 101.

"Lastly, with respect to the metamorphosis of the *Cestoid* worms, we have an example of it presented to us in Miescher's interesting memoir on the forms which the genus *Tetrarhynchus* passes through; but which also in fact includes nearly all we know about them; whilst the great work of Professor Eschricht, upon the genus *Bothriocephalus* of this family, has given an entirely new view of those animals, constituted of almost innumerable joints, so that they are to be regarded, according to his anatomical investigations, not as single but as *compound* animals, viz., compound *Trematoda* or *flukes*, so that each joint is to be compared with a *distoma*,—a view of their nature to which Baer had some time before alluded, and which had also struck Creplin and Mehlis, on account of the resemblance of the sexual organs, but which is now for the first time entitled to the greatest attention, having been rendered probable by so many anatomical researches. However, I cannot, for my part, entirely coincide in this view of the *Cestoid* family of worms, for in whatever way the joints and their reciprocal connection are considered, compound animals are presented, whose construction is entirely different from that of all other animals. The tapeworm is certainly not a single individual, but consists of several; that is, it is constituted of the head, which is an animal, and of the progeny derived from it. This view is much supported; it is even proved, by the fact that the offspring (joints) in a state of progressive development, never actually become animals similar to that from which they spring, (the cephalic joint,) which alone remains dissimilar to all the rest, never acquires any developed sexual organs, and consequently never generates any ova, which the others produce in great abundance; whilst the cephalic joint is furnished with a mouth and suctorial acetabula, proceeds from an egg, and is the animal upon which the development of all the rest depends, and is thus in fact one of those animals to which we have in the preceding pages given the name of '*nurse*.' If this view be correct, which time will determine, it will be seen that the *Bothriocephali*, just as all the other animals

mentioned in this Essay, which are developed through alternating generations, present quite another and more significant resemblance to plants, than that set up by Professor Eschricht. The individuals which are fostered by, and *appear* to proceed from, the head by the so-called transverse division, thus attain such a degree of perfection, that the ova are fully formed in them even before they become detached from the '*nursing animal*;' and, as defensive cases for the ova, they are passed in the natural way from the animal which they have infested, in order probably to reach, eventually, another similar animal, under another form, and as other individuals." P. 104.

In concluding our notice of this remarkable work, we are anxious to point to some of the more important results which it seems to establish. And, first of all, it will not be superfluous to state, as corroborative of the author's views, that many of the facts upon which they repose have been ascertained by other and earlier investigators, and are acknowledged to be well founded; thus, whatever doubts may be entertained, they must have reference not to the phenomena themselves, but to the way in which these are to be interpreted. They, the phenomena, have usually been regarded as instances merely of metamorphosis or transformation; but, as M. Steenstrup justly observes, the essential objection that a *metamorphosis* can only imply changes which occur in the *same* individual, has been overlooked; and, further, that when from an individual other individuals originate, something more than a metamorphosis is concerned.

"Thus, it is quite erroneous to term a *Scyphistoma* the larval condition of *Medusa aurita*, since *Scyphistoma* never becomes a *medusa*, but is the *quasi* mother of some scores of them. Sars and Lovén have taken a more correct view of the relation in which these creatures stand with regard to each other, seeing, in the development of the *Medusæ* and *Campanulariæ* a series of *generations* undergoing metamorphosis. It is of the more essential importance that the distinction between an alternation of generation and a metamorphosis should be understood, because a metamorphosis may readily occur in one or other of the alternating generations themselves, as we see, for example, in the *Distomata* and *Aphides*." P. 6.

In the next place, it is to be remarked that, however bizarre and exceptional the phenomena in question may seem to be, they in reality fall within the general laws of development, of the fixity and universality of which, when correctly viewed, they offer a most striking example. It is here, as in all other sciences, perceived, that the admission of exceptions is indicative of imperfect knowledge and of limited observation; for, in proportion as facts multiply and afford the materials for induction, what had hitherto seemed to be isolated and exceptional instances are found to form but a part of a whole series of phenomena, all conformable to fixed and undeviating laws. In the case before us, it would have been a real exception, if the imperfect animals, forming the intervening generations, had been prolific; but it has been already shown that they have no ovaries, (or more probably that they possess those organs in an undeveloped state,) and we may, with the author, further assume, "that the 'nursing' individuals are never themselves gemmiparous, but that they are born with germs in the organs (uteri) in which the embryos are afterwards nourished."

In connection with this way of viewing the subject, it is not one of the least interesting results, that the curious economy of the bee and wasp, of

ants and termites, which has up to the present time been regarded as a thing sui generis, is shown to be but a part of a mode of generation extensively prevailing among the lower animals, and which is itself only a modification of, and not a deviation from, the normal female action—the nutrition of the germ.

We have already pointed out, what cannot have escaped the notice of our readers, that many instances of what have hitherto been regarded as gemmiparous and fissiparous generation, will probably be shown to belong to the system of alternate generation. Nor is it attaching too much importance to these researches to affirm, that they will throw considerable light upon some of the still obscure parts of the reproductive process, and, among others, on equivocal generation.

Among what the author properly styles secondary results, are some highly important facts: such as "that the *Cercariæ* are *larvæ* of entozoa, of the genus *Distoma*, and in fact of those species which inhabit the interior of fresh-water snails (in the liver, &c.;) *that* the entozoa pass part of their existence in a state of freedom in the water external to the snail, which they afterwards inhabit, and that they re-enter them from without; *that* whole established divisions of families of animals must be abolished, since they include only undeveloped forms, or forms which bear the same relation to the true and perfect form of the species, that the 'workers' among ants and bees bear to the fertile female of those insects; and, finally, *that* several forms which have been considered as of different species and genera are seen to be stages in the development of one and the same animal." To this enumeration must also be added the important light which is thrown by these inquiries into the character of what have been regarded, either as asexual or hermaphrodite animals, but which, at all events in many instances, are nothing else than imperfectly developed females.

M. Steenstrup thus philosophically sums up his researches:—

"I conclude with the remark, that, inasmuch as in the system of '*nursing*,' the whole advancement of the welfare of the young is effected only by a still and peaceful organic activity, is only *a function of the vegetative life* of the individual, so also all those forms of animals in whose development the '*nursing*' system obtains, actually remind us of the propagation and vital cycle of plants. For it is peculiar to plants and as it were their special characteristic, that the germ, the primordial individual in the vegetation or seed, is competent to produce individuals which are again capable of producing seeds or individuals of the primary form or that to which the plant owed its origin, only by the intervention of a whole series of generations. It is certainly the great triumph of Morphology, that it is able to show how the plant or tree (that colony of individuals arranged in accordance with a simple vegetative principle, or fundamental law,) unfolds itself through a frequently long succession of generations, into individuals, becoming constantly more and more perfect, until, after the immediately precedent generation, it appears as *Calyx* and *Corolla* with perfect male and female individuals; stamens and pistils (so that even in the vegetable kingdom the grosser hermaphroditism does not obtain, which is still supposed to take place in the animal,) and after, the fructification brings forth seed, which again goes through the same course. It is this great and significant resemblance to the vegetable kingdom, which in my opinion is presented by the *entozoa*, and all '*nurse*' generations, and to which I have alluded in the preceding Essay; I might almost say, that the condition of continued dependence incidental to the animal life, is, to a

certain extent, one of less perfection than that which is presented in the progressive elevation in development effected by the agency of the vegetable life." P. 115.

We have extended this article beyond the limits originally designed for it, having ourselves derived the greatest satisfaction, and we may add instruction, from perusing the deeply scientific and profound work of M. Steenstrup; and we should not do justice to our own sentiments, if we did not congratulate the Ray Society, and that excellent observer Mr. Busk, on having been the medium of making known to the English public, a series of researches equally interesting to the physiologist, the naturalist, and the pathologist.

ON DISORDERS OF THE CEREBRAL CIRCULATION, AND ON THE CONNECTION BETWEEN AFFECTIONS OF THE BRAIN AND DISEASES OF THE HEART. By *George Burrows*, M.D. 8vo. pp. 280—Plates. London, 1846.

THE substance of this work has already been made known to a portion of the profession in the Lumleian Lectures delivered by the author in 1843 and 1844; but the subject treated of is one of such high importance, and the conclusions arrived at must, if admitted as sound, enforce so great a modification in the management of a large class of dangerous diseases, that an extended publication and criticism of the doctrines advanced is called for.

The emancipation of the mind from the despotic influence of great names is always a difficult and sometimes a very delicate task. There prevails so natural a feeling of veneration and gratitude for those who have preceded us in the paths of laborious investigation, and so many of their dogmas and principles are supported with able reasoning, well devised experiment, or sagacious observation, and have firmly resisted attacks from all quarters, that when any young practitioner (and it is ordinarily such who undertake opposition to any prevalent opinion) ventures to suspect that some of these may be fallacious, he is apt to be met with sneers or rebukes, and although he may have reason and truth on his side he will oftentimes fail in securing their reception. The difficulties, too, in breaking up a routine practice, however erroneously this may be based, can only be appreciated by those who have attentively observed the tenacity with which old prejudices and practices adhere even to well-informed persons, to say nothing of that preponderating class in all communities who continue to mechanically perform certain actions because they always have done so. When, however, we look back upon the history of medicine, and find that hardly a doctrine or theory once advocated by the ablest observers, holds its ground, and that what were once considered by acute men as axioms in practice, are now discarded as sheer fallacies by the merest tyro, we see the necessity of ever and anon re-examining the foundations of our medical creed, and submitting it to the reforms and improvements which the progress of science, the increased means of in-

vestigation, and the accumulation of facts have placed at our disposal. Still, it required some courage on the part of Dr. Burrows to announce to the assembled sages and seniors of his college that the principles upon which their practice in an important class of maladies is founded are erroneous, and to inculcate the absolute necessity of adopting measures of investigation to the use of which several of them are incompetent; but in his lectures there is an absence of self-sufficiency, a deferential mention even of the authorities he feels called upon to differ with, and an earnestness of tone in what he advances, that preserve him from the imputation of being a mere innovator, and enforce his claims to attention. Disciplined under the auspices of one of the best clinical teachers this metropolis has ever possessed, and furnished with the wide field of observation a large hospital alone can give, he has advanced no opinion without mature consideration and examination, and has advised no departure from routine practice without furnishing ample evidence of its inefficiency or its hurtfulness.

The work is divided into two portions. In the first of these, the erroneous doctrines respecting the cerebral circulation which have been commonly entertained are examined and refuted. In the second, the frequent dependance of functional and structural cerebral affections upon a diseased condition of the heart or its membrane is set forth.

I. *On the Circulation of the Brain.*—The opinion that *the brain under all circumstances of health and disease contains absolutely the same quantity of blood*, seems so contrary to common sense and daily experience, that its disproval would seem scarcely to require the careful elaboration Dr. Burrows has bestowed upon it; but when we consider it has been and is entertained by men of the highest eminence in the profession, when names such as Monro, Abercrombie, and Clutterbuck can be cited in its favour, it is evident that something beyond a mere negation of its correctness is called for. Its defence is based upon the mechanical structure of the cranium, and experiments upon animals. The cranium, it has been said, is a perfect sphere, the contents of which are protected from the effects of atmospheric pressure. Allowing that the bony case prevents that direct effect of pressure upon its soft contents which some other portions of the body are exposed to, the indirect effect exerted upon the blood entering at the orifices of the base is no less obvious.

In regard to the experiments upon animals, those which were performed by Dr. Kellie, and recorded in the first volume of the Medico-Chir. Trans. of Edin., have been quoted, first by Abercrombie, and subsequently by various other writers. The following are Dr. Kellie's propositions:—

"1. That a state of bloodlessness is not discovered in the brains of animals which have died by hæmorrhage; but, on the contrary, very commonly a state of venous cerebral congestion. 2. That the quantity of blood in the cerebral vessels is not affected by gravitation, or posture of the head. 3. That congestion of the cerebral vessels is not found in those instances where it might be most expected; as in persons who die by hanging, strangulation, suffocation, &c. 4. That if there be repletion or depletion of one set of vessels (arteries or veins) in the cranium, there will be an opposite condition of the other set of vessels."

Dr. Burrows submits these experiments of Dr. Kellie to a renewed

examination, and is naturally surprised at finding that, in contradiction to the *first proposition*, instances of a sheep and dog are given, whose brains, after they were bled to death, contained considerably less quantity of blood than did that of a dog who was killed by tying the jugulars and carotids. In the face of such a contradiction Dr. Burrows saw no other resource than the institution of new experiments. He killed two rabbits, the one by opening the blood-vessels of the throat, and the other by strangulation. On examining the brains of each, 24 hours after, he found that of the former completely blanched, and that of the other as completely congested. Plates representing these conditions are appended to the volume. "In fairness to Dr. Kellie I should state, that I have attended at the slaughtering of sheep by butchers, and find the brains of those animals much less depleted than the brains of rabbits which have died of hæmorrhage. But the sheep did not die of simple loss of blood; but partly from the division of the pneumo-gastric nerves and cervical portion of the cord. These lesions, no doubt, influenced the appearances." The experiments which Dr. Burrows has made in reference to the influence of *posture* are also in contradiction with the conclusions of Dr. Kellie; for animals placed under the same circumstances after death, excepting the dependent condition of the head, presented very different appearances of the brain as regards congestion.

"It may now be asserted that the encephalon is not exempted from the law in physics—the gravitation of the fluids to the lowest part of the corpse. The discovery of the operation of this force on the blood within the cranium after death, suggests a precaution very essential to be followed, when it is desired to ascertain the precise amount of congestion of the cerebral vessels at the time of death. In such cases, a ligature should be placed around the throat, and drawn sufficiently tight to compress the cervical vessels, and arrest all flow of blood through them. This precaution will be most required in the examination of bodies, where, from the kind of death, the blood may be suspected to remain fluid in the heart and great vessels. The depending or elevated position of the head during the examination of the body, will not then induce deceptive appearances, which mislead us in our conclusions as to the previous amount of congestion in the cerebral vessels." P. 21.

To support this third proposition, Dr. Kellie adduces some instances of the brains of persons who had been hung presenting no remarkable congestion. Dr. Burrows, although maintaining that congestion of the cerebral vessels is generally produced by this or any other description of death which kills by obstructing respiration, admits that such congestion is occasionally absent. He attributes this to the rope having imperfectly compressed the external jugular on one side, and still more to the subsidence of the fluid blood during the subsequent suspension of the body, through the unobstructed cervical vessels, as also through the vertebral sinuses and spinal veins. In examining the bodies too, of those who have died from strangulation, the neck is usually cut across, and the thoracic organs inspected prior to the brain, the blood from which last thus becomes discharged through the divided cervical vessels.

Dr. Kellie's fourth proposition is founded upon the assumption that, the amount of blood contained within the cranium is always the same. This, we have seen, Dr. Burrows denies; and therefore does not admit the necessity of the adjustment of the equilibrium between the two classes of

vessels. He believes, indeed, that the amount of *extra-vascular serum* in the cranium is very variable, depending upon the amount of blood in the arteries or veins being more or less than normal.

On Vascular Pressure within the Cranium, and its Influence on the Functions of the Brain.—Dr. Abercrombie denied the existence of such pressure—1, because "the cerebral substance is composed of inelastic fluids which are incompressible;" and 2, "because the brain is incompressible by any such force as can be conveyed to it from the heart through the carotid and vertebral arteries." In reply to the first of these, Dr. Burrows observes that the brain, although very incompressible, is highly elastic. The existence of an outward pressure produced by the passage of the blood into the vessels is seen when a portion of the cranium has been destroyed by accident or disease. Movements of the brain, dependent upon the arterial pulse and upon the respiration, have long been observed and commented upon under these circumstances. Whatever diminishes the accession of arterial blood to the brain, diminishes also the former of these, while ligatures around the external jugulars much enfeeble the latter. In the ordinary condition of the cranium this pressure is reflected back upon the brain.

"In conclusion, I believe it to be most important to bear in mind, when considering various pathological states of the brain, that its substance not only contains a variable amount of blood at different times, but that it is also subjected to a constant vascular pressure. This pressure arises partly from arterial and partly from venous distension: also, it is increased during expiration, and diminished during inspiration: and although the substance of the brain is very unyielding and incompressible, nevertheless it sustains, and is influenced by, this vascular pressure. I consider it essential, in studying the physiology or pathology of the brain, to have constantly in remembrance the existence of this vital force. Now vital forces, just as the most efficacious remedies, when they exceed or fall short of their proper amount, are capable of producing the most serious ill effects in the animal economy. Numerous causes may affect this momentum of the blood in the vessels of the head, and hence give rise to very different degrees of vascular pressure: at one time it may become excessive, oppress the organ, and suspend its functions: at another time, it is insufficient, and seems to be inadequate to sustain the cerebral functions. The injurious effects of modifications of this pressure on the brain would be much more often exhibited, were it not for the ample development of the venous system in the cranium and spinal canal, which affords such ready exit for redundant blood; and for another peculiarity in the anatomy of the parts contained in the cranium. The peculiarity to which I now advert, is rarely pointed out by teachers, and is not sufficiently estimated by pathologists. I allude *to the large amount of extra-vascular fluid in the cranium*, even in health, and which, in the form of serum is found in the ventricles and membranes of the brain, as well as disseminated through its substance. This fluid, very appropriately designated *cephalo-rachidean*, or *cerebro-spinal*, varies greatly in amount at different times: and, from the anatomy of the parts, as well as from experiments, it would appear that a portion of this fluid readily changes its site from the cranium to the spinal canal, and conversely." P. 50.

The existence of this serum during life has been shown by the experiments of Magendie, Longet and others, and anatomy exhibits the free communications which exist between the ventricles and the cavities of the

spinal and cerebral membranes. The vivisections of Ecker and others show us also that the situation of this fluid may be subjected to considerable change by modifications of pressure. Dr. Burrows regards this fluid as supplemental to the other contents of the cranium—increasing or diminishing in quantity inversely to the quantity of blood and serous matter contained within it. It may also be a means of equalizing pressure over the whole cerebro-spinal mass. When arterial or venous congestion of the brain is suddenly induced, the increased pressure which is exerted expels a portion of this serum into the spinal canal, while, when blood is abstracted from the cranium, the vacated cranial space is occupied by the spinal serum. When the cranium is healthy and normal, the cerebral substance may in this way accommodate itself to a temporary increase of blood; but when the increased determination to, or obstruction of return of blood from, the brain becomes more permanent, or the cranium contains other abnormal substances, the additional pressure will not be borne. Whenever there is an increased amount of solid matter in the cranium, whether from hypertrophy, tumours, extravasation, &c., whatever excites the heart's action and temporarily increases the degree of vascular pressure must increase the disturbance of the functions of the brain. "It seems to me probable that many permanent structural lesions within the cranium do not affect the functions of the brain by pressure, except when there is some cause in operation capable of inducing vascular congestion, or when the lesion is of a mechanical nature, or is gradually increasing."

On the other hand, whatever diminishes the heart's power in these morbid conditions of the brain mitigates the cerebral symptoms; but if such deficiency prevails in the healthy state of the brain, this organ suffers then from *insufficient vascular pressure*, and *syncope* results, this state being produced by such deficient pressure of the brain, and not, as usually supposed, from the inadequate supply of blood furnished to it. Thus, in its most simple form, it may be induced by a mere moral emotion diminishing the heart's energy, so that the blood is not propelled with sufficient energy to maintain an adequate pressure upon the cerebral substance. On account of the additional labour imposed upon the heart, the syncope is more likely to occur if the person is erect, and is relieved by his assuming the horizontal posture. The greater rapidity with which this state is induced by the abstraction of blood in the erect posture proves that the posture rather than the amount of blood lost is the efficient cause.

"I am the more anxious to direct attention to the foregoing explanation of the phenomena of syncope, because a very different opinion has been advanced in a recent work (Lib. of Medicine, Vol. 2) in extensive circulation among the junior members of the profession. Thus, in an Essay on Apoplexy (p. 92), it is asserted that syncope differs from apoplexy only in the extreme feebleness of the heart's action; but the cause producing loss of consciousness, sensation, and motion, is stated to be the same in both affections. In either case, it is said, owing to the peculiarities of the circulation within the cranium, *pressure* is exerted on the brain; and in some cases it is difficult to distinguish the states of apoplexy and syncope from each other. 'Thus either from increased or diminished action of the heart, pressure on the brain may be produced by over-distension of its vessels; in the first case, of its arteries, and in the second, of its veins.' Here we find it promulgated that apoplexy and syncope are to be attributed to the same physical cause, viz., pressure on the brain; and that in syncope

the pressure arises from the diminished action of the heart occasioning fulness of the cerebral veins. Now, I believe, that so far from syncope being occasioned by pressure on the brain, it will be found, as I have stated at some length, that every method of diminishing vascular pressure on the brain to any great extent, whether it be accomplished by depressing moral emotions, by sudden loss of blood, by the erect posture of the body, or by contrivances which diminish the momentum of the blood flowing towards the brain, will almost certainly induce syncope.

"But if syncope be produced by venous congestion, causing pressure on the brain, would any practitioner of experience attempt to overcome it by the use of those remedies most likely to diminish venous congestion of the brain, and the consequent pressure on that organ? Would he be bold enough to place his fainting patient in the erect posture, or draw blood from the jugular vein? I have also shown by experiments, that when an animal is bled to the point of fatal syncope, that, so far from finding venous congestion of the brain after death, all its vessels are, on the contrary, ex-sanguine. It appears to me, that syncope differs from apoplexy in every respect but in this one, viz., that in both there is a total temporary abolition of the functions of the brain. The causes producing the abolition, and the means to be employed to restore the functions of the brain, are generally quite opposite. Without presuming to be hypercritical, there is cause to regret that such erroneous doctrines as to the nature of so alarming a condition of the system as syncope should have been disseminated by modern writers." P. 64.

Dr. Burrows does not attribute the disturbance of the functions of the brain which takes place in *anæmia*, so much to the actual deficiency of blood in the organ, as to the defective amount of vascular pressure exerted upon it. Thus, in *anæmia from hypertrophy of the brain*, in which the dense cerebral mass is dry and destitute of blood, none of these symptoms are present: but in general anæmia, resulting from loss of blood, the various severe nervous symptoms are all at least temporarily mitigated by posture, stimuli, and whatever favours the momentum of the blood entering the brain.

The effects which have resulted from the *ligature of one or both carotids* also illustrate the doctrine here laid down. Mr. Key tied the right common carotid in a woman æt. 61, and she died in four hours, having manifested stertorous breathing. The carotid on the right side was found nearly obstructed and the vertebrals small. The brain was healthy, and the sudden diminution of the momentum of the blood in the cerebral arteries seems the only mode of explaining her rapid death. Although such a result does not usually follow the ligature of one common carotid, yet a reference to the numerous cases, collected by M. Longet and Dr. Chevers, show the frequency of the speedy supervention of marked disturbance of the cerebral functions or the subsequent occurrence of hemiplegia dependent upon cerebral disorganization.

"These subsequent phenomena appear to me to arise from two causes; partly from the insufficient supply of blood to the disorganized hemisphere of the cerebrum, and partly from the compression of the exsanguined hemisphere by its fellow, the vessels of which still continue to be liberally supplied with blood. In healthy states of the circulation within the cranium the forces distending the blood-vessels in either cerebral hemisphere are equal, opposite, and counterbalance each other: but so soon as the free supply of blood to one hemisphere is cut off by the ligature of the common carotid, the vascular distension in the other hemisphere becomes a source of pressure on the exsanguined side,

Hence probably the cause of the commencing hemiplegia, which gradually increases with the disorganization of the cerebral substance. I am inclined to attribute the successful termination of this operation in some cases to the opportune loss of blood from a wound in the throat prior to the application of the ligature;* or the same happy result may be ascribed to a cautious preparatory venesection before the common carotid has been secured. By such loss of blood the circulation has been quieted, and the difference in the momentum of the blood in different parts of the brain has been, if not obviated, at any rate very much diminished. It might be objected that the effects upon tying one of the carotids in the human subject are too diversified to admit of the foregoing explanation; but, in reply to such objection, it may be urged, that, although certain effects are usually produced by large abstractions of blood, still the results of blood-letting are very dissimilar in different individuals." P. 77.

The effects which result from obstructing the circulation of blood through the carotids have induced some practitioners to seek from it a remedial agency. Thus, Mr. Prescott of Calcutta, has tied the carotids for the relief of severe *epilepsy* and *cephalea* in two cases; and Dr. Parry employed compression of these vessels in mania, headache, vertigo, &c. When, however, we consider the irremediable disorganization which is often present in obstinate cases, the difficulty of assigning in many the precise causes of their production, and the temporary character of the remedy, by reason of the quick re-establishment of the circulation by anastomosis, we are surprised that so careful a practitioner as Dr. Burrows should recommend this operation.

"Although the ligature of the common carotid is attended with risk to life in some cases, (perhaps in the proportion of one death in four operations) still experience proves that, where proper precautions have been taken, the operation is not so dangerous as many suppose. Therefore, in violent and hopeless cases of epilepsy and some kindred maladies, which are characterized by extreme cerebral congestion, it appears to me that, other remedies failing, this operation may be fairly resorted to. I am aware of the responsibility of advocating a remedy attended with risk of life; but are not all our best remedies most violent poisons in the hands of the unskilful? But this truth does not forbid their use to the

* An interesting case in which both carotids were successfully tied has been recently recorded by Dr. Ellis in the *New-York Journal of Medicine*, *Sept.* 1845. Pettish Hill, æt. 21, was accidently shot, *Oct.* 21, 1844. The ball passed from just above the spine of the left scapula along the neck into the mouth, making its exit through the upper lip. There was little hæmorrhage, and he was transported twelve miles in a litter. On the seventh day hæmorrhage from the wound of the tongue occurred, but was readily suppressed by compressing the left carotid. On the eighth day, hæmorrhage recurring, the left carotid was tied below the omo-hyoideus. No unpleasant symptoms followed, but on the eleventh day, when a slight pulsation of the temporal artery was felt, hæmorrhage recurred, and again the next day, being temporarily arrested by pressure on the right carotid and on the orifices of the wound. This, however, caused too much pain to be borne, so that, *four days and a half after* the first operation, the *right carotid* was tied. The only bad symptoms which followed were some dyspnœa and cough, which were relieved by a small depletion and then giving small doses of aconite. At the date, *June* 18, 1845, the young man enjoyed comfortable health, and was attending to business. No pulsation could then be felt in either temporal artery.—(*For a Case by Dr. Warren, see our present Periscope.*)

more expert. So may this powerful method of influencing the cerebral circulation be justifiable in aggravated cases of the class referred to, and where the precept of Celsus *satius est anceps remedium experiri quàm nullum*, may be fairly put into practice." P. 79.

On Apoplectic Coma.—Upon the nature of coma in apoplexy authors afford us no efficient explanation, Abercrombie confessing his ignorance of it. Dr. Burrows believes that too exclusive an attention has been paid to the recognition of visible physical cause of pressure, and suggests that there are several causes capable of suspending the functions of the brain in the same manner as experiment proves their power in destroying those of the cerebro-spinal nerves. These are 1, pressure on the nervous fibres; 2, division of nervous substance; 3, disorganization of nervous matter; 4, interrupted supply, or deficient momentum of blood in the venous substance; 5, narcotics. Numerous cases have been recorded in which death with all the symptoms of apoplexy has occurred, and yet the brain found to be in its normal condition. Various explanations have been proposed by those who maintain the invariableness of the quantity of the blood contained in the cranium: but Dr. Burrows believes that in such cases a congested state of the vessels, produced either by determination of blood to or its detention in the brain, has existed. Of the three cases recorded by Abercrombie two had been freely bled, and no considerable turgescence of the vessels was observed; but in the third, treated only shortly prior to death, the vessels were turgid and the substance of the organ highly vascular. But, supposing this congested state of the brain be relieved by depletion, and no structural lesion be discovered, how do we explain the occurrence of death? This, Dr. Burrows is disposed to refer to the brain becoming saturated during the suspension of consciousness, and the existence of stertorous breathing, with undecarbonized blood, so that the patient actually dies from the effects of imperfect respiration, and not from pressure or lesion of the brain.

The ordinary explanation of the occurrence of death in *sanguineous and serous apoplexy* by the compression of the brain exerted by the effused fluids, is also deemed erroneous by Dr. Burrows. Portal, Abercrombie, and other high authorities admit that the distinction between serous and sanguineous apoplexy is not a real one, that the serum is only secondary upon pre-existing congestion, and that the symptoms bear no relation to the amount effused. The coma cannot then be dependent upon the effused serum, nor is it so when the extravasated fluid is blood; for large and old extravasations have been found with no coma, while this last has prevailed in cases in which slight and recent extravasation only existed. Two cases, which fell under the author's care, and in which transient coma only manifested itself with copious cerebral hæmorrhage, are related. These and numerous similar ones, prove that "extravasation of blood cannot of itself be looked upon as the cause of the coma, although it is so of the paralysis concomitant with the coma, and which remains after the coma has disappeared." The coma, in the majority of cases, is attributable to *attendant and co-existent congestion.*

"When extravasation of blood within the cranium takes place very slowly, as from the rupture of the diseased coats of an artery, independent of determina-

tion of blood to the brain, or general congestion of its vessels, it is highly probable that the effusion does not produce the symptoms of apoplectic coma, but gives rise to some modification of hemiplegia, the paralysis being to a greater or less extent, according to the situation and quantity of effusion. As long as extravasation is actually going on, while the blood is pouring forth, it most probably produces pressure on the surrounding cerebral substance, just as the blood escaping from a divided artery in any other part of the body would compress any obstacle with a force equal to the momentum with which the blood was circulating in that vessel. When cerebral hæmorrhage has stopped, I suspect the blood ceases to be a real source of general pressure, although, as it increases the quantity of extra-vascular matter in the cranium, it also offers additional resistance to the entrance of the normal quantity of blood into that cavity; hence healthy vascular distention becomes excessive, and the symptoms of general cerebral pressure are easily developed. Thus the compression produced by extravasation will depend more on the rapidity and situation of the effusion than on the amount. If the effusion be slow, and not near the base, although the amount be considerable, the effects will be slight in comparison." P. 101.

As the coma may exist without effusion and effusion without coma, in the majority of cases, the coma is therefore due to the pressure induced by vascular congestion. Other causes may induce coma, as for example the circulation of venous blood through the brain; but this is the explanation offered of the coma of apoplexy. That ordinary causes of determination of blood to the brain, such as violent exercise, vivid emotions, and intemperance, do not oftener lead to the production of coma may be attributed to the anatomical provisions for preserving the uniformity of pressure and an easier circulation of blood provided by the tortuosity of the arteries and the arrangement of the sinuses of the dura mater; while the facility with which the extra-vascular serum escapes into the spine is an additional protection against the effects of such sudden congestion.

II. *On the connection of Apoplexy and Hemiplegia with Diseases of the Heart.*—Dr. Burrows believes the profession is by no means sufficiently aware of the extent of the influence which diseases of the heart exert in inducing functional and structural diseases of the brain. If the explanation of the cause of apoplectic coma be correct, it may be expected that a diseased condition of the heart or its valves, so commonly found co-existent with congestion in other parts of the body, will be frequently present. But none of the standard writers on this disease seem to have noticed such connection prior to Portal, who was much in advance of his German and English cotemporaries. Drs. Kellie and Clutterbuck deny the connection between the diseased condition of the one organ and that of the other, and Dr. Abercrombie seems entirely to have overlooked it. Bright and Andral however admit this connection, but it is by Dr. Hope that it has been placed in the strongest light. He believed, with M. Bouillard and Bertin, that hypertrophy of the heart predisposes more strongly to apoplexy, than even what is termed the apoplectic constitution.* Richerand agrees in

* At a subsequent page (143) Dr. Burrows has an interesting note upon this so-called constitution. He says—

"It is a popular belief that persons with a peculiar conformation of the body, namely with large heads, red faces, short necks, and capacious chests, are predis-

this opinion; and Dr. Copland admits that the lesions co-exist too frequently to be explained by mere coincidence.

Amid such difference of opinion Dr. Burrows has endeavoured to form a statistical account of the frequency of disease of the heart in cases of apoplexy and hemiplegia; but has found this a matter of difficulty, inasmuch as few authors have afforded precise information as to the condition of the heart in this affection, being content with recording the lesions they have met with in the brain. However, from such sources as furnished the necessary data, and from cases observed by himself, he has drawn up the following table of 132 cases.

		Diseased Heart.
Andral . .	25	15
Clendinning	28	15
Hope . .	39	27
Burrows . .	34	23
Guillemin .	6	4
	132	84

From this, it would seem that three-fifths of a given number of cases of apoplexy or sudden hemiplegia manifest distinct signs of disease of the heart. M. Rochoux, however, states that, of 42 cases examined by him prior to 1818, three only presented hypertrophy; but upon examination of the thirty cases detailed in his work, Dr. Burrows finds that the state of the heart was examined only 14 times, and in four of these there was disease of either the muscular or valvular structures. It is to be remembered too that lesions of the heart were very imperfectly appreciated prior to 1818, and that M. Rochoux restricts the term apoplexy to cases in which actual hæmorrhage is discoverable. In reference to the question of which of the cardiac lesions most frequently gives rise to cerebral affections, Dr. Burrows states that, of the 38 instances of diseased heart observed by Andral and himself in 59 cases of apoplexy and hemiplegia, 19 were examples of hypertrophy with valvular disease, 10 of simple hypertrophy and 8 of valvular disease.

"I have thus endeavoured by facts and arguments, to point out the frequent and intimate relation subsisting between structural changes in the heart and these cerebral affections. This relation appears to me in many cases to be that of cause and effect. I have already quoted the opinions of Portal and some others who have entertained a similar view of the pathology of these cerebral

posed to apoplexy: and that persons of spare habit with longer necks are exempt from that disease; so that if a person of this latter description is attacked with apoplexy or hemiplegia, considerable surprise is expressed. My experience causes me to doubt the accuracy of these opinions. The former class of individuals are usually the subjects of considerable hypertrophy of the heart, and hence suffer from habitual determination of blood to the brain, and perhaps hypertrophy of that organ. No wonder that they should suffer from attacks of apoplexy. But I have met with many instances of apoplexy and hemiplegia among the poor, where the individuals have been pallid and attenuated, with slight figures; in fact, presenting the very reverse of the so-called apoplectic make; and in such cases, upon making a careful scrutiny of the heart and lungs, I have discovered signs of valvular disease in the heart, or perhaps of extensive emphysema of the lungs; and these diseases probably combined with changes in the arterial coats."

affections; but I am not aware that any other author has presented so connected and extended a review of the arguments and facts, which ought to have the effect of establishing a pathological doctrine of great importance in its application to the treatment of these disorders of the brain.

"Although I have thus maintained the paramount influence of the heart, both in its healthy and diseased states, upon the circulation and functions of the brain, still I am fully sensible that lesions of other organs, especially of the lungs, kidneys, and liver, have a similar, though less frequent and direct, influence in disordering the cerebral circulation, and producing apoplexy, hemiplegia, and epilepsy.

"In opposition to the opinions entertained by many respectable authorities, that the quantity of blood within the cranium is at all times nearly the same, and that the heart does not influence the cerebral circulation, my own observations, supported by facts already detailed, convince me that in many, perhaps the majority, of cases of apoplexy and hemiplegia, the primary disease is not situated within the cranium. I would go further, and affirm, that in many cerebral affections apparently depending on effusions of serum or blood, there is no farther primary disease of the brain than there is of the cellular tissue in anasarca, or of the peritoneum in ascites, or of the skin in purpura, or of the stomach in hæmatemesis. There is, indeed, a palpable morbid condition of these several tissues and organs where the effusion or ecchymosis takes place: but it is generally dependent upon a morbid state of some other viscus, which greatly interferes with the circulation in the parts where the effusions are detected. A hypertrophied left ventricle, or valvular obstruction in the heart, will lead to lesions within the cranium similar to those observed in the stomach and peritoneum, when there is obstruction to the circulation through the portal veins in the liver.

"If the pathology of the brain in apoplexy and hemiplegia be analogous to that of other organs which suffer from effusions of serum and blood, how much must this knowledge improve the routine treatment of apoplexy, which has so extensively prevailed. Does not this view of its pathology render more intelligible those different varieties of the disease, which are described by ancient writers, although they could not account for the differences?" P. 125.

Period of Life most prone to Apoplexy and Hemiplegia.—Dr. Burrows observes, that the statements of authors as to the liabilities of different ages are mostly erroneous in not being compared with the numbers living at such periods of life. He institutes an analytical comparison of this kind in 215 well-marked cases; whence it appears that the number of apoplectic cases increases in each successive decennial period from 20 to 70 years of age, while the numbers living gradually diminish. From the researches of Dr. Clendinning it also appears that the proportionate weight of the heart increases with advancing life—so that hypertrophy of that organ is a change which is concomitant to the period of life when apoplexy is most prevalent. Dr. Clendinning has moreover shown that, while the average weight of the adult brain, when the heart was healthy, was 50·5 ounces, in diseases of the heart it was 52·5 ozs.—a condition he regards as the effect of the cardiac disease. Even a moderate, temporary, congestion of a brain already abnormally large may easily induce an apoplexy; and in persons of advanced age, the cerebral arteries being diseased in structure, may easily give way to even moderate congestion.

1. *Treatment of Apoplexy and Hemiplegia.*—During the *attack* more attention to *raising the head* than is usually bestowed is desirable, for its power of depleting the cerebral vessels is, as proved by the author's ex-

periments, very considerable. In deciding upon the propriety and extent of *bloodletting*, Dr. Burrows lays the greater stress upon the importance of the indications derivable from *cardiac auscultation*. Without this, the condition of the *pulse* will be found in many cases to be very perplexing. If no disease of the heart can be discovered, or if this consist in simple hypertrophy, depletion may be carried as far as the cerebral symptoms demand.

"But, suppose the examination of the heart discloses the existence of valvular disease to the extent of obstructing the circulation through its cavities, here the pulse will be a most deceptive guide as to the propriety or impropriety of abstracting blood. If the mitral valve be principally implicated, and allow of regurgitation from the left ventricle, the small and irregular pulse so commonly observed with that lesion, would probably dissuade from the free abstraction of blood which the cerebral symptoms might require. If, in another case, the aortic valves be found diseased to the extent of not only obstructing the onward current of blood, but also of allowing regurgitation into the ventricles during its diastole, there will propably be associated with this lesion considerable hypertrophy of the left ventricle. Here will be observed a full and vibrating or thrilling pulse, but a pulse of increased action without real power, and hence a deceptive pulse; and one which, if it be regarded without reference to the structural changes of the heart, would invite to a more copious abstraction of blood than was called for by the general symptoms. In each of these last-mentioned cases greater relief to the symptoms will be obtained by a free local abstraction of blood from the vicinity of the heart (either by cupping from beneath the left mamma, or between the left scapula and spine) than by a much larger depletion by venesection." P. 142.

Auscultation would also dissuade us from large depletion, notwithstanding a hard and full pulse, if by it we detected serious valvular disease or ossific deposits. In other cases we may find dilatation of the cavities of the heart and extensive emphysema of the lungs, and be deterred from aggravating by large depletion the condition of the heart which has induced the cerebral congestion, notwithstanding that great congestion and dyspnœa may be present. "I should suggest the employment of cupping-glasses to the nape of the neck, or between the scapulæ, with the internal administration of stimulant diuretics, diffusible stimulants, and the application of rubefacients to the sternum."

2. *The treatment during the Stage of Cerebral Excitement supervening soon after the seizure.*—The pain in the head, flushing of face, knitting of brow, &c., together with the more active condition of the circulation, which supervene after recovering from the depletion, &c., employed during the attack, are symptomatic of inflammation commencing around the effused clot, and are generally markedly relieved by local depletion, application of cold, purgatives, spare diet, and quietude. When the heat of scalp has subsided, a blister to the occiput relieves the oppressive headache; and if the patient be not very aged or much exhausted, small doses of mercury, short of inducing ptyalism, are useful. Together with the above symptoms, or after they have disappeared, the patient may be subjected to a spasmodic and distressing neuralgic condition of the palsied limbs. The author has tried numerous local remedies for these severe pains, but with little success. When symptoms of remaining cerebral irritation have continued, he has found slight local depletion from the

head and evaporating lotions to the limbs the most useful proceeding, and where symptoms of cerebral irritation have been absent, leeches applied to the limb itself have proved serviceable.

3. *Treatment of Paralysis following Apoplexy.*—When patients have suffered only a slight attack, or have been quickly relieved by prompt treatment, they sometimes incautiously are allowed to put themselves too fast forwards (although still suffering from the palsy,) as regards getting up, mental occupation, diet, &c. In this way, they endanger the production of a fresh extravasation, before the first one is encysted, and the softened brain has recovered its consistency and excitement or inflammation, and consequent disorganization of the cerebral substance may too be induced. Some cases illustrative of the ill effects of such imprudence and incautiousness are related.

When sufficient time has been allowed for the restoration of the cerebral substance to its normal condition, we may proceed to endeavour, by means of counter-irritants, to excite the suspended functions of the nerves of the palsied limbs. These are sometimes useful, and always sure to employ the attention of the patient. Dr. Burrows thinks little of the utility of electricity, and still less of that of strychnia, which indeed sometimes changes the wearing pains of the limbs into acute suffering. Regular frictions and well-devised exercises of the limbs are useful.

Functional Disturbance of the Brain, induced by Diseases of the Heart.—Not only may cardiac disease give rise to the serious and fatal lesions of the brain now adverted to, but to various other symptoms indicative of disturbed cranial circulation. "Recurring attacks of vertigo, headaches, rushing of the blood to the head, epistaxis, somnolency, nervous irritation, and even insanity, may often be traced to the operation of cardiac disease, which has not attracted the notice of the patient or his medical attendant."

Dr. Burrows relates several cases of *epistaxis* dependent upon the disturbed state of the circulation consequent on heart-disease. The congestion of the cerebral vessels so induced may, in one case (or at different periods of the history of the same case,) give rise to an epistaxis, and in another to an internal hæmorrhage. "If the foregoing observations be correct, they impart an additional significance to the occurrence of epistaxis, which is often regarded as an isolated and unimportant symptom. This hæmorrhage may, I believe, often be considered as strictly pathognomic of an obstructed circulation through the heart, as hæmoptysis is symptomatic of tuberculated lungs, or intestinal hæmorrhage of an indurated liver." Dr. Latham first called the author's attention to another class of cases manifesting various cerebral symptoms the chief of which is *headache*, and in whom the original cause of the symptoms is hypertrophy of the heart without valvular disease. These persons have usually been spirit-drinkers; they are liable to profuse hæmorrhages, and ultimately become the subjects of general dropsy. Their faces are usually pallid, and their pulse peculiarly hard and incompressible. Corvisart has forcibly alluded to the aggravated forms of disturbance of the mental and other cerebral functions which attend aggravated diseases of the heart, plunging the patient into despair, and sometimes leading him to suicide.

Affections of the Brain and Spinal Cord dependent upon Acute Diseases of the Heart.—The observations hitherto made apply only to the chronic structural changes of the heart; but acute diseases of this organ may also give rise to symptoms so indicative of severe affections of the nervous centres, that the primary cardiac affection may be entirely overlooked even by experienced observers. Several of such cases have been published from time to time, but Dr. Burrows is the first who has assembled any number of them (sixteen) together, and endeavoured to connect them by a satisfactory explanation in extension of the principles already laid down. They consist of examples of rheumatic or idiopathic pericarditis or carditis in which the symptoms were those of inflammation of the brain, delirium, myelitis, insanity, coma, chorea, or tetanus. In respect to this last order of morbid manifestations, Dr. Burrows observes that it behoves us, in idiopathic trismus and tetanus, the pathology of which is so obscure and the treatment so unsuccessful, to carefully examine for this source of eccentric irritation. "It is a melancholy reflection, but I fear a just one, that numbers have perished from these supposed diseases of the spinal cord, when in truth the morbid action has been in the heart, although that has not been detected." Dr. Bright has already (Med. Chir. Trans., vol. 22) signalized the frequent occurrence of chorea as a result of rheumatic pericarditis.

From the detail of these cases it is evident that "the most formidable diseases of the brain and spinal cord may arise from irritation of the nerves of the heart, without any structural change in the nervous centres themselves." Some have supposed that they only occur in connection with rheumatism and its consequent pericarditis; but of the 16 cases here detailed, no rheumatic affection existed in seven. In two or three the pericarditis was idiopathic, and in the others came on in the course of various chronic diseases. Dr. Burrows, without denying the possibility of metastasis to the encephalon, states that, in no one of the 11 fatal cases enumerated was any trace of disease of the brain or its membranes discernible; and in four fatal cases recorded by Dr. Watson, an accumulation of serum beneath the membrane was the only morbid symptom discovered. Dr. Bright, remarking that in the cases of chorea induced by pericarditis, inflammatory action was found on the exterior of the pericardium, as well as on the pleura, infers that the phrenic nerve distributed over these parts is the medium of communicating the irritation to the spinal cord. M. Bouillaud likewise has observed that the nervous disturbance in pericarditis especially occurs when this affection is complicated with diaphragmatic pleurisy. Such cases are, however, not necessarily attended with nervous excitement; while Dr. Burrows has observed a few intances of pericarditis unconnected with pleurisy, in which such existed. Dr. Hope refers the peculiar expression of features attendant upon bad cases of pericarditis to the propagation of the irritation through the pneumogastric nerves to the spine, and its reflection hence to the face through the portio dura. Dr. Watson explains the nervous disorders in these cases simply by the disturbed state of the cerebral circulation induced through the embarrassment of the heart's action by the inflammation of its tissues.

"In collecting and collating the foregoing examples of endocarditis and pericarditis, my object has been to draw attention more closely to a class of cases, the real nature of which is so likely to be overlooked; and to enforce the neces-

sity of an early examination of the heart by means of auscultation, in all obscure and intractable affections of the brain and spinal cord. The advantages of such an examination are rendered very conspicuous by a comparison of the relative mortality of the cases where the cardiac disease was detected, and of those where it was not suspected during life. Of the 16 recorded cases, 11 proved fatal, and only 5 recovered. In 4 of the successful cases the diagnosis of cardiac disease was satisfactory, and only suspected in the 5th. In only 2 of the 11 fatal cases was an affection of the heart detected during life, in 1 it was suspected, and in the remaining 8 cases there was no suspicion of acute disease of the heart until it was revealed by examination after death.

"I shall conclude this section with some observations upon the treatment of these cases. It appears that only 5 of the 16 terminated favourably. In 4 the cardiac disease was detected at an early stage, and remedies employed to control the exciting cause of the nervous symptoms. These remedies were the abstraction of blood by venesection, and by cupping from the region of the heart, the application of blisters over the cardiac region, and the free administration of mercury combined with opium, so as to produce mercurial affection of the mouth. In two other cases the disease of the heart was indeed detected during the life of the patient, but nevertheless it ended fatally. It will be instructive to inquire into the cause of the want of success in the treatment of these two. In the one, although a slight affection of the heart was discovered on the day of the patient's admission, still it was not until two days afterwards, and only 24 hours before the boy's death, that the physical signs indicated the existence of pericarditis, and it was only from that period that the active remedies above described were employed to subdue the cardiac inflammation; but they proved ineffectual. Upon the other occasion the pericarditis was only discovered the day before the patient's death, when his system was already exhausted by general dropsy of some weeks' duration, and by five days' continuance of the peculiar urgent nervous symptoms which sometimes indicate the presence of active cardiac inflammation. The want of success in the treatment of these two cases is sufficiently accounted for by the late period at which the cardiac disease was detected." P. 214.

The very complete analysis we have presented of the contents of this work supersedes any lengthened statement of the value we attach to these. Our readers are in a condition to judge for themselves, and we much mistake if they do not pronounce their verdict in favour of the author. That he has ably expounded an important physiological principle, and exhibited a too neglected pathological condition is plain enough, and the only difference we have with him is one of degree. Like most observers whose attention has been directed to some particular subject, he perhaps extends the application of his doctrines to a larger proportion of cases than a farther investigation will warrant. When too we observe so many instances of heart-disease, the subjects of which manifest no cerebral symptoms whatever, we feel the necessity of caution lest we sometimes mistake a mere coincidence for cause and effect: and we believe that apoplexy is more frequently dependent upon a local diseased condition of the blood-vessels or cerebral pulp than the author seems willing to admit. However, there can be no doubt the connexion of the lesions is in many cases such as he represents it; this forms an important fact always to be borne in mind in the management of such, although the frequency of its occurrence may not be as yet determinable. We can, however, scarcely allow that the attention of well-informed practitioners has not of late been considerably directed to the production of head symptoms by heart-disease, as this has now been an established doctrine for some time past in our medical schools.

I. De la Folie, considérée sous le Point de Vue Pathologique, Philosophique, &c., &c. Par *L. F. Calmeil*, D.M.P. Paris, 1845.

On Insanity, considered in a Pathological, Philosophical, Historical, and Judicial point of view, from the Revival of Learning in Europe until the Nineteenth Century. By *L. F. Calmeil*, Physician of Charenton. Two Vols. 8vo. pp. 1050. Paris, 1845.

II. An Act (8 & 9 Vic., c. 100) for the Regulation of the Care and Treatment of Lunatics. With Explanatory Notes and Comments. Edited by *G. Forbes Winslow*, M.D. 12mo. pp. 173. London, 1845.

III. Observations and Essays on the Statistics of Insanity. To which are added the Statistics of the Retreat, near York. By *John Thurnam*, M.D. 8vo. pp. 350. London, 1845.

IV. The American Journal of Insanity. October, 1845.

V. A Letter to Lord Ashley, M.P. By *W. B. Costello*, M.D. 1846.

VI. Clinical Lectures on the Principal Forms of Insanity. By *John Conolly*, M.D. (Publishing in the Lancet.)

VII. Annual Report of the Managers of the New York State Lunatic Asylum. By *Amariah Brigham*, M.D. 1845.

The above publications treat of the past, present, and future condition of the Insane, upon each of which points we are desirous of offering a few remarks. It would be surprising indeed, if, amid the now generally prevalent disposition to search out for the causes and means of relieving the social, moral, and physical evils which afflict the unfortunate, the cause of the Lunatic, the most helpless of all sufferers, were forgotten. That happily this is not the case the labours and devotion of so many highly-gifted individuals throughout Europe and America, the progressive melioration of his condition, the warm sympathy with which the public has watched the various attempts at accomplishing this, and the readiness with which our Government and Legislature have so recently accorded most extensive means for future improvements, combine to prove.

The history of the management of the insane presents but a humiliating picture of human intelligence and progress. It does not seem that any provision for even their safe-keeping existed prior to the Reformation, and long after the establishment of "Bedlam," indeed to comparatively modern times, the more harmless were allowed to roam about, objects of the jest, contumely, and cruel treatment of the thoughtless and ignorant; while such as manifested dispositions for violence or insubordination, were sub-

mitted to a system of management, the sole rule of which was domination by brute force, and its sole result the production of an enormous amount of the most terrible suffering. Yet did not all this arise from any indisposition to succour the afflicted; for we find, as one of the natural and earliest results of Christianity, the establishment of hospitals for the sick and wounded; and the vast numbers and great resources of some of those devoted to the cure of leprosy in the middle ages prove to us that our ancestors were in no-wise wanting in beneficence, when a mode of exercising it was indicated to them. That there were no establishments at an earlier period for the treatment of the insane arose from the fact of the nature of the malady being misunderstood, and no doubt in some measure from a far less proportionate and absolute number of persons being the subject of it than in modern times; for that the number of persons liable to insanity, and the varieties of the disease, have increased in modern times, and as a consequence of the highly-wrought and somewhat artificial condition of modern society, cannot be doubted. But our forefathers failed in recognising the malady in numerous instances in which it presented itself. M. Calmeil's Treatise on Insanity is almost entirely occupied in detailing instances in which symptoms that would be clearly recognized in our day as insane delusions were believed to result from demoniacal possessions and sorcery. We must confess that prior to the perusal of this work we had no idea of the extent to which these erroneous views have prevailed on the Continent. The accusations of sorcery and witchcraft were tolerably numerous during the dark periods of our own history; but the wholesale denunciations and punishments recorded in M. Calmeil's work infinitely exceed these. During the 15th and 16th centuries, ten, twenty, or more than a hundred such persons at a time were handed over to be dwelt with by the law; and in one reign alone, that of Francis I., (1515-47,) 100,000 persons were destroyed or otherwise punished on this account. The class of persons, likewise, who endured these persecutions was different. In this country they were mostly wretched old women whose ugliness or eccentricity had rendered them remarkable; on the Continent they were usually members of some of the numerous religious convents. Numbers of women, submitted to the austerities of these abodes, became attacked with perverted religious ideas, and frequently by convulsive movements and various other hysterical symptoms. They confessed and gloried in the most revolting accounts of their profanation of the religion they had sworn to observe, as also their intercourse with and obedience to diabolical agencies. Exorcism was the remedy resorted to. Priests and bishops devoted days and nights to the employment of every known mode of expulsion; and usually, so far from any benefit accruing, the consequence was, that the disease became contagious in the convent and not infrequently epidemic in the district; so that months or years might elapse ere a decent tranquillity were restored. During their paroxysms these poor women, hitherto of irreproachable lives, confessed to the greatest enormities, and did not hesitate to accuse their dearest friends or venerated superiors as participators or originators of these. The stake was put into requisition on every side, and hundreds and thousands of poor creatures perished the victims of their own fancies or of the delusions of others. On some occasions, persons falsely accused, excited by the religious cere-

monies they were subjected to, eventually acknowledged all the enormities attributed to them, and even exaggerated these; while the very priests engaged in the exorcisms, occasionally became themselves afterwards the subjects of similar delusions. M. Calmeil reviews the particulars of all the considerable epidemics of this kind on record at great length, and justly observes that the same delusions are to be observed among our insane at the present day. Much more infrequently, however; for every epoch of history may almost be said to have its delusion characteristic of the condition of society at the time. M. Calmeil thus expresses himself upon this point:—

"It has been proved, for this last half century, that insanity is liable to take its colour from the religious persuasions, the philosophical or superstitious ideas, or the social prejudices which happen to be actually in vogue; that this colour varies in the same country according to its political circumstances, civil commotions, literary productions, theatrical representations, or the description, progress, and direction of its industry, arts and sciences. Thus, since elementary ideas of physics and chemistry have become to some extent popular among us, we find many of the visionaries disreason in our large towns concerning electricity, balloons, burning-glasses, telegraphs, air-guns, and optical experiments. What numbers, since the first experiments of Mesmer, have complained of being victims of the persecutions of the magnetizers and somnambulists. Long after the Reign of Terror had ceased to oppress France, the unfortunate persons who, during the storms of 1793, had felt the danger of oppression, and whose reason had afterwards lost its equilibrium, trembled anew for property and riches, for the preservation of their lives, and for the safety of their relatives and friends. On the contrary, those of the insane who had taken an active part in the incendiary movements of that terrible epoch, vividly exhibited their fears of the resentment of the clergy and nobility. Under the reign of Napoleon, insanity had its heroes who employed themselves in giving commands, as if they had whole armies under their direction. After the fall of the glories of the Empire, when the remembrance of our disasters, reverses, and deceptions were rankling in the hearts of all true citizens, the lypemaniacs were apprehending new invasions, cruelties and insolence on the parts of the conquerors and the Cossacks. In our own day, the melancholics are affrighted at the soldiery, the police, the jury, the courts, the guillotine, and the galleys. At all periods, the expression of insanity has undergone analogous variations. When we have once acquired a knowledge of the nature of the ideas, sentiments, and passions which are fermenting in the bosoms of societies and families, so that we are enabled to appreciate the intellectual and moral condition of the masses at different historical periods, we almost know by anticipation in what direction insane ideas must lean in the different phases of civilization.

"In the 15th century, insanity especially bore the impressions of the superstitious ideas and theological doctrines then in repute. It was necessarily so. These doctrines had been exhibited, developed and defended in the schools, taught in the religious houses, explained to every one from the pulpits, and amply commented upon to all the faithful at the confessional. Persons who then lost their reason, almost always erred in ideas or sensations relative to demons, angels, supernatural beings, just because such subjects were familiar to them, and had made a deep impression upon their minds and imaginations. It is certain that the inquisitors, who in many places performed the functions of judges, accepted the most improbable and atrocious assertions. They, indeed, went much farther than this, for they often obliged the insane to unfold the symptoms of their disease in the midst of torture, and afterwards sent them to finish their lives at the stake. The facts we shall advert to will exhibit to us the different shades of delirium which were mistaken by our ancestors. Every one

now allows that the possessed, the lycanthropes, and the demoniacs were the subjects of a morbid condition; and how must we pity so many unfortunate creatures, when an undiscovered disease led them to declare that they were in accord with the devil, to curse their Creator, to outrage Nature and Providence, and trample under foot all that should inspire man with respect and veneration? But how can we also refuse some indulgence to those who were charged with the preservation of morals and religion and the execution of the laws, when it is evident that ignorance as much as fanaticism contributed to close their hearts to pity?"

In reference to this latter point we must remember, before we charge these persons with cruelty rather than ignorance, that the belief in the power of supernatural agents was not a mere vulgar credence, but one held almost universally, and that even by persons of the strongest minds, as indeed is amply proved when we cite the names of Luther and Melancthon. Moreover, although in the 15th century ecclesiastics were almost the only persons who interfered in the management, relief, and punishment of persons supposed to be possessed, and doubtless by their exhortations, threats, and imposing public ceremonials, increased the manifestations of disease which might have been relieved by segregation and medicinal appliances—yet, even in the following century, men, bearing the important names of Ambroise Paré, Fernel, J. B. Porta, and Cardan, did not hesitate to refer such symptoms to demoniacal agency, and did not suggest more rational procedures for their removal.

We have not space to pursue the consideration of any of the numerous narrations brought under notice by M. Calmeil, whose work, as a minute and laborious record of the various insane delusions which have manifested themselves during the 15th, 16th, 17th and 18th centuries, will prove as valuable an aid to future historians of the human mind as Hecker's History of the various other epidemic visitations of the Middle Ages. For the medical inquirer the book also possesses some additional interest, from the author prefacing the history of the delusions of each century with an account of the ideas on mental pathology entertained by the leading men of the epoch—tracing them from the purely theological views which exclusively prevailed in the 15th century to the juster ones which immediately preceded those enounced by Pinel in the 18th. In this way, the opinions of Bonet, Boerhaave, Morgagni, Lorry, Bayle, Sauvages, Senertius, and especially those of our countryman, Thomas Willis, are analyzed at considerable length. The histories of the extato-convulsive epidemics prevailing among the persecuted Huguenots and Jansenists in the 18th century are detailed with great minuteness; and those who are now staring and gaping at the wonderments of modern mesmerism, may find in these narrations the prototypes of every pretension, and marvels as regards the endurance of physical suffering not yet attempted to be rivalled. M. Calmeil also enters into a detailed refutation of the pretensions of Mesmer.

If an ignorance of the pathological condition of their victims may be urged in extenuation of the cruelties practised upon them by inquisitors and exorcists, an equal ignorance of the most appropriate means of relieving or assuaging the consequences of such state prevailed long after its existence had been explicitly acknowledged. Fear was the only passion sought to be wrought upon, and no engine for its excitement was deemed

too harsh in its operation; so that the condition of the helpless lunatic, as regards positive suffering, during the last century and commencement of the present, was more hapless than that of the prisoner when prisons were such as Howard has depicted them. When we find Cullen and other enlightened and humane practitioners sanctioning the use of blows and stripes as a curative measure, we may easily imagine the dreadful cruelties which must have been perpetrated when the administration of these and of other coercive means was entrusted to brutal and ignorant persons. But a far graver reproach than that of ignorance, namely, apathy, has, until now, attached to our own country. Who could have believed that, so long after the grand experiments of Pinel in France and the Quakers in England had demonstrated the feasibility and superiority of a humane mode of treatment, the labours of Esquirol in the one country, and of Parliamentary Committees in the other, should have been enabled to expose to light the revolting abuses they laid open? Nay, at this very time, more than half a century after the above epoch, a large portion of which has been passed in profound peace, and with every circumstance favourable to the diffusion of the knowledge of improvement, the Metropolitan Commissioners have found poor wretches chained and ironed and wallowing in their excrement and filth, uncared for and unseen; while Dr. Conolly saw a patient at Rouen, who had been tightly confined in a straw crib for nine years! The perusal of the history of the Retreat near York, as detailed by Dr. Thurnam, gives rise at once to the feelings of pride and shame: pride, that so noble and so successful an experiment should have originated in our country; and shame, that so little practical good has resulted to the mass of the insane from its institution. It is true that the Retreat was visited from all quarters at home and abroad, and that its success shamed the governors of the York Asylum into an inquiry and redress of the dreadful grievances of that establishment: but the disclosures of the Committee of the House of Commons in 1815, eighteen years after the opening of the Retreat, and again in 1828, too plainly exhibited how little general practical benefit had resulted from the example. Various legislative measures have been from time to time passed, as some glaring abuse has come to light, but none of so extensive and comprehensive a character as Lord Ashley's Act, just come into operation, and passed in consequence of the representations of the Metropolitan Commissioners, whose Report we reviewed some time since.*

The object of Dr. F. Winslow's little work is to give an account of the provisions of this new Act; and all those having any thing to do with the insane will find it a most useful companion. We will advert to a few of the most important provisions of the new measure before we state our opinion of its probable operation, and of the *desiderata* it yet leaves unprovided for. In the first place, an amended system of *inspection* has been organized, without which indeed all other improvements would have been nugatory. The Commissioners are to be a well-paid and permanent body, possessing the most extensive powers for supervision and redress of grievances. Why three of the six are to be barristers it would be some-

* See Med.-Chir. Review, Jan. 1845, p. 115.

what difficult to assign a reason, except that the members of the long robe have enjoyed a monopoly of government appointments from time immemorial—so that the conceding even a moiety to our profession is very acceptable, if it be only for the novelty of the thing. All who are acquainted with the cruel and neglectful treatment which unfortunate lunatics have received at the hands of their keepers in private lodgings, cottages, &c., will rejoice to learn that henceforth this class of persons is to be submitted to licensing and inspection. Their exemption hitherto from this, and the consequent impunity of their proceedings, however nefarious, has long been a disgrace to our age and country; and—

"If this were the only amendment effected in the law, Lord Ashley would be entitled to the gratitude of every humane mind for having introduced it. * * * Independently of the horrid cruelties to which these unfortunate persons were of necessity exposed, was it possible, I ask, under these unfavourable circumstances, to carry out successfully any thing like a successful or efficient plan of medical treatment? Little or nothing could be done to re-establish mental health. Shut out from society, pent up like a wild beast in a small ill-ventilated apartment, left to the exclusive surveillance of a person whose existence, perhaps, depended upon the party continuing in a state of derangement, no prospect of cure could be entertained. As far as *data* will enable us to form a judgment, it has been satisfactorily established that the ratio of cures in cases of insanity confined in unlicensed houses, only amounted to 10 per cent., whilst in licensed it averaged 40. This is a startling fact, and ought at once to awaken the attention of those whose friends or relations are exposed to these disadvantageous results."—*Winslow*, p. 38.

Well might Lord Ashley declare that, if the calamity of insanity overtook him, he should wish to be placed in a public asylum. Wealth in these dreadful cases, instead of as usual procuring an assuagement of evil, is an inducement to its augmentation. In this point of view we entirely disapprove of "persons deriving no profit" from the charge they have undertaken being exempted from the provisions of this Act. The sound policy of the following dictum of Dr. Conolly seems to us unquestionable. "Every insane person should in some way or other become the public care, and be looked after by responsible officers of the state, so as to ensure the hope of recovery and liberty to every curable case and proper protection to all." The exception opens a door to evasions, and the history of the insane proves to us there may be many conceivable cases in which justice may not be done the lunatic even by his own friends and relatives.

The new Act, likewise, enforces a particularization of the special facts which have led to the signature of the certificate consigning the individual to duress. A very proper provision, and one, we have reason to know, is not uncalled for, medical men having wilfully or carelessly signed certificates which they must have withheld, had a detail of their reasons been demanded of them. We have never, however, been enabled to perceive the necessity of two certificates being required. In the great majority of cases one practitioner alone has had the opportunity of observing the patient with sufficient attention to enable him to sign the document in an obscure case, and the second signature has then often been obtained as a mere matter of form, or after injurious delay. The difficulty of obtaining it will be now still greater; for the cases are numerous in which

a second practitioner, a stranger to the patient, may be unable to specify the circumstances of an aberration of intellect which may nevertheless exist.

In the third place, if the keepers of private asylums are to be submitted to more active inspection, (which indeed if they know their own true interests will be courted by them, as it will be found eminently instrumental in increasing the reputation of well-conducted establishments) so will they be protected against vexatious law proceedings for false imprisonment in the case of alleged lunatics—they having taken the precautions required by the Act.

Besides the Bill, so ably analyzed and commented upon by Dr. Winslow, Lord Ashley introduced another, having for its object the supplying additional accommodation, by compelling the various counties to provide asylums for their poor. A former Act conferred upon them the *power* of so doing without, however, insisting upon their putting it into force. How far they have done so may be judged from the following extract from Lord Ashley's Speech.

"I feel it necessary to call the attention of the House to the principal defects which are pointed out by our Reports as to Pauper Lunatics and County Asylums. 1st. That there are 40 counties in England, and only 16 county asylums, and 12 counties in Wales, and only one disgraceful borough asylum. Of the 24 counties having no asylums, one has 500; two upwards of 400; three upwards of 300; seven upwards of 200; and eleven nearly 100 lunatics each; and Wales has 1000 lunatics. The second defect is that of the 16 counties which have asylums, one has 800, one has 600, one has 500, one more than 300, three more than 200, and the rest more than 100 lunatics, for whom there is no accommodation in the asylums that have been erected, and no other receptacle. The third defect is, that all the existing asylums are full of incurables (of 984 patients in Hanwell, only 30 were reported as curable; and only 20 of 382 confined in the Surry Asylum,) or persons said to be incurable. The fourth defect is, that no system has been adopted in county asylums to give preference to urgent cases, or those capable of cure. The fifth defect is the detention of lunatics in workhouses, where there is no sufficient medical or moral treatment. The sixth is, there is no real visitation or true account of those lunatics who are not in asylums; for example, the lunatics of North and South Wales, and those in England not in asylums, being 9,339 with their friends or in workhouses."

Sir James Graham properly observed, after listening to these facts, and to the detail of some of the horrors resulting from their existence, that "the time, I think, has arrived when what was permissive shall be compulsory—when the counties throughout England and Wales shall be compelled by law to find sufficient means and accommodation for the care and custody of those unhappy persons." The creation of the new asylums has therefore been resolved upon, as also of other receptacles for incurable, or to use the more proper appellation employed by Lord Ashley, *chronic* cases. This last provision has been much disapproved of by many practitioners conversant with the management of the insane, and especially by Dr. Conolly, who believes by any such arrangement the interests and comforts of the old cases will be sacrificed. With all deference to so deservedly a high authority, we can scarcely believe this will be the case. Nothing can be worse than the present state of affairs. The asylums, expensively furnished with every curative appliance, are choked up with

cases in which these are of no avail, while hundreds and thousands awaiting their turn for admission, which had this been promptly accorded to them would have furnished a large proportion of cures, fall into a hopeless condition in consequence of the delay. They never have their fair chance of cure offered them, and in this way an immense amount of needless suffering and expense is engendered. We are glad to find the change meets with the approval of so experienced an observer as Dr. Thurnam, who also offers some useful suggestions respecting it.

"The proposed plan of erecting asylums for the *care* of the decidedly incureable and comparatively harmless in addition to hospitals for the *cure* and care of other classes of the insane, appears to me to be worthy of every encouragement. The most desirable plan would seem to be that of making such asylums appendages to the hospitals, and of placing their internal government in subordination to the directing physicians of the latter. An assistant physician or other medical officer should be appointed as the resident head. Such asylums should, when possible, be within a quarter or half a mile from the hospitals with which they are connected. Their construction will properly be more simple, the officers and servants less numerous, and their general economy altogether less costly than that of their sister establishments, the hospitals. The formation of such divided but inter-dependent establishments, as that of hospitals and asylums united with each other under a common external and united internal government, will whenever carried out, constitute an important era in the public provision for the insane poor of these kingdoms; and will, I believe, be found, not only more economical than our county asylums as at present conducted, but, by affording greater facilities for the admission of recent cases into hospitals, also to result in a larger proportion of recoveries and a diminished mortality." 81.

Dr. Conolly observes that, many of the incurables form most eligible attendants upon other patients; and that, if confined by themselves, they would be far less happy and become more and more neglected in proportion as their incurability became better established. The retention of a few such persons as aids to the servants might be advisable, although we cannot but think that in the treatment of curable patients their intercourse with persons of sane mind should be preferred wherever possible. In regard to the neglected condition of the incurable or chronic and epileptic insane we can see no reason whatever why it should take place, and it would imply a want of inspection, and an omission of appliances, certainly not contemplated by the originators of the change. Of course the arbitrary separation of cases into curable and incurable is out of the question; but the adaptation of different establishments to the requirements of various classes of patients seems to us feasible and desirable.

Allowing as we do that the changes in the law effected by Lord Ashley are very great improvements, and perhaps as extensive as the present state of opinion will admit, we may still state our belief that they come very far short of the requirements of the case to be provided for. We believe the system to be radically bad that admits of the care of the lunatic being made an object of private speculation and emolument. His condition is so peculiar that it fully warrants a departure from the ordinary mode of proceeding in this country, and *that he should become, whatever his condition in society, rich or poor, an object of the special and direct management of the State.* That humanity and economy alike demand this change

in respect to the *poor* is apparent from the apathy with which local bodies have allowed their wants to remain so long unprovided for, and the disgraceful jobbing which too often takes place when money has to be expended in their relief, so that the results are very inadequate to the outlay. In what other mode are uniformity of management and the rapid spread of improvement in it to be secured? Is it not notorious that the magistrates and governors in some counties most disgracefully neglect the duties of inspection, and in others, unwarrantably interfere with the details of treatment, and impose regulations which are quite inconsistent with the true welfare of the inmates of the establishments under their control? Surely the nature of the management of lunatics should not depend upon the chance of selection of a body of efficient persons from a class in which the most erroneous views concerning all medical matters so freely prevail. One general, comprehensive system is required.

But we believe State interference is also desirable in the case of the *rich lunatic*. His case is indeed a peculiar one: for, it is a lamentable fact that, when he has once lapsed into this unfortunate condition, his friends, too often regarding his calamity almost in the light of a stigma on the family, or even occasionally profiting by his mischance, do not always evince an over-weaning anxiety for his restoration to society. This may not be frequently the case, and may, and sometimes is, counteracted by the highmindedness of the principal of the establishment in which the lunatic may be located; but we hold that, where even exceptive instances exist, and where such temptations not to favour a cure may influence many meaner minds, the State should step in and rescue her unfortunate citizen from his predicament. Moreover, with every disposition to do justice to their inmates the proprietors of many, very many, asylums, especially in large towns, are not possessed of space and appliances adequate to the efficient treatment of the insane—their safe custody being too often all that is accomplished. It may be said that a vigilant inspection will counteract the evils now adverted to, and so it will some of them: but the difficulty of organizing a truly effective system of inspection is great beyond conception, if not insuperable. The visitors can only come occasionally, and although they may check flagrant delinquencies, none but residents within the very walls of the asylum itself can ensure the due enforcement of the best contrived modes of management. We believe, then, that asylums for both rich and poor lunatics should be under the immediate direction of Government, and *managed by well-paid, responsible, resident medical officers*, who could have no interest whatever but in the speedy recovery or the melioration of the condition of those committed to their care. Moreover, there is a class of persons between the two extremes we have mentioned whose means are limited, yet position in society respectable, and for whom the occurrence of insanity now, owing to the high charges at private asylums, is a sentence of degradation to pauperism, with all its accompanying obstacles to cure. Such persons might be provided for in some such establishments as we have been contemplating, with an attention to their prior habits and present comforts, at charges which could not prove remunerative to private speculation.

So frequent have the instances of sane persons unjustly confined in asylums been brought before the public, that a main object of legislation

has been of late years, the prevention of this enormous abuse. In effecting this, however, it seems to have run into the opposite extreme, and imposed a *veto* upon the admission of persons who would be benefited by residence within the walls of an asylum. We have now a patient exhibiting every symptom of threatened insanity whose condition we have no doubt would be rendered far more favourable by temporary seclusion, and yet, under the present state of the law, no asylum could admit him. We do not doubt that many cases might be checked in the bud by a prompt resort to appropriate measures, among which, separation from accustomed associations is one of the most important. Vigilant inspection, or above all the system of asylum-management above adverted to, would prevent the possibility of such persons being improperly detained, if even inadvertently admitted. Dr. Costello's pamphlet contains some useful observations on this head.

"As the law stands at present, retirement into an asylum is interdicted to those who would repair to them voluntarily, nay cheerfully, and who, it must be acknowledged, could not take any step, under such critical circumstances, better calculated to restore mental health and peace. But a nervous person cannot be admitted to share the advantages best suited to his condition, without the certificates of two medical men that he is of unsound mind. The law has no sympathy for mere apprehensions: it turns a deaf ear to the *egrotare timeus:* he must allow himself to get worse, before the law will allow him to resort to the best means of getting better; then, and not till then does it relent, and show pity to the victim of its delay. * * * * * The class of persons now referred to, having a full knowledge of what they do, ought to be enabled to resort to asylums of *their own free will and accord*, to avail themselves of the advantages so suited to their infirmities. They require modified isolation, without separation from the world, and where can these conditions be better united, than in a well-conducted asylum, adapted, as it should be, to the treatment of disorders of the mind, even in their mildest forms? If a sufficient security against abuse were provided, and this could be easily accomplished, there could be no valid reason for refusing those benefits to nervous patients as such. But those benefits, important as they are, would not be the sole ones resulting from a change of the law. The very character of the asylum would be changed. From a prison, which it is now so universally regarded, it would become an hospital, and those prejudices which now operate so extensively against persons attacked with insanity would disappear."

These, and other remarks to the same purport, seem to us dictated by sound sense. Every one is aware how important, in a curative point of view, it is that the treatment of a case of insanity should be commenced as early as possible: and yet, until some overt act has been committed, perhaps months after the premonitory symptoms, or sufficient derangement has manifested itself to convince two medical observers of its existence, treatment does not commence.

STATISTICS OF INSANITY.

The dangers of the indiscriminate use of the Numerical System are nowhere brought to light stronger than in the Statistics of Insanity. The accumulation of large masses of undigested figures has given rise to conclusions which a scrutinizing examination of some of their particulars will by no means warrant.

Dr. Thurnam has exhibited more than one generally prevalent error derivable from such hasty generalization. The first chapter of his work is entitled, "*On the Methods of deducing and exhibiting the Results of Treatment; and on the Circumstances in the Character of the Cases admitted capable of influencing these Results.*" He points out the uncertainty and errors which must result from a want of uniformity in the employment by different observers of such terms as "recovered," "improved," "cured;" and states that, in some establishments, as the York Asylum, prior to 1814, the returns have been purposely falsified. For want of attendance to these and various other particulars, to be afterwards noted, very erroneous and unjust conclusions have been frequently drawn as regards the comparative merits of different establishments.

1. *Calculating the Proportion of Recoveries and Mortality.*—For this, different plans have been adopted. Dr. Thurnam calculates the per-centage of *recoveries* upon the *admissions*, while Mr. Farr calculates it upon the *discharges*, stating that, "if the mortality continued the same, the probability is, that the patients to *be discharged* would, *cæteris paribus*, be discharged cured, relieved, and dead, in the same proportions to those already discharged." To this strange statement Dr. T. demurs, seeing that the recoveries of the discharges are rarely less than 50 per cent., while of the cases remaining not more than from 10 to 20 per cent. can be considered curable. The "remaining" cases in large hospitals, which have been some years in operation, may be considered as representing the incurables, and as furnishing their principal annual mortality. A table, shewing the per-centage of recoveries at several asylums, proves that the per-centages calculated on the admissions do not bear any definite proportions to those calculated on the discharges.

"It is sufficient that we should be aware that the true per-centage of recoveries is one which, in different institutions, in different degrees, is intermediate to that calculated on the admissions and that calculated on the discharges. While, therefore, I freely admit that the plan of calculating them on the admissions fails in exhibiting with precise accuracy the results of treatment in any hospital for the insane, I yet believe that it affords a much nearer approximation to the truth, when considerable periods are concerned, than that calculated on the discharges; and that therefore it is very decidedly to be preferred to the latter method."

Another table is given, shewing that, in the asylums at Lancaster, Glasgow, and Hanwell, and the Retreat, the proportion of recoveries calculated on the admissions is least in the infancy of the respective institutions, and increases with their age, the very reverse being observed when the calculation is made on the discharges. After the Lancaster Asylum had been in operation five years, the proportion on the *admissions* was 28·8, and on the *discharges* 52·7; but at the end of 26 years the proportion on the *admissions* was 40·3, and on the *discharges* 48·1. At the Retreat, at the end of five years, it was 26·1 on the admissions, and 62·1 on the discharges; while at the end of 45 years these figures were 47·8 and 55·6.

Mr. Farr has exposed the fallacy of calculating the *mortality* upon the *admissions*, as practised by Esquirol, Burrows, and most other authors. The proper method consists in taking the mean annual per-centage of

deaths calculated on the *average number resident;* but unfortunately few asylum reports furnishing aggregate results state their mean numbers, resident or average population. A table is given, in which the mortality is calculated in both these modes. From this we find that the *mean annual mortality per cent. of residents* was 10·75 at Hanwell; 7·45 at Nottingham; 10·5 at Bethlem; 4·78 at the Retreat; 13·67 at Lincoln; and 15·54 in the Metropolitan Licensed Asylums; but that, when this was calculated upon the *admissions*, the per-centages of these respective institutions were as follow: 34·7; 12·2; 5; 23·1; 18·5; and 27·9! No safe conclusion can be come to from *observations conducted over a brief period only;* and although, under any circumstances, "the undiscriminating comparison of aggregate results is nearly always very fallacious, yet it is particularly so when these apply to short periods, and especially when such periods are the *first* in the history of the institutions to which they refer."

"From a particular investigation of the statistics of a large number of asylums, both in our own and other countries, I find that the *proportion of recoveries*, in nearly every instance, has gone on materially increasing for a considerable period, often amounting to 30 or even 40 years, after their first establishment. The reason of this is evidently found, as has already been hinted, partly in the large proportion of old cases often admitted upon the first opening of the institution; and partly, though in a less degree, in the circumstance of the recovery, in a certain number of cases, requiring a rather considerable period for its completion. On the other hand, the *mortality* is generally more favorable during the early history of an asylum; and during the first 20 or even 30 years of its operations, as the proportion of recent cases admitted increases, and as the old cases die off, it usually continues to undergo a material increase, which often amounts to 50 or 100 per cent. upon the mortality of the first five years. From the following tables we may, I think, conclude that a period of from 20 to 30, or in the case of a small institution, a still greater number of years must elapse before we are authorized in concluding that the experience of a hospital for the insane at all fairly represents the average results of treatment which either have been, or will be, obtained in it." P. 19.

2. *Modifying Circumstances of the Cases themselves.*—Besides the sources of error derivable from faulty methods, others may depend upon the previous circumstances and character of the cases admitted not having been taken into account. Some of these Dr. Thurnam passes in review.

(a.) *Sex.*—As the result of investigating the returns of numerous asylums, both in this and other countries, Dr. T. found, in all but two, the *proportion of recoveries was greater in women than men.* At Glasgow it was so 4 per cent., at Lancaster 7, at Worcester, U.S. 19, at the Retreat 20, at Charenton 23, and at York Asylum 28 per cent. At Hanwell, however, the male recoveries exceeded the female by 5 per cent., and at Bloomingdale, U. S. (where patients with delirium tremens are admitted) by 28 per cent. So, too, the *mortality of female lunatics* is much less than that of male. In the general mortality of this country the *excess on the side of the male* does not exceed 5 or 6 per cent.; but in the York Asylum it is 93, and at St. Luke's 96 per cent., or very nearly double. It is 72 at Hanwell, 57 at Glasgow, and 34 at the Retreat. At the Belfast Asylum alone has the mortality of women exceeded (12 per cent.) that of men.

Hence, in estimating the results of treatment at different institutions, we must always remember that these will be less favourable in proportion as the male admissions preponderate.

In connexion with this part of the subject, we may here advert to Dr. Thurnam's Essay (re-printed in another part of this volume) upon "The *Relative Liability of the Two Sexes to Insanity*." He believes the prevailing opinion, that females are most liable, to be founded upon erroneous data. Esquirol having collected tables from all countries, and finding by them that there were 37 male to 38 female lunatics, hastily concluded that the disease therefore preponderated in the latter. According to the Census of 1841, however, the excess of females of all ages over males is 4 per cent., and about 8 per cent. above 15 or 20 years of age. The excess is as high as 12 per cent. from 20 to 30, and is 6 per cent. from 30 to 40, and 4 per cent. from 40 to 50. Thus, supposing the liability to insanity only *equal*, we ought to have a preponderance of females, especially between the ages of 20 and 50, within which period insanity chiefly occurs for the first time. The only institutions in which a material preponderance of female admissions for considerable periods occurs are St. Luke's and Bethlem, amounting to 20, 30, or even 45 per cent. This may, in some degree, perhaps, be accounted for by peculiarities of the metropolitan population; for while, in the kingdom at large, the excess of females between 20 and 50 is but 8 per cent., it is 18 in the metropolis. The preponderance, however, does not extend to the upper and middle classes, for the male admissions in the licensed private asylums exceeded the female by 38 per cent.

Esquirol has moreover fallen into the fallacy of comparing the *existing* instead of the *occurring* cases, but the mortality of male (taking the asylums generally) exceeds that of female lunatics by 50 per cent.; so that existing cases accumulate much faster in women than in men, and therefore afford no data for judging of the comparative frequency of the disease. Owing, too, to the greater number of relapses which take place in women, the number of admissions is raised higher than the true proportion.

Table No. 15, shews the sex of 67,875 lunatics admitted into various British asylums to 1844. Of these, 47,183 were paupers—24,372 males, 22,811 females: 19,314 were in private asylums—10,980 males, 8334 females; and 1379 were mixed—furnishing 692 males, and 687 females.

"It is indeed highly probable that different countries, and perhaps even the same country at different periods, as well as different communities, and different ranks and classes, in the same country, may vary very much as regards the proportion in which men suffer from insanity more than women. Thus, a comparison of the proportion of males and females of the two classes of pauper and private patients admitted into different classes of asylums, leads to the conclusion that, in this country, a larger proportional number of men become insane in the higher than in the lower ranks of society. This may, however, admit of explanation, if, as is not improbable, women in the humbler ranks of life, who are the subjects of insanity, become sooner pauperized than men under the same circumstances. It also appears tolerably well ascertained that a larger proportion of women, relatively to the other sex, become insane in France than in England; though, as we have seen, this is less certain as regards the metropolis when compared with the rest of this country. In this respect we have seen that

the statistics of our own metropolis appear to resemble France rather than those of the rest of England." P. 153.

The experience of the Retreat would at first sight seem to militate against the author's deduction, inasmuch as the number of women admitted has exceeded that of the men by 18 per cent., *i. e.*, 45 men have been admitted to 55 women. But, among the Quakers, the females of all ages exceed the males by 20 per cent: and above 15 years of age the excess reaches 30 or 35 per cent. So that, supposing the experience of the Retreat to represent the proportions among the Quakers at large, there are still from 10 to 14 per cent. more males than females attacked. However, in the various points in which females have the advantage over males they have it less so among the Quakers. "This is worthy of notice, as it is probably due to the greater general regularity of life in the men of this community as compared with that of men in the community at large; or, at least, than in those parts of it which furnish inmates to the asylums compared."

(b.) *Age.*—The statistics of lunatic asylums shew that the *probability of recovery* is greatest in the young, and decreases regularly as age advances; but that the *mortality* increases much more rapidly with age than it does in the general population. Dr. Thurnam has an interesting paper upon the "*Liability to Insanity at different Ages.*" He criticizes the conclusion Esquirol and others have arrived at from the examination of numerous tables of *existing* cases, that individuals become more obnoxious to insanity the longer they live after maturity. By examining the ages of 12,575 admissions into British Asylums, it will be found the greatest number have occurred between the ages of 30 and 40; but the period at which a patient is first brought to an asylum is a very uncertain indication of that at which the attack of insanity first came on. By comparing the ages of *occurring* cases at Hanwell (1840-4) with the numbers living at corresponding ages in England and Wales, Dr. Thurnam arrives at the conclusion that the period of life most liable to insanity is from 20 to 50 or 60. From 30 to 40 the liability is usually greatest, and it decreases in each succeeding decimal period. So far from the liability being greatest in old age, it is nearly twice as great from 30 to 40 as from 50 to 60, and much more than twice as great than at any age subsequent to 60. At the Retreat the ages between 20 and 30 are those most liable.

(c.) *Rank, prior habits, &c.*—The prior condition of the patient in this, as in other diseases, much influences the probability of recovery and amount of mortality. The Retreat is an establishment for persons of a select and favourable class. The recoveries there have been at the rate of about 50 per cent. and the mortality is but 4·7 per cent., while at the Wakefield asylum the recoveries are 43·6 and the mortality 15·7. At Lancaster the recoveries are but 39·8. The mortality of the Metropolitan Licensed Pauper Asylums is 20·68 per cent., while in those for private patients it is but 10·94. In all these cases other circumstances may doubtless exist for the explanation of the discrepancy; but it is obvious that, in comparing the results derived from one asylum with those obtained at another, we must especially bear in mind the condition of society of their respective populations.

(d.) *Causes of Disease.*—Dr. Thurnam observes that too great careless-

ness has occurred in many institutions in registering assigned causes, and in drawing inferences from numerical results thus obtained. For our parts we see almost insuperable difficulties in obtaining accurate details on this head, and quite agree with the following opinion of Dr. Conolly: "With all our care, a true account of the causes and commencement of the malady is so difficult to be obtained, that I look with much incredulity on all statistical returns that have been made in this or any other asylum as relates to these particulars."

(e.) *Imperfect Classification and Specification of the Forms of the Disorder* have also led to erroneous conclusions.

(f.) *Duration of the Disorder.*—That the *recoveries* are frequent in proportion as the cases are *recent* is well known, and of vital importance it is that it should be still better known. Dr. Thurnam divides the consideration of this section of his subject into two parts, the *duration of the disorder on admission*, and the duration of the *treatment or residence*. At the Retreat, the probability of Recovery in cases admitted within the first 3 months, is 4 to 1, and when uncomplicated with serious bodily disorder 9 to 1: while in cases not admitted until after 12 months, it is less than 1 to 4. When the cases are separated into only two categories, viz., those in which the disease is of less, and those in which it is of greater duration than 12 months, the difference in the amount of recoveries is very striking. Thus, we find in the first of these classes they were 49·26 upon the admissions at Maidstone; 50·95 at Lincoln, and 61·87 at the Retreat; while, for the other class they were but 4·84, 9·62, and 18·88. The mortality is, however, greater in recent cases, for at the Retreat its mean annual amount during 48 years has been 7·3 per cent. in cases in which the disease has existed but three months, and but 4·57 when it has existed more than 12 months. In comparing the experience of different institutions, we must not neglect to take into account the *duration of residence or treatment*, which must materially influence the amount of recoveries and deaths. Thus, in some, discharges are compulsory within a twelvemonth, and, in others, the expense leads to the withdrawal of patients. At the Retreat, cases are allowed to remain an unlimited period, nor are they hurried away too early in convalescence: and its large proportion of cures and low degree of mortality may be in some degree due to this prolonged residence. One-third of the entire recoveries occurred after the first year, and nearly one-sixth after the second.

Dr. Thurnam concludes this part of his subject with some directions for collecting and registering the Statistics of the Insane, which are well worthy of notice; and indeed all who contemplate the contribution of additional facts concerning the various conditions of the insane will do well to peruse the observations of this cautious and able writer.

MANAGEMENT OF THE INSANE.

Dr. Thurnam passes in review the various particulars of the treatment of the insane capable of influencing the statistical results, and cites the experience of the York Asylum prior to its reform, in proof of the extent of the operation of these. He also presents tabular views of the results

of treatment in several of the principal British and foreign asylums. Into these details we cannot follow him; but may refer to a few observations, and collate them with remarks on the same topics contained in some of the various Asylum Reports* before us.

1. *Exercises, Occupations, and Amusements.*—The important agency of these has now been long established, and most of the recent improvements have consisted in carrying them still further into operation than heretofore. Open air exercise is best of all, and the Irish district asylums are now usually furnished with farms of 20 acres and upwards. The miserable inadequacy in this respect of several of the establishments in the metropolis and other large towns is to be much deplored. Dr. Thurnam states that the amount of labour he saw performed in many Scotch and some Irish asylums was surprising; and there may obviously be an extreme sometimes in his direction. There is more difficulty in finding employment for the men of the more opulent classes, and at the Retreat there is always a larger proportion of females than males engaged. The officers of the Lancaster asylum speak of the great advantages which have attended the increase of the sphere of occupation during the last few years. Of 325 inmates 195 there followed husbandry or some trade occasionally, and above 100 might be seen actively employed daily. In this establishment too, much attention is also paid to amusements, such as country excursions, evening parties, games, keeping animals and plants, engravings and books, &c. Dr. Brigham has seen immense advantage derived from a fancy fair, and the consequent erection of a green-house. He encourages music and dancing, but not amusements which require the mixing of the sexes.

2. *Government and Attendants.*—Dr. Thurnam, as indeed nearly every one who has written on the subject, insists upon the great superiority of a *resident* to a visiting physician: and if there is one point more important than another in the management of an asylum it is the entrusting to this resident officer the supreme and undivided control over the whole of the inmates, sane and insane. Even matrons are frequently mischievous officers from their officious intermeddling. The proportion of attendants to patients is very various in different establishments. Thus, at Wakefield it is but one to 22, at Hanwell to 18. In the Irish district asylums it is 1 to 13, or with assistants 1 to 9. At Lincoln it is 1 to 9. In the Scotch asylums 1 to 10. At Sieburg, according to Jacobi, it is 1 to 7 or 8; and at the Retreat it is 1 to 6 or 7. In establishments for persons in the easy classes of society the proportion should not be less than this last; but in the pauper establishments where the patients often assist each other, it need not be more than 1 to 12 or 15. Dr. Brigham states that the proportion in the State Asylum of New York, is about 1 to 10. Both he and the officers of the Lancaster Asylum lay great stress deservedly upon the

* We shall feel obliged by our friends in various parts continuing to forward us these documents. Even when not requiring special comment they are often of great use in aiding the formation of correct opinions.

employment of efficient and respectable persons; and the latter have endeavoured to instruct the attendants in the nature of the duties required of them, and have considerately so arranged that they may have opportunities of obtaining sufficient relaxation. There cannot be a doubt that the nurturing a respectable body of officials of this kind is of the last importance. Their non-existence has thrown, and will yet throw, obstacles in the way of rapid improvement. Their education, comfort, and due relaxation are well-earned by their arduous and important duties.

3. *Clothing.*—As a general rule, the insane require warmer clothing than the healthy, especially as regards their feet, the circulation being often feeble, and the functions of the skin imperfect. We must not wait for their expressing their sensations, which they often will not or cannot do, or these may not exist. The suffering from cold was one of the worst enormities of by-gone management, and Dr. T. states that, even now, he occasionally witnesses patients in large asylums without shoes and stockings, or a sufficiency of other garments. "It is very desirable," says Dr. Brigham in his Report, "that extra and better garments should be sent with those accustomed to them, that when they become better, and when they walk or ride out, attend religious worship, &c., their self-respect may be preserved. This is important, and should not be neglected."

4. *Diet.*—Dr. Thurnam treats upon this at considerable length, but we have space for but a few remarks. He justly observes that a liberal, nutritious, but simple diet, is that upon which patients thrive, and most recoveries occur. He thinks the diet should approximate to that habitually employed by the middle classes, thus raising that of the pauper, and rendering that of the wealthy man somewhat plainer. He believes that the dietaries of some of the pauper asylums contain too little animal food. He gives a diet table of seven asylums in which great discrepancies exist: and states that, in three of those in which the better diet prevailed, the recoveries were 43·7 per cent., and the mortality 9·35: while in the others, where a poorer dict existed, the recoveries were only 36·75 per cent., but the mortality was 14·54. Other causes may have also contributed to produce this result. Dr. Brigham employs only the best food, which is served up in crockery (at Lancaster the patients, too, derived great satisfaction for the substitution of this for wooden trenchers,) and furnished to the inmates as if they were boarders and not patients. As a general rule he does not limit the amount eaten. "The insane require full as much food as the sane, and we think rather more; many of them have been reduced by sickness or by their real or imaginary troubles, before they came under our care, and when they begin to recover eat very heartily. The total increase in weight of the 132 discharged last year was 1565 pounds."

5. *Pharmaceutical Treatment.*—Dr. Thurnam observes that, although he agrees with those who proscribe lowering treatment as a general rule, and approve of a supporting regimen, he is far from discountenancing moderate local depletion and other antiphlogistic means in appropriate cases. The overlooking bodily disorder in insanity, and the treating the

disease by mere routine of bleeding, purgatives, or emetics, are both erroneous extremes.

6. *Restraint.*—The practicability of abolishing all personal restraints has been finally established by Dr. Conolly's grand experiment at Hanwell, so lucidly and minutely described in his Lectures. How Dr. Stewart, of the Belfast Asylum, can state, "that total abolition of restraint appears to be only one of the many vulgar delusions and speciously popular *ad captandums* of the day," we cannot imagine. From the epoch of the institution of the courageous experiment may be dated a vast progress in the improved condition of the insane; for if its complete imitation could not be always realized, it at least exhibited to all the shallow pretexts upon which excessive restraints rested, and put their much longer continuance out of the question. The consequence now is, that, in every well-conducted asylum, cases requiring restraint of any kind have become wonderfully few, shewing how much groundless fears and harsh treatment had multiplied them heretofore. Taking Dr. Conolly's explanations, however, as we find them in his Lectures, in which he complains that his mode of employing seclusion has been misrepresented, we must still retain the opinion formerly expressed, that the restraining the few patients who yet require it by means of slight bands, as practised by Drs. Brigham, Thurnam, and others, is preferable to seizure by numerous attendants and seclusion. Of course the most vigilant care is supposed to be taken that the restraint shall be discontinued as soon as possible, and that it shall never be resorted to, save by the sanction of the physician. Happily, however, cases requiring either restraint or seclusion are becoming fewer and fewer; and it is cheering to find, from the report of the officers of the Lancaster Asylum, that the accommodations of that edifice have been increased, at even the expense of its apparent security, with the best effects.

"As illustrative of the effects produced on the insane by the removal of obstacles to freedom, or such as suggest a feeling of confinement, it may be remarked, that the attempts to escape from the wards were diminished after the facilities of escape were greatly increased. In 1826 an addition was made to the building, called the criminal wing, constructed in every way so as to offer most formidable obstacles to escape, by means of strong dark cells and iron gates. It is somewhat remarkable, that the only unrecovered patients who have escaped from the building since its establishment, have made their exit from this portion especially adapted for their security."

7. *Religion.*—Dr. Thurnam, in common with most other principals of asylums, testifies to the great benefit which accrues to many patients from participation in religious worship; and he offers some very suitable advice to ministers upon the mode of performing this delicate duty, and the classes of subjects best suited for commenting upon to the insane. It is obvious here that every thing depends upon discretion, and that a rash, over-zealous man may do immense mischief; and, indeed, there can be no safety in the employment of any such agency, unless it is implicitly submitted to the control and direction of the physician. In an article upon this subject in the "American Journal of Insanity," Jacobi's advice, that the chaplain should render himself familiar with writings on insanity, is

reiterated, and a knowledge of anatomy and physiology recommended; but we doubt the soundness of the recommendation, as a smattering of this sort is more likely to beget a self-sufficiency and perhaps contrariety of opinion, adverse to an entire submission to the views and directions of the medical officer. The mischief which an injudicious or fanatical chaplain may do may be in some degree judged of by the following passage from a letter written by the chaplain of the Virginia Asylum, U. S., and quoted (with disapprobation) in the journal just referred to. "If the insane can be made to comprehend religious instruction, why may they not become interested in their spiritual condition? And if they should become as deeply interested in their spiritual welfare, as we have known some of the entirely sane to do through the instrumentality of religious instruction, why may not *this religious concern operate on the principle of revulsion in effecting their cure?*" Dr. Thurnam observes that the services should not exceed one hour; but we think half that time, exclusive of the singing, should suffice.

8. *Schools.*—It is a disgrace to our English Asylums that these have not been established within their walls. Their success in Paris has been recently and graphically described by Dr. Conolly, and Dr. Brigham thus speaks of those established in the New York Asylum:—

"Our confidence in their utility, and even their necessity for the improvement of many of the insane, has increased. We find the patients who have recovered there, look back to their attendance in school as the greatest enjoyment they had, and often allude to it, and to the advantages they derived from it, in their letters. A manual or book of instruction, containing short precepts and maxims for the guidance of the insane, respecting the preservation of health—the government of the passions and control of the feelings, portions of which they would commit to memory, we believe would do much good, and we hope to be able to prepare such in the course of another year. Those who have been once insane are so liable to a recurrence of insanity, that we feel as if we had not done a patient all the good we ought by curing him of one attack, but that we should endeavour so to instruct him, that he may prevent another. We have great confidence, in many instances, of *man's power over himself to prevent and control insanity.* But to accomplish this many need to be enlightened."

The present article has run over a larger space than we intended it to have occupied; but we trust that some of the topics adverted to in it will not prove devoid of interest at a period when we are upon the eve of a great change, and we believe improvement, in the provisions for lunatics in this country. We are not sufficiently acquainted with those intended for Ireland and Scotland to be enabled to offer an opinion concerning them. The existing district asylums of the former country are described by Dr. Thurnam and others as being well-conducted establishments, so that their multiplication is very desirable. In Scotland much, very much, remains to be done to place the condition of lunatics, and indeed paupers in general, upon a satisfactory footing. Dr. Conolly has recently exhibited the general excellent management of the large, indeed far too large, asylums of Paris; and the accounts we receive of those of the United States*

* We are not aware of the exact present provision for the insane in America.

are even more satisfactory. Indeed, it is cheering to find the cause of humanity progressing on every side. Enlightened observers are now engaged in all parts of Europe and America in contriving meliorations, superintending their execution, and recording the results; so that, ere long, vast bodies of valuable and authentic information will have become accumulated. The medical directors of public insane establishments seem to be free from the jealousies and rivalries which sometimes impede the usefulness of our profession, an honourable emulation and courteous co-operation stimulating and augmenting their power of benefiting those committed to their charge. With the increased numbers of these officers consequent upon the erection of the new asylums, it is very desirable that some plan for the uniform preparation and exchange of Reports and for occasional meetings should be organized. An Association has been formed for such purposes in the United States, and we believe upon a small scale among ourselves. In the former country, too, a cheap quarterly periodical, "The Journal of Insanity," has been established by Dr. Brigham, of which, judging from the few numbers which have irregularly reached us, we can report favourably. How far it would be expedient to establish some such a medium of intercommunication here is perhaps doubtful, when the abundant facilities offered by existing periodicals are considered.

DER MECHANISMUS DER RESPIRATION UND CIRCULATION, ODER DAS EXPLICIRTE WESEN DER LUNGENHYPERÄMIEN. EINE PHYSIOLOGISCH-PATHOLOGISCHE UNTERSUCHUNG. Von Dr. *A. Mendelssohn.* Berlin, 1845.

The Mechanism of Respiration and Circulation, or an Explanation of the Nature of Congestion of the Lungs. A Physiological and Pathological Inquiry. By Dr. *A. Mendelssohn.*

THE author, in the physiological division of his work, has two objects principally in view; in the first place, to determine the cause of the well-known obstruction occurring in the pulmonary circulation in consequence of any impediment to the free admission of air into the lungs; and, secondly, to show that a particular influence is exerted by the normal expansion of the lungs, on the movement of the blood within the pulmonary artery and its capillaries. It does not appear that Dr. Mendelssohn is acquainted with the numerous researches which have been made in this country in connection with the first part of his inquiry; and the consequence is that, on the most important points of which he treats, the author has been anticipated, especially by the extended and valuable experimental inquiry of Dr. Reid.

In 1843, however, Dr. Pliny Earle stated, in the American Journal of Medical Science, that there were 20 public asylums, providing 2647 beds, and possessing 671 acres of ground.

There are no nerves to which so much attention has been directed as those of the par vagum, not only on account of their intrinsic importance, but more especially because physiologists have been anxious to avail themselves of the unwonted facilities these cords present for experimental investigation, to determine the influence which is exerted by the nervous system upon some of the most recondite operations of animal bodies—digestion, secretion, circulation, and the production of animal heat.

Although the erroneous theories which owed their origin mainly to the well-known experiments of Dr. W. Philip and Sir B. Brodie, have been gradually replaced by more accurate views, we are still desirous of offering a few remarks upon the functions of organic life and their relations to the nervous system, as upon these subjects many misconceptions still prevail.

One of the most valuable results which has in late years sprung from the comprehensive study of the organic laws, is the conviction that the various processes by which nourishment is accomplished in the animal, are essentially dependent on the same principles and are submitted to the same laws, as those which govern the phenomena of nutrition in the plant. The respiratory action, for instance, is really the same, whether it be accomplished by the leaf of a vegetable or by the lung of an animal; in both cases the immediate object is the introduction of oxygen into the system, and the consequent formation of carbonic acid; the ultimate end being the development of caloric. True it is the conditions for realizing these results are very different in the two kingdoms; for, whereas, in the case of the plant, all that is required is the contact of the leaf with the atmosphere, and the circulation of the sap; in the case of the animal, a complicated series of actions is required for bringing about the contact of the blood with the atmospheric air. Now, if we inquire what share the nervous system has in the respiratory function, it would not be difficult to prove that it is merely concerned with these incidental actions—the movements, namely, of the chest and the circulation of the blood; for as to the ultimate actions, the introduction of oxygen, the evolution of carbonic acid, and the development of caloric, they are phenomena as strictly chemical in their nature as when they occur in the vegetable. Considerations of this kind will enable the physiologist to assign to the nervus vagus its real share in the breathing process, and thus to avoid the error of supposing that it has some obscure and unknown power of generating animal heat. A similar train of reasoning might be applied to the secerning function, which, as it goes on most actively in vegetables where there are no nerves, as it is effected in animals by the same mechanism as in plants, namely, cells, and as moreover, it continues after the nerves have been divided, we must presume is essentially independent of the nervous centres. But, as in the instance of respiration, it is very possible to conceive, that although the act of secretion is thus independent, the conditions necessary to bring it about, the circulation of the blood for example, may require the interposition of nervous agency; and moreover, which has not been sufficiently attended to in this inquiry, the discharge of fluids may, in many cases, as it certainly does in some, demand the influence of the nerves.

The experimental part of the work of Dr. Mendelssohn does not present, for the reason above stated, much novelty. He found, as Dr. Reid

had already determined, that the division of *one* vagus nerve, produced no injurious effect upon the lung on the same side, nor upon the general health of the animal; in five experiments of this kind "the lungs were perfectly healthy," (p. 24.) The effects of dividing the nerves on both sides were the same as those ascertained by preceding experimentalists. The fourth and fifth series of experiments are interesting, as showing that paralysis of the glottis, induced by dividing the recurrent laryngeal nerves, produces the same fatal effects in the lungs as if the two vagi nerves were themselves cut through; vascular congestion, increased density, so that pieces of the lungs sank in water, effusion of a reddish or yellowish matter containing nucleated cells, corpuscles, and in some parts the red particles of the blood, were the results. The number of respirations was diminished, and there was a rustling or grating sound; out of eight rabbits operated on, five died in from two to five days, but two recovered. (P. 29-36.)

Although the author has thus shown that some of the effects of dividing the recurrent nerves are similar to those attending the section of the par vagum, the vast difference and superiority of these latter nerves, as the excitors of the respiratory movements, a distinction apparently overlooked in the work before us, must never escape the notice of the physiologist in researches of this nature.

Some other experiments are related, but as the general views of the author may be gathered from the subjoined extracts, in which his principal conclusions are summed up, it is not necessary to detail them.

"The paralysis of the branches of the nervus vagus which are distributed to the lungs, is not the cause of the affection which develops itself in the lungs after the division of both nervi vagi in the neck.

"The excision of both nervi recurrentes causes the same affection in the lungs, though somewhat later, as the excision of the nervi vagi; from which it must be inferred that the paralysis of these two nerves, which ensues upon the excision of the two nervi vagi in the neck, is an essential cause of the above-mentioned affection.

"The conclusion of Longet, in his physiological resumé on the functions of the nervi vagi and recurrentes, that 'animals deprived of their recurrent nerves respire more quickly than in the normal state,' must rest upon false or misinterpreted facts, since my researches constantly prove the contrary.

"The preceding experiments and their results prove that, the different functions which have been attributed to the branches of the nervi vagus going to the lungs, are erroneous; they also shew that nothing more can be at present affirmed than that these branches bestow the sensation possessed by the mucous membrane of the bronchi, and that they contain motorial fibres for the bronchi.

"The phenomena resulting from the division of the nervi vagi or recurrentes, consist of a retardation of the circulation, a dilatation of the capillary vessels, an exudation of plasma into the tissue of the lungs and into the air-cells, and of the usual metamorphoses of this exudation: they are therefore in all respects analogous to the process, which, in pathology, is termed pneumonia.

"The stasis of the blood in the capillary vessels of the lungs, which is the cause of the alterations above described, is, as such, not yet known, and all that is seen is the alteration produced by the loss of the influence of the vagus upon the lungs and their function, whilst the cause of the change is not determined. I have, however, seen the same alteration that is caused by the excision of the nervi recurrentes, produced by opening the cavity of the pleura, by injecting a thin fluid into the bronchi, or by introducing a hard body into one of the bronchial tubes. If, out of all these researches, we select that which is common,

the law will appear—that the hindrance to the entrance of the air during inspiration, or more accurately, to the expansion of the air-cells, determines as a consequence a stagnation in the capillary vessels of the lungs.

"The preceding facts justify the presumption that the influence exerted by respiration upon the systemic circulation, which Poiseuille has determined, may be much farther extended than has heretofore been supposed; that, in fact, this influence, in virtue of a power which is generated in the lesser circulation by the expansion and contraction of the lungs, is communicated to the vessels running to and within the lungs; this presumption attains to certainty by experiments.

"The afflux or increased pressure of the venous blood towards the chest, and the diminished pressure of the arterial, is not determined, as hitherto supposed, by the rarefaction of the air within the thorax and the corresponding augmented atmospheric pressure, but is the consequence of the suction (*aspiration*) exerted on the blood by the elongation of the pulmonary artery, together with its branches and the capillary vessels of the air-cells.

"The suction, which is exercised by means of the elongation of the pulmonary vessels upon the blood contained in the veins, constitutes also the mechanism by which the processes of digestion and nutrition are connected with respiration, inasmuch as the chyle and the lymph are, by means of the ductus thoracicus opening into the subclavian vein, brought under the influence of this latter process: the materials of nutrition are, as it were, pumped into the blood by means of the expansion of the lungs.

"If from the starting-point which has now been gained, the results of these experiments be once more regarded, it is easy to show why, after the section of the nervi vagi, of the nervi recurrentes, after the introduction of a hard body into the bronchus, after an opening into the pleural sac, a stagnation must arise in the lesser circulation, and as a consequence in the greater circulation also; in other words, how in these instances pneumonia is caused, whilst after a narrowing of the air-tube, or after ligaturing the abdomen, only a stasis is determined in the greater circulation, whilst the lungs remain free.

"The influence above described, which the expansion and contraction of the lungs exert upon the greater circulation, is the cause of the closing of the fœtal passages, of the alteration of the circulation, and of the hypertrophy of the left heart developed in the first week of life."

These experiments and their results are so far useful as confirming the investigations of other physiologists, and especially because they show that the true cause of the changes induced by the division of the vagus nerves, is to be sought in the diminished quantity of air admitted in a given time into the lungs: thus, it matters not whether this diminution depends on the decreased number of respirations, as occurs when both vagi are cut; or on the paralysis of the glottis which mechanically impedes the entrance of the air, and which is induced, especially in young animals, and in all animals where the glottis is proportionally small, by the section indifferently of the vagi or recurrents; or on the obstruction produced by introducing solid bodies or liquids into the windpipe; in all these cases the diminished amount of oxygen introduced leads to an embarrassment of the pulmonary circulation, and to consequent effusion into the air-cells and small bronchial tubes. The fact is, that, in all the above instances, there is an approach to that condition, which in asphyxia attains its climax, and which consists in an obstruction situated in the rich vascular plexus spread over the walls of the air-cells, consequent, as we believe, on the absence of oxygen.

The explanation of Dr. Mendelssohn is, however, different to that just

given; the principal object of his work is indeed to prove that, during the normal expansion of the lungs, the pulmonary artery, together with its branches and the capillaries proceeding from them, are all elongated, and thus attract the blood from the right side of the heart by suction; and that it is the loss of this power which leads to the congestion and effusion. The following extract will suffice to convey the author's opinion upon this point. "If an opening be made into the pleura, and a communication with the external atmosphere be thus caused, the lung immediately collapses and no longer expands during inspiration. The lengthening of the vessels, in virtue of which the blood is sucked into them out of the right ventricle, also ceases; and thus, one of the causes accomplishing the circulation in the lungs failing, the blood must move more slowly and with difficulty. The want of the power exerted by expiration also contributes to the same result; for, in the normal process, the air contained in the lungs, is, by the force of expiration, so much compressed, as to acquire a density exceeding that of the atmosphere, and in this manner it exerts a general pressure upon the pulmonic capillaries and tends to empty them of their contents. The blood in the collapsed lung is propelled only by a power which is equal to a part of the contractile force of the right ventricle, the pressure, which normally is exercised by means of the motion of the lung and thereby promotes the pulmonary circulation, being entirely absent." After stating that the vessels are in this condition pumped full of blood by the right ventricle, the author observes, "the dilitation of the capillaries and the exudation of a large portion of the plasma into the areolar tissue of the lungs and into the air-cells are the necessary consequence of the circumstances above described."—(p. 142.) This theory is similar to the views supported by the older physiologists, that in asphyxia the collapse of the lungs is the essential cause of that impediment to the pulmonary circulation which it is well known occurs, but which, in the present day, is attributed to the cause we have assigned, the want, namely, of oxygen, an explanation that is not likely to be shaken by the experiments and arguments adduced by Dr. Mendelssohn.

In the second part of the work, occupying two hundred pages, the author enters into a long and somewhat tedious discussion respecting the several varieties of pneumonia, seeking to apply his physiological researches to pathology; but as a consideration of this branch of the inquiry would necessarily exceed our limits, we must here conclude by remarking that, although these researches are unnecessarily overlaid by a large number of extracts from various writers, and present little that is original, they may be useful to the physiologist by confirming the results obtained from other sources.

MEDICAL NOTES ON CHINA. By *John Wilson*, M. D., F. R. S., &c. Inspector of Naval Hospitals and Fleets. 8vo. pp. 267. London, 1846. Churchill.

AT the beginning of the year 1842, the Minden, a two-decker, was fitted up as a hospital-ship, and sent out to China, in consequence of our continued hostilities with that country, under the command of Capt. Quin. The medical department was entrusted to the superintendence of our author. Everything appears to have been provided by the Admiralty on the most liberal scale. The limit to the supply of medicines, medical necessaries, comforts and nutriments, including wine, ale, &c., was that of demand only, and this was left to the discretion of Dr. Wilson. It is altogether very pleasing to read of the excellent arrangements that were made to render the vessel a complete and efficient moveable hospital in every respect.

She arrived at Chusan on the 15th of August. From about the time of reaching the Cape of Good Hope, there had been a tendency on board to wounds and other injuries assuming the characters of sloughing ulceration. This was probably owing to the operation of a partially vitiated state of atmosphere on board, the result of crowding and inefficient ventilation, with which the unvarying diet of the crew, and also high temperature of the weather, may have co-operated; for we find that "on the long passage upwards of five hundred men were located in the main deck, which could not be so well ventilated as that portion of ships of war generally are, on account of the whole deck being planked over to protect the men from wet; but while that object was attained, the not less essential one of efficient ventilation was necessarily interfered with. Reid's ventilating apparatus was diligently worked, but as it was constructed for the hospital portion of the ship, the lower and orlop decks, it could exert no direct influence on the main-deck. At the same time, those decks, the lower and orlop, were filled with stores for the fleet, including salted provisions, from which there might be emanations prejudicial to health, and contributing to the vitiated atmosphere which excited the sloughing ulcer. It is right to state that the ship was rendered as clean as her crowded state would permit, and that the pump-well was kept dry and well-aired, through the medium of a tube connected with the ventilating machine."

Although the temperature at Chusan was not very high—the range being from about 76 to 84—upon the ship's arrival in the Chinese seas, much languor and depression were experienced by most of the crew. Diarrhœa and bronchial affections were found to be the prevailing diseases in the crews of the Thalia and Wanderer, the only two vessels lying in the harbour. In the North Star, then off the mouth of the Woosung River, a few cases of malignant Cholera had occurred: several of these proved rapidly fatal. It may be worth noticing that, in some of these cases, there was no purging, and but very little vomiting; yet the collapse was as great as if there had been profuse evacuations upwards and downwards.

During the month of September, we learn that "the principal form of diseased action in this harbour (Chusan) continued to be febrile, with tendency to, if not in every instance, complete development of periodic

movements, and affections of the mucous surfaces, either of the alimentary or respiratory organs. Besides diarrhœal complaints, there have been many cases characterized by frequent vomiting and purging of watery fluid, with rather severe tormina occasionally, and feelings of faintness, constituting a mild modification of common atmospheric cholera."

All the prevailing diseases exhibited the characters, more or less distinctly, of an adynamic or asthenic type—weak vascular re-action, and general debility. Hence the use of lowering remedies, especially blood-letting, was not well borne. In some cases there was a sudden and unaccountable sinking of the vital powers.

In the beginning of October, there were received on board the Minden, fifty soldiers of the 98th Regiment, which had suffered terribly from intermittent fever, dysentery, &c., while engaged in military operations upon the coast of the mainland in the Yang-tse-Kiang. This regiment had entered the river, little more than three months before, upwards of 800 strong; and now but 70 rank and file were capable of performing even the lightest duty: there was not one healthy man among them. Dr. Wilson very properly inquires, how it came to pass that this regiment should have suffered so much more than any other, seeing that all the troops were alike exposed to the miasmatic influence of the climate. He is inclined to attribute a good deal to the circumstance of the existing state of the health of the men at the period of their arrival in the Chinese seas. The 98th left England in December, 1841, on board the Belleisle, of 72 guns. This ship took out no fewer than 1300 persons in all, including the crew, and she was upwards of six months upon her voyage. Not to allude to the necessarily imperfect ventilation of the decks, when so many were collected together, and to the consequent deprivation of the air, it is to be remembered that very few fresh meals could be procured for such a company during the whole of that period. It says much indeed for the excellence of the present arrangements on board our ships of war, that neither scurvy nor any other epidemic broke out among this large assemblage of human beings crowded together in so small a compass. But, as Dr. Wilson very justly remarks, though regulations can accomplish much, they are not omnipotent; they may modify and counteract for a time the hurtful influence of certain agencies; but they cannot eventually resist the injurious effects of crowding, accumulation of animal exhalations, imperfect ventilation, uniform diet, and monotonous manner of life. He therefore most reasonably argues that these noxious influences, "although they did not excite disease on the voyage, yet, by the constitutional debility and deterioration which they induced, they rendered the body highly susceptible to the action of the causes of endemic disease on the shores of the Yang-tse-Kiang, to which the soldiers were exposed on their first landing;" and he then throws out a useful suggestion that deserves to be thought of by officers under like circumstances:—

"It may be concluded that, if the men, at the completion of the voyage, could have been landed for a while, when, with fresh air, moderate exercise and fresh provisions, they might have conjoined healthful amusements, they would at the same time have gained constitutional vigour, and lost susceptibility to disease before entering on active field operations, and thus have escaped the excess of disease and destruction, which all but annihilated their efficiency on first disem-

barking in China. It is stated, as strongly confirmatory of the above opinion, and is a very important fact, that the men longest quartered on the orlop deck, where ventilation was particularly defective, suffered in a much higher ratio than the others on landing." P. 32.

We need scarcely say that many of the poor fellows of the 98th, the victims of the malarious poison on the Chinese coast, sunk soon after their admission into the hospital-ship. The following is Dr. Wilson's account of the post-mortem appearances generally found in the bodies of the dysenteric patients.

"The examinations have presented in all cases, though in different degrees, extensive disorganization of parts, the seat and general character of the destructive change being remarkably alike in the whole. Ulceration has been all but universal, and obviously in most instances of long standing, the ulcers being scattered more or less thickly over the whole of the rectum and colon of various forms and superficial dimensions, generally circular, and from the diameter of a split-pea to that of a shilling or more, the surface being purulent, hæmorrhagic, ragged or clean and smooth, like the effect of excision; of various depths, sometimes destroying the mucous tissue only, sometimes the muscular also, being generally arrested by the peritoneal, but occasionally perforating it. In most cases, the rectum has been much thickened and indurated, in many assuming a semi-cartilaginous character; in other cases, it and the colon have been attenuated in some places while they were thickened in others; and in both, at spots free from absolute ulceration, the mucous lining has been loose and lacerable, or abraded. Frequently there has been serous infiltration of the peritoneal covering, the mucous lining having a leaden hue, through which light-coloured spots were sometimes interspersed, giving it then the appearance of dark marble; occasionally, it has had a purple colour, with a rough granular surface, resembling the exterior of a mulberry; and it has not unfrequently happened that the whole tube has been so much softened, broken-down, and disorganized, as to tear on the slightest touch, or fall to pieces on being lifted. With scarce an exception, the organic lesions have been limited by the ileo-cœcal valve, the small intestines and stomach presenting a perfectly healthy appearance, although venous congestion and small spots, denoting increased vascular action, have been found occasionally. When it is said that the small intestines and stomach presented a perfectly healthy appearance, it should be added that portions of the former, generally the distal extremity of the ilium, were often, in cases of long duration, much attenuated, and pale, and diaphanous, like a prepared colourless membrane." P. 57.

In two cases only, was the structure of the Liver at all affected. Indeed, with the exception of the great intestines alone, all the organs and tissues of the body were, in a vast majority of instances perfectly healthy. The amount of disorganization in the great gut, that is sometimes compatible with the prolongation of life, is truly wonderful. It was often observed that the patients shortly before death seemed to be somewhat better: after some slight exertion, such as emptying the bowels, gargling the mouth, or so forth, they became faint, and soon expired. In some of these cases, there had been a profuse salivation (not the effect of mercury) accompanied with ulceration of the gums, œdema of the cheeks, &c.: this was always a bad symptom.

The number of cases of Sloughing Ulcer received on board the Minden, for some months after her arrival at Chusan, was considerable. In some, the ulcerative destruction of parts was very deep and extensive. In one case, whose particulars are related, the ulcers commenced on the dorsum of both feet and over the lower ends of the tibia and fibula.

"When death took place, the right foot was in a state closely approaching that of a perfect skeleton, the metatarsal, and great part of the tarsal bones, being not only exposed, but as completely denuded, and cleared of every incumbent tissue, as if they had been carefully dissected. The articulating structures between the metatarsal and phalangeal and tarsal bones respectively, were also completely destroyed; so that it was necessary, some time before death, when dressing the foot, to support it with care at the extremities, to prevent the metatarsal dropping from its tarsal portion. The ulcer on the left leg, at death, embraced three-fourths of its circumference at its lower third, and extended upwards three inches, leaving the tibia and fibula bare for two inches." P. 73.

The co-existing dysenteric disease utterly forbad any thought of amputation. In one instance,—"certainly a very uncommon, and, if not an unprecedented, at least an unrecorded, one"—the effect of the miasmal poisoning was, apparently, the production of *spontaneous dry mortification of both hands*. The patient, a seaman, 20 years of age and previously in excellent health had suffered from ague and flux in the Yang-tse-Kiang. Soon after arriving at Chusan in October, the affection of the fingers began with aching pain in them, especially at night, preventing sleep, and afterwards with increased suffering; the affected parts were at first dark-coloured, and then black and lifeless. He was received on board the Minden about a month after the earliest appearance of the symptoms. At this time, "the disease had reached the distal joints of the thumbs and all the fingers, but had extended to the second joint of the index fingers, to that extent the circulation had entirely ceased, the parts being dead and black. Complaints of acute pain in the hands, shooting up the arms in the line of the ulnar nerves, and becoming much more severe at night;—pulse 108, sharp and irritative, skin dry, tongue healthy, bowels at present act pretty regularly, little appetite, no line of separation between the dead and living parts. The gangrene did not advance after he was received into hospital; the line of arrestation became evident a few days subsequently; separation of the sphacelated extremities, principally natural, but partly artificial, including the removal of projecting portions of bone, proceeded favourably; granulation and cicatrization followed, and, on the 26th, the cure of the stumps being nearly complete, and the general health restored, as his ship was on the point of sailing, he was discharged."

The patient had been treated with full doses of quinine, anodynes, and with occasional remedies for intestinal disorder, which recurred more than once during the course of treatment. In many respects, this case resembled one of *gangræna senilis*, or that form of mortification which arises from excessive cold. Dr. Wilson does not hesitate to regard this curious local affection as the result—an uncommon one, it must be acknowledged—of malarious poisoning.

As we have already said, intermittent fever and dysentery were the prevailing diseases at Chusan. Cholera was of rare occurrence. No case had been observed among the fleet anchored there, till the 4th of November on board the Thalia. The case proved rapidly (in five hours) fatal; the patient had been in perfect health before the attack. Dr. Wilson appears to be a decided non-contagionist; for he says:—

"As yet, it is a solitary case among the ships of war, though there have been some cases among the Indian troops quartered near the town, and the merchant seamen in the inner harbour. The subject in this case was one of a party from the ship, employed for three days previously about a junk, which had sunk with

copper on board, close to the shore of the inner harbour, and which they, with considerable labour, partly under water, had been getting up and landing. From that shore, the cause of the disease, whatever it may be, was, there is every reason to believe, in this case, derived; it was one of great intensity, and not destitute of useful information, for it goes to prove the purely terrestrial origin of cholera, its incommunicability by person, and the power of accidental agency—predisposing causes—in its production. The place where the party was employed possesses all the visible qualities of a malarious soil; the patient died in the ship among his messmates where there was no restraint, and no other person was affected by the disease, and all the other men employed in the same duty have heretofore at least escaped it." P. 43.

A week subsequently, another case occurred on board the same ship; but it was much less severe, and the patient recovered.

"The disease has not appeared in any other ship of the squadron, and it should be observed that no other is anchored so near to the plain of Tinghae and the inner harbour. Cases occasionally occur still in the merchant ships stationed there, as well as among the troops; but neither the number, nor proportion, nor the proportion of resulting mortality, is known, as there are no official returns, and the pressure of other duties prevents personal inquiry to any extent." P. 45.

While the squadron had been in the Yang-tse-Kiang off Nankin, there appear to have been about 90 cases, in all, of Asiatic or malignant cholera; of these, 30 had proved fatal. But by far the most serious scourges were agues and the flux. The crews of all the vessels suffered more or less severely from these endemic pestilences; and sad was the havoc among some of them. There was, however, one ship in the squadron, the Apollo, which was so much more healthy during the whole time that she was in the Chinese seas than all the rest, that we are naturally led to inquire as to the probable cause of her superior immunity from disease. This frigate was in the Yang-tse-Kiang during all the operations off Nankin; remained there till the signature of the treaty, and then stayed at Chusan for three months before returning to England. During the whole of this period, she was healthy, absolutely and relatively. Upon communicating with her captain and surgeon, our author found that the only peculiarity in the hygeienic arrangements on board the Apollo was, that the lower parts of the ship were regularly washed and purified by pumping water in and out every day in very hot weather, and three or four times a week, when the temperature was low. By this means, though the space under the limber boards was never absolutely dry, no impurities arising from leakage or otherwise were ever allowed to accumulate and give rise to the offensive effluvia of bilge-water. It is a proverbial saying among nautical men, that "a leaky ship is a healthy ship." Whether this be true or not, the prevailing opinion among the medical officers of the navy for many years past has been, that it is an important thing for the health of the crew, that the lower parts of a ship, as well as the inhabited decks, be kept as dry as possible. No one is likely to question the truth of this remark as far as the decks are concerned; but it is very different as respects all the parts under the limber boards; and for this very simple reason, that it is scarcely possible, by any means, to keep them absolutely dry. Now, if this cannot be done, the consequence must be, that there is always under the flooring a quantity of confined and stagnant fluid, which is constantly exhaling offensive and necessarily deleterious effluvia. Such then being the case, it is

certainly not unreasonable to believe that it would be better to wash out all the lower parts well and frequently than to leave them in this condition. The frequent admission and expulsion of water will have the effect not only of preventing such an accumulation, but, it may be also, of absorbing and carrying off the impure air, as it is formed, that is necessarily generated wherever there is any decaying matter present; and that such a generation is continually going on in some vessels cannot well be doubted.

Dr. Wilson tells us that, when he was in the West Indies, about sixteen years ago, it was then the usual practice, upon the arrival of a ship on that station, especially if there was any appearance of fever on board, to take out all her stores, and whitewash and dry (by means of stoves) the whole interior, from the hatches to the keel, till all sign of dirtiness or dampness was removed. But what was the result of all this labour, according to his experience? "It happened frequently—more frequently than otherwise—that fever, if it had begun, instead of being arrested or moderated, became more frequent and severe after than it had been before; and often set in at no distant period, if it had not shown itself previously. This relationship the writer pointed out, and offered some remarks on, both in the 'Memoirs of West Indian Fevers,' and the 'Statistical Report on the Health of the Navy,' in the West Indies, especially the former. He was satisfied then, as he still is, that the process in question did not answer the laudable end for which it was ordered, that, namely, of staying or retarding the march of the destructive disease; nay, that it often precipitated the eruption of the disease, and rendered it more violent."

We were certainly not prepared for such a result as this; nor can we well perceive how the thorough purification—by the dry or wet process—of a ship's interior (and this, too, be it remembered, in harbour, where every possible facility is procurable) should ever promote the development or spread of any fever. Whether, indeed, the regular and frequent washing out of the hold by the free admission and forcible expulsion of water might not be advantageously substituted for the operation of dry cleansing is a subject that well merits the attention of all naval surgeons, and we trust that they will not fail to test the value of the practice in different climates. Our author is evidently inclined to think favourably of it; and his opinion is deservedly entitled to much consideration. He alludes to the late very lamentable case of the Eclair (recently returned from the coast of Africa,) and suggests that the process noticed above might perhaps have been employed with benefit.

The fearful pestilence on board this steamer was, he thinks, derived, in a great measure at least, from the ship herself.

"Every thing in the progress, character, and cessation of the disease in the Eclair, so far as the writer has the means of judging, points to its local origin, and shows that it was generated on board, its cause being the product of chemical action within the vessel, probably in her lower and least accessible parts. It has been alleged that the disease was propagated by personal contagion. This is not the place to discuss the subject, but it may be asked in passing, where are the proofs of the allegation? The great cumulative argument, and the only one which can satisfy rational scepticism, *that, namely, of its extension to healthy persons living among diseased persons, after the latter were removed from the place where they sickened, is altogether wanting.* Had any one person attending on or com-

municating with Dr. Rogers and the other fever patients, after they were moved from the Eclair to a sound ship, been affected, the person so affected, not having been on board the Eclair, the case would assume an entirely different aspect." P. 93.

With the removal of the affected from the Eclair, and the termination of fever in them, the disease entirely ceased.

What has now been said in reference to the (probable) good effects of frequent lavation of the interior of a ship's hold, may be applied with equal force to the lavation of the ship's company, by the regular and, when possible, the daily use of sea-bathing, or ablution with salt water. Sailors have, in this respect, a great advantage over soldiers, who are so often not only quartered in an unhealthy district, but have few or no facilities of personal ablution and refreshment.

The Minden remained at Chusan until May, 1843, and then sailed for Amoy, on her way to Hong Kong. Perhaps a few remarks here on some of the more prominent features of Chusan and its people may not be unacceptable to our readers.

The island is described as being highly productive. Every spot of ground, from the sea-beach to the summits of the hills, is under cultivation. Rice is the principal growth; but, besides this grain, millet, maize and wheat, are also grown. There is abundance too of excellent vegetables. There are very few forest trees to be seen. Bamboo is, in the arts of life, what rice is in respect of food—a substance of almost universal application.

The Chinese seem to have anticipated, in practice at least, some of the recent suggestions of scientific agriculturists on the highly important, albeit somewhat unsavoury, subject of manure. Human ordure is everywhere most carefully collected and preserved; it is valued to a degree which would seem perfectly ridiculous in other places.

"Close to the houses, which in the country are grouped into hamlets in the hollows, there are placed large earthen pans, into which are thrown all fragments of decomposable substances, to which are added the desired quantity of fluid, including urine. With heat varying from 80° to 86° in summer, destructive fermentation proceeds rapidly, and multitudes of maggots are developed, giving life and motion to the loathsome compound. Scarce anything can be conceived more disgusting than the contents of these vessels; but the Chinese disregard their offensive qualities, looking to their useful ones. They arrange them in rows along the public paths, and always place them near, if not in contact with, their houses, that they may at once commit to their keeping every particle of available matter. When putrefaction has proceeded to the point of giving the highest degree of fertilizing power to the mass, it is transferred to buckets, and carried to the fields, often to considerable distances, and when the ground is steep, with great expenditure of human strength; for, except when yoked to the plough or harrow, man's labour is not lightened by ox, horse, or other quadruped. Placing a bamboo across the shoulders, with a bucket at each end, they carry what they consider, and what no doubt is to them, very precious stuff, in large quantities, to distant places, and often difficult of access; showing at once the value of productive soil, the industrial habits of the people, and the muscular power of the persons who perform this part of the farm labour. These receptacles of filth, and sources of fertility, are not confined to the country; the towns have an abundant share of them." P. 12.

The cold in the winter season at Chusan must be severe; for we are

told that the people store up immense quantities of ice, chiefly for the purpose of preserving fish, which forms a large portion of their food in summer.

The chief diseases of the natives appear to be the same as those which have affected our troops quartered in the island, and the crews of our ships anchored off it. These, as already mentioned, are remittent and intermittent fevers, and bronchial and intestinal irritation, in the form of catarrh, diarrhœa, dysentery, and so forth. Cholera has occasionally prevailed to a great extent, and occasioned severe mortality. The principal external affections seem to be scrofula, ophthalmia, and various cutaneous maladies, including elephantiasis. Scabies is disgustingly common; nor are much pains taken to get rid of it: even a Mandarin feels no shame in shewing his hands—and this at the dinner table too—covered with the itch!

The food of the working classes consists almost exclusively of vegetables, especially rice, with salted fish. The immoderate use of tea, and of opium and tobacco-smoking, must tend to deteriorate the constitution. The opium is used as a luxury; the tobacco has become almost a necessary of life: "they never drop the tobacco-pipe; smoking from morning to night, and drinking largely, at short intervals, a miserably weak infusion of coarse tea." Their habits in reference to ablution, are intolerably filthy; they seldom or never change their garments. The following remarks on Elephantiasis may be read with interest.

"Elephantiasis, judging from the number of cases casually seen, is as common here as at Rio de Janiero, or even at Barbados, where it is so prevalent as to have given rise to one of its names—*Barbados-leg*. It is generally, and there is reason to believe, justly, considered as an endemic disease in the proper meaning of the word. Yet, on examining the topographical and appreciable climatorial constitution of the three places, few points of agreement can be discovered between them, though in some peculiarity of these things in conjunction with the modes of living and personal practices, the causes of endemic disease must consist. In the latter particulars, it is true, in diet and domestic management, there is considerable similarity between some classes of persons, in the positions named. Both at Rio de Janeiro and Barbados, elephantiasis prevails principally among the negroes, who, like the poorer Chinese, subsist almost exclusively on vegetables, with a portion of salted fish. Like them, they are also very often dirty in their persons, their cabins, and their clothes. These things may have much influence in producing the disease, but it is evident that they are not of themselves sufficient; for other classes and races of men, who feed as poorly, and are not more cleanly, are not affected by it.

"Respecting the nature and rational treatment of elephantiasis, there is fortunately less doubt than on the subject of its origin. It consists essentially in inflammation of the sub-cutaneous cellular tissue, which leads to effusion, the effused matter becoming imperfectly organized, occasioning tumefaction, and impairing more or less the motive power of the limb. Repeated attacks of inflammation, effusion, and consolidation, produce, if not arrested, in the course of time, generally many years, the highly enlarged, tuberculated, scaly leg, resembling very closely that of an elephant. As the disease advances, the functions of every tissue, especially of the skin, ligaments, and bones, are injured or destroyed, by increasing pressure, till, beginning with the separation of the toes, disorganization proceeds to a fatal issue." P. 25.

By far the most efficient local remedy is the copious abstraction of blood by incision or free puncture of the diseased integuments. The genital

organs are, it is well known, a common seat of Elephantiasis. Besides the diseases already mentioned, Small-pox is not uncommon: Syphilis too is frequent in the sea-port towns.

When the Minden arrived at Hong-Kong in the beginning of June, remittent (not intermittent, as at Chusan) Fevers and Dysentery were found to be very prevalent. Very generally these two morbid states were associated together; so that it was often difficult to say which was the primary and principal disease. "Whichever appeared first, it constantly happened that, as one series of morbid actions declined, the other rose." This circumstance added greatly to the difficulty experienced in the treatment of the patients; for what promised relief for the one state, was anything but advisable for the other. As the severity of the fever declined, it often, nay generally, assumed an intermittent type; and very frequently when this was subdued, gastric irritation with flux set in, to be succeeded perhaps by the recrudescence of the periodic fever.

In most cases of the Hong-Kong remittent fever, the cerebral symptoms were strongly marked; hence it was usually called "head fever" among the sailors. In the worst cases, the febrific miasm acted like a concentrated poison, paralyzing all the powers of life. In some instances the symptoms, at the outset, resembled a good deal those of malignant cholera. Dr. W. scouts the Broussaian idea of the disease being the result of any local inflammation: it is truly and strictly an essential fever. Dissection threw but little, if any, light, upon its nature: congestion, not inflammation, was the prevailing character of the morbid appearances observed in the viscera.

Sloughing ulcer was a much more rare affection at Hong-Kong than it had been at Chusan. This, and the circumstance of remittent taking the place of intermittent fever, were the chief points of difference in the medical constitutions of these two localities. In the dysentery of Hong-Kong, the stomach and small intestines were more frequently affected than in the same disease at Chusan.

On the whole, Hong-Kong seems to be anything but a healthy spot. The troops have suffered fearfully from the effects of miasmal poison—arising, no doubt, from the swampy ground in the neighbourhood of the new town Victoria, which used to be cultivated for rice plantations. Dr. Wilson remarks more than once that those persons, who resided in the central parts of the town, suffered much less from fever and dysentery than those living in the outskirts. He strongly recommends that the cultivation of rice (which, it is well known, always requires a great deal of moisture) should be entirely abandoned, and that of wheat and other cereal grains, be substituted. But it should always be borne in mind that, unless the latter part of this recommendation be carried into effect, no change at all should be made; as "a cultivated rice-field is much less dangerous than the broken, swampy, weed-covered condition of the surface which follows its neglect." It is therefore a most important point for all settlers in a place like Hong-Kong to attend to this circumstance.

We shall now direct the attention of our readers to the most practical parts in the present volume—those which describe the treatment of the diseases which constituted the great bulk of the cases received on board the hospital ship, while stationed at the two ports in the Chinese seas,

Chusan and Hong-Kong: and first, of the *Remittent Fever*, endemic at the latter place.

In the worst cases of this fever, characterized by extreme depression, the first and most prominent indication was to sustain the sinking energies of life by the use of stimulants and cordials. When reaction followed, the principal remedies were local abstraction of blood, blisters, occasional aperients, effervescing draughts, and moderate doses of calomel with or without opium, and sometimes with ipecacuan. In a few cases, more active depletion was necessary; but this was rare: in some cases, the detraction of blood was clearly injurious. The chief danger consisted in the marked tendency to excessive debility and fatal collapse; and, as this was apt to supervene even after the stage of reaction had set in, the greatest watchfulness was required on the part of the medical attendant. Hence "the practice was often one of expedients, and was based less on fixed principles than the pride of science is willing to admit." Dr. Wilson is very earnest in deprecating the use of large blood-letting, merely because the pyrexial symptoms may run high during the exacerbation of the fever. The local abstraction of blood is almost always much safer, and is, moreover, generally sufficient. The doses, in which calomel was given, were either one grain every two hours, or three, four, or five grains two or three times in the 24 hours. This was considered a safer practice, and better suited for the ends aimed at, than scruple or half-drachm doses two or three times daily. Dr. W. candidly confesses that, on the whole, the results of the calomel treatment were not very flattering to the practitioner's hopes. There was reason to fear that the appalling collapse and rapid sinking were sometimes rather accelerated than otherwise by the unguarded use of this energetic medicine.

When the remissions became more decided, and the exacerbations less violent, the chief reliance was placed upon quinine; but our author expressly says that, "although it sometimes answered expectation, it more frequently disappointed. Here, as in intermittent fever, neither it, nor any other form of cinchona, could in many instances be long tolerated, being speedily followed by, if it did not excite, *flux*."

We must not close our remarks on the treatment of Remittent Fever without mentioning that Dr. Wilson has strongly insisted, at the beginning of his work, on the great importance of emetics exhibited at the outset of most febrile affections, including Cholera and choleroid attacks. He very justly remarks, that "the practitioners of the last age made much more use of emetics than those of the present; they might employ them without due discrimination, and therefore empirically in some instances; and they might entertain erroneous notions as to the ground on which they were administered: but, however these things might have been, it may be affirmed that emetics do not, at present, hold the rank they are entitled to among therapeutic agents; and that, especially at the onset of many febrile diseases, they are undeservedly and injuriously neglected."

Intermittent Fevers.—The most frequent type was the quotidian, and then the tertian: quartans did occur, but not often. We need scarcely say that the treatment consisted in the exhibition—premising a warm bath, and occasionally a brisk purge—of quinine; two grains in solution every

three or four hours during the intermission. Under this plan, "success was sure, and generally speedy," even when the disease had continued for a length of time. (The cure was never considered complete till some weeks had elapsed after the last accession, however slight, of the fever.) This remark, however, applies only to those cases in which the fever was uncomplicated. Whenever there were other forms of disease, more especially Dysentery, co-existent, the greatest difficulty was experienced in effecting a cure. The very existence of the febrile disease was often masked by the intestinal affection.

"Sometimes it was, for a while, so much disguised as to require close observation and questioning to detect its existence; and when its recurrence was evident, there was often regularity in the time of accession, as well as in the duration, and development of the different parts of the paroxysm. Rigor was seldom well-marked; sometimes the hot stage was wanting, the paroxysm consisting chiefly in profuse sweat, with sensations of chilliness rather than heat; instead of being ushered in by coldness and shaking, the invasion was occasionally characterized by numbness or total temporary suspension of sensibility in the lower extremities; and there were other anomalies and discrepancies of other kinds in the febrile paroxysm, difficult to describe, and unnecessary to detail, which were yet of such a nature as to leave no doubt of their being products of the miasmal poison and the integral parts of the periodic fever." P. 204.

The intestinal irritation prevented the exhibition of *the* remedy for the ague. Neither quinine, nor any other form of cinchona, could be borne; and even simple bitter infusions often seemed to disagree with the patients, after the flux had been arrested. Under these circumstances, a trial was made of *arsenic*. The following practical remarks on this potent remedy deserve notice:—

"In many instances, it failed to subdue the disease or lessen its violence, but in others, which, without its intervention, seemed hopeless, its remedial efficacy was unquestionable. When there was neither destructive organic lesions nor helpless exhaustion, conditions alike irremediable by any means, its power over the periodic paroxysm soon became apparent; and, when fully established, generally continued till their recurrence was fully prevented, and the febrile part of the combined affection was consequently cured. While acting thus favourably on the fever, it did not, as was apprehended, aggravate the flux; nay, it seemed, though the appearance was not so clear as to warrant a positive conclusion, to produce a beneficial effect on it also. At any rate, while it did not injure, it did not prevent the use of such means as might prove directly curative in the alvine affection." P. 205.

The dose, usually employed, was three or four minims of the solution in camphorated mixture, or infusion of Diosma or Buchu leaves, (which acts serviceably in some forms of flux,) two or three times daily.

Acute Dysentery.—In the early stage, when there is high fever, and the abdominal symptoms are severe, a large bloodletting—from 25 to 30 oz. if faintness does not occur—followed by a warm, or rather a hot (105°) bath, is the remedy on which we must chiefly rely.

"Within a few hours—six will be sufficient to show the efficacy of what has been done—if there be not the most unequivocal yielding of all the urgent symptoms, recourse must be had to local bloodletting. From fifteen to twenty-five leeches should be applied to the abdomen, and ten or twelve round the margin of the anus; a moderate quantity may often be drawn from the margin of the

false ribs, about the same time, by the cupping instruments. On the completion of these measures, the bath may again be employed; after which, the patient being dried, and speedily carried to bed between blankets, the abdomen should be covered with a light warm poultice. Twelve hours hence, or twenty-four hours after admission, if there be any doubt as to the disease being mastered, and yielding rapidly, a large blister must be laid on the abdomen, not a patch covering a hand-breadth, but a sheet extending from the pit of the stomach to the pubes, and from one iliac hollow to the other." P. 208.

Calomel and opium—to which, in most instances, ipecacuan or antimony (?) will prove a useful addition—are the internal remedies which, in our author's opinion, deserve the chief reliance. The average doses may be stated at four grains of calomel, one grain and a half of crude opium, and the same quantity of ipecacuan, to be given every third hour. The patient should not be teased with any other medicine. By the use of the means, internal and external, now mentioned, "nine cases in ten of the form of dysentery under consideration, at the stage indicated, will be successfully treated."

Dr. Wilson does not approve of the use of purgatives in acute idiopathic dysentery. "Retained secretions," he quaintly remarks, "like pent-up fæces, have a larger place in the imagination of the practitioner than in the person of the patient."* The theory, as well as the practice, of Dr. Wilson is much more likely to find favour and followers than the strange doctrines—would that they were merely so—of Dr. MacGregor, as described in the last number of this Journal. With respect to the *modus operandi* of mercury—in conjunction with opium—in dysentery, our author is of opinion that it acts, not by exercising any specific power over the intestinal affection, or any peculiar influence on the liver, as some writers have supposed, but solely as a general sedative and antiphlogistic. Hence its marked utility, not in dysentery only, but also in all febrile affections in which any part or organ of the body is "in such a state of vascular derangement, inflammatory or congestive, as to threaten rupture of vessels, abscess, ulcer, gangrene, or other form of disorganization." And here we may remark, that it is a great, although far from being an uncommon, fallacy to suppose that the mercury must be pushed to the extent of salivation, if we hope to effect a cure of dysentery and some other diseases. It is surely enough—we are appropriating the words of our author—that the morbid condition is removed, whether one of the ordinary, certainly not curative, effects of the medicine be realized or not. Not to be satisfied with this point argues a want of common sense, not to talk of professional reflection.

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* This allusion to retained secretions, &c., suggests to our mind that it might perhaps be well if medical men were in the habit of examining the more obvious chemical properties—we allude more particularly to those of acidity and alkalinity—of the thin alvine evacuations in a disease like acute dysentery. That there is sometimes an inordinate excess of acid in the black vomit of Yellow Fever is a fact that has, we believe, been distinctly proved, and to which reference was made in a late number of this Journal, No. 85, p. 281.

Whether the association of mild alkaline medicines along with mercurials and opium might not be beneficial, is a question not undeserving of attention. Does not chalk act as an astringent, in most cases, by neutralizing a predominating acid in the intestines?

But mercury is not always remedial in dysentery, even when administered with judgment and in moderation. In some cases, the system will be found to be intolerant of its action; it produces great depression, and, if persevered with, fatal collapse. The treatment of such cases requires the greatest watchfulness and discrimination. As a matter of course, the use of the mercury must at once be suspended, or altogether given up.

When the Dysentery is chronic, astringent enemeta—zinci sulphat. gr. x., aquæ ℥ vj., tinct. opii 3 ss.—have been found extremely useful in allaying the irritation of the bowels and checking the intestinal discharges. The exhibition of mild aperients, as magnesia and rhubarb, castor oil with a few drops of laudanum, is often beneficial in this stage of the disease.

The Dysentery that prevailed on the coast of China was unusually intractable; and for this reason, that it was seldom an idiopathic uncomplicated affection; but most frequently associated with Intermittent Fever, secondary to, and alternating or co-existing with it. In many cases it was associated with ulcers of the extremities; "generally as one form of disease rose in force, the other fell." These complications cease to surprise us, when we bear in mind that the three morbid actions were doubtless attributable to one and the same cause, viz., miasmal exhalation.

The febrile excitement in the first stage was seldom so great as it usually is; nor were the patients able to bear the same amount of depletion. The doses of calomel, opium, and ipecacuan required to be considerably diminished. Salivation was carefully avoided; the exhaustion, consequent thereon, was too great for the patient's strength. Dr. Wilson believes that many lives have been sacrificed to the injudicious administration of mercurial medicines.

"In a few cases, when the dejections consisted chiefly of blood, acetate of lead produced good effects:" to such cases, its beneficial influence was confined. The sulphate of Zinc did not maintain its reputation in any form of the disease. The nitrate of Silver was not more successful. With respect to astringents generally, (in the chronic disease,) our author makes the important practical remark, that none of them "had the power of restraining the discharges even temporarily, in most cases; and, when that effect was produced for a little, it was constantly accompanied by more severe tormina, and soon followed by increased frequency of stools."

The application of a few leeches to tender parts of the abdomen, or round the anus, always produced relief. Effervescing draughts with a few drops of opium or hydrocyanic acid were most grateful, and, often too, very useful. In lieu of blisters to the abdomen, the following rubefacient was employed with good effect:—

"An ointment, consisting of equal parts of mercurial and iodine ointment, with a small portion of cantharides plaster, was rubbed carefully over the abdomen, to the extent of a drachm daily; after which, a flannel roller was applied. In this way the irritation of a large blistered surface was avoided—no light consideration, when there was tendency to sinking; and the derivative action thus established, without considering any other, was not only more steady and constant, but also more effectual on the whole. If vesication, in a slight degree, took place, the application was suspended, to be resumed if required." P. 220.

In a great number of cases, the disease resisted every variety of medicine and mode of treatment. We have already mentioned that the infusion of the Diosma Crenata or *Buchu* seemed to answer, on the whole, better

than any other vegetable tonic in giving strength to the bowels, in many of the forms of chronic flux. During the progress of convalescence, chalybeate preparations were occasionally of marked benefit: the *tinctura sesquichloridi*, in full doses, was preferred. Under its use, the complexion often improved, and strength was gained. In all cases, the food that was allowed was given in small quantities at a time and of the simplest nature: "moderate quantities of farinaceous food were (often) alone compatible with a curative process."

With respect to the Dropsical effusions that so frequently occur in the course of chronic dysentery, it will be best to give Dr. Wilson's observations in his own words:—

"In all such cases, the principal object aimed at was remedy of the condition on which the dropsy depended, comparatively little attention being paid to the effusive effect. That condition consisted almost universally in organic weakness; organic obstructions, considering the nature of the primary disease; and its powers, real and imputed, of producing structural lesion in other places, were singularly rare. Little was expected from diuretics, and they accomplished even less than was looked for. To specify the various means resorted to in the dropsical complications, would be little more than a repetition of the measures used in periodic fever and flux. Irregular ague, frequent purging, ascites, anasarca, and extreme debility, co-existent, presented as hopeless a subject for medical management as can well be conceived. Of such cases, there were many, most of which proved altogether intractable; but, in some of them, decided advantage, more certainly than from anything else, was derived from the combination of arsenic and buchu.

"In a few cases where effusion was confined to the abdomen, anasarca having ceased, and other cavities being clear, friction with the compound ointment, already twice referred to, was followed by marked improvement. Under its use, tumidity subsided, sometimes permanently. Whatever may be thought of the physiological fitness of the application in this or the other affections, no doubt is entertained of its practical utility." P. 225.

Perhaps this will be as good a place as any to mention that there was an unusual tendency to the generation of Intestinal Worms among the sick in China. Dr. W. remarks upon this subject:—

"There is great disposition to the formation of intestinal worms, almost exclusively lumbrici, here, (Hong-Kong,) as well as at Chusan, which are sometimes generated in extraordinary numbers, being occasionally voided by the mouth, but more commonly by the anus, and giving no symptoms of their existence till they are discharged: they are found in masses, on post-mortem examinations, after fever and flux; the former, as frequently as the latter, when protracted. Their extensive production has been ascribed, erroneously it is believed, at Chusan, to the water found there, containing, as it sometimes does, portions of earthy and vegetable matter; for they are as numerous at Hong-Kong, where the water is singularly clear, and free from admixture of any kind, except small quantities of mineral substances, which it holds in perfect solution, and which cannot be supposed conducive to such effects. There is little apparent difficulty in accounting for the abundance of these parasites in China; it can scarcely be questioned, that the excessive tendency to, and occasional accumulation of, them, arises out of the enfeebled, unhealthy condition of the alimentary apparatus, more particularly of the interior membrane; being infested by depraved secretions, and coated with adhesive mucus, it ceases to perform its proper functions adequately; and from the same cause, becomes the prolific bed of those creatures. Neither their origin in the intestines, mode of production, nor introduction, is of any practical

importance; it is probable that their ova or elementary germs are extensively diffused, and constantly carried into the stomach with dietetic substances: it is certain that their existence is incompatible with a sound state of the living parts, in which they are developed; and that what is required for their prevention is vigorous healthy action in these parts." P. 193.

Sloughing Ulcer.—We have already remarked that there was a marked tendency to this very troublesome form of disease on board the Minden in her outward voyage. *Then*, it was believed to arise from the operation of a peculiar miasm evolved from the hold and lower parts of the ship; and that this was the cause is rendered highly probable by the circumstance that, when all the stores were taken out at Chusan and the vessel well cleaned out, no new cases occurred for a considerable time. The morbid tendency, however, became afterwards re-developed, in consequence of the operation of malarious miasms; for the connection between the ulcerative disease, and the periodic fevers that prevailed, was too obvious to escape the notice of any one. They doubtless arise from the same morbific influence, and are but different indications or exponents of its operation upon the system.

By far the most effectual remedy in the *treatment* of the sloughing ulcer was the employment of free incisions in the surrounding integuments.

"In a great majortiy of cases, the sloughing process had advanced far before the patients were received; in some, as formerly stated, bones were denuded, and tendons and ligaments destroyed. But in very few, even where the destruction was greatest, was treatment by incision omitted; and in those only where, from fever or flux, there was great constitutional debility. The amount of incision was regulated by the extent of disease in the tissues under and around the ulcer. In some instances, where it did not descend below the integuments, the ulcerative process being phagedenic rather than gangrenous, and the destruction neither very rapid, nor reaching under tissues, it was sufficient to relieve the more superficial vessels, and to substitute scarification for what is understood by incision. More frequently, however, it was necessary to use the knife freely, passing it through the skin, and into the underlying cellular structure. Whatever the proper depth might be, the scalpel was carried quickly from beyond the limit of surrounding disease to the ulcer, often through it. The distance between the incisions varied, but was generally less than a quarter of an inch; their direction was most frequently parallel in the line of the limb, occasionally radiated from a circle, clear of the affected integuments, to the ulcerated centre, according to the position of parts, and degree of vascular action. In many cases, it was necessary to repeat the practice; in some, frequently." P. 227.

The effects of this mode of treatment were generally prompt and most satisfactory. The relief to pain and irritation was often immediate; and, although the remedy was a painful one, patients sought rather than shunned its repetition, upon the recurrence of bad symptoms; so unequivocal was the benefit derived from it.

"Instead of sanious fœtid discharges from the ulcer—its ashy, livid, or black surface, and abrupt margin—there was secretion of pus, separation of sloughing matter, and a crop of florid healthy granulations; the surrounding parts, which had been tumid, darkly inflamed, or œdematous, or having both conditions combined, became flaccid and shrunken, assuming the pale complexion of health. In no instance did the sloughing action extend to the incised surfaces, which either healed speedily by adhesion, or more slowly, but not less surely, by granu-

lation. This, which may appear strange, is unquestionable, and an important fact; for if there were any doubt respecting it, the value of the operation would be lessened, if not rendered problematical." P. 228.

But it was not only in the Sloughing Ulcer that so much benefit was obtained from the treatment now recommended. In many cases of Indolent Ulcer—with flabby granulations, and a thick, hard, cartilage-like margin—"nothing availed so much as carrying a scalpel from the periphery of the diseased tissues to the centre, cutting freely, and at many points, through the indurated ring which formed the limit, and prevented the extension of cicatrization." In some cases, the practice required to be repeated several times.

To promote the flow of blood, and to keep up a discharge of serolymphous fluid from the incisions, water dressings, warm or tepid, were always employed for two or three days, or longer if deemed necessary. When the sloughing process had ceased, and the ulcer had put on the appearance of healing, the solution of Chloride of Lime and diluted Nitric Acid were found useful, by gently stimulating the part to more vigorous action. "In some cases, where there was unhealthy, phagedenic or partially sloughing, action, the parts beyond the ulcer being sound or little affected, undiluted nitric acid was very useful; and in others, especially those marked by œdema and other symptoms of inaction in the surrounding surface, a solution of nitrate of silver, more frequently the rod itself, was applied with excellent effect, to the diseased integuments, as well as to the ulcer."

After the ulcer had healed, or nearly so, much benefit was often derived from the daily affusion of cold sea-water upon the limb. Considering the constitutional origin of the form of Ulceration now under notice, and its intimate association with Ague and Dysentery, we need scarcely say that appropriate internal treatment was invariably employed along with the use of those external remedies which we have already described.

The results of the treatment were, upon the whole, very satisfactory, as appears from the subjoined statement:—

"One case sketched in a former page, both lower extremities being affected, with great complication and resulting debility, terminated fatally. In another instance, it was necessary, on account of extensive, progressive, and hopeless disorganisation, to remove a limb below the knee. But all the other cases ultimately terminated in recovery. From the amount of destruction, the restorative process was protracted in most; it was uncertain and vacillating in many, frequently arrested in some, the condition being all but desperate, and giving rise to grave questions about the duty of amputation. In such a state of things, it is much to escape with life, and entire limbs; and, although some of the subjects may never be fit for very active duties, on account of the extent and position of cicatrized parts, the effects of the measures instituted, and most zealously, as well as skilfully carried out, are felt to be not only satisfactory but gratifying." P. 232.

The hot season—from May to October inclusive—of 1844 was altogether much less sickly than the corresponding period in the preceding year. It is often, nay generally, impossible to account for these changes in the "medical constitutions" of different seasons: for, as far as we yet know, there is no steady relation between the condition of a season in point of salubrity, and any barometric, thermometric, hygrometric, or other appre-

ciable states of the atmosphere. Although, however, we cannot explain the cause of such hygienic differences, while the physical conditions of the air and earth are seemingly very much the same, the truth of the fact cannot be disputed; and due attention to it, it should ever be remembered, is of great importance in estimating the value of any remedies or modes of treatment which may be recommended, at any time, from their alleged superior efficacy. A line of practice, that may suit the epidemic of one year or of one place, may be utterly valueless or even injurious in that of another. Hence the fallacy of laying down any dogmatic rules as to the treatment of an epidemic or endemic disease, without reference to the "medical constitution" of the particular season or locality; and, at the same time, the danger of building any structure of therapeutic principles on the results obtained by Mr. this and Dr. that, in such a year or such a place. Dr. Wilson very justly remarks, that "a practitioner arriving here (Hong-Kong) in the summer of 1844, would find a much larger proportion of the sick under his care recover, than did the practitioner of 1843; he would, probably, with a laudable desire to accomplish more than the men earlier in the field had done, modify the means of treatment used by them, or apply something different: and to this modification or alteration of means, not to the comparatively slight morbid impression, he would be apt to ascribe his better fortune, counting it the effect of professional merit, not of changed circumstances. Having satisfied himself of this, he, and some others who are prone to believe much and to hope much, readily arrive at the conclusion, that his predecessors were inefficient practitioners; that they did not understand, or did not properly perform, what was required of them by the obligations of their office; and that they are, therefore, chargeable with the heavy crime of having allowed men to die who might have been saved."

He sums up with the humiliating, but nevertheless the useful because the candid, admission that—

"The more that is seen of some forms of disease, and the more closely they are studied, including especially the precipitous fevers of the tropics, the more likely is the conviction to come and deepen, that medicine has often but little remedial control over them; and grave questions will then arise as to whether artificial appliances, confused and contradictory as they often are, may not prove injurious rather than beneficial—if not curative or tending to cure, will they not become instruments of harm? Experience also shows that, while we think we are making rapid onward progress, we are often moving in a circle only, returning, after a long, toilsome, unprofitable journey, to the spot where our ancestors arrived long ago. This truth, which should humble and correct, not arrest, us in the course of diligent sober inquiry, meets us at every turn. One example out of many may be alluded to, namely, the treatment of dysentery in India, though that of fevers in general would not be less in point." P. 190.

In short, more benefit is, in many cases at least, to be looked for in the direction of Hygienic and preventive, than of Therapeutic or curative, Medicine. The great improvements that have been effected in preserving the health of our sailors, more especially by the superior arrangements in reference to their food and clothing, give ample encouragement for the reasonable expectation of still further meliorations in the way of protection from disease, by the diligent application of different hygeienic measures

to the varying circumstances in which our soldiers and sailors may be placed. We have seen the strikingly good effects of a particular practice on board one ship—the Apollo. Why then may we not expect that other plans and methods of hygeienic improvement may be discovered? The subject is one of first-rate importance, and should be never out of the mind or thoughts of naval and military surgeons while engaged in those climates where fevers, dysentery, and such like diseases are always more or less prevalent.

Dr. W.'s last Chapter gives us a brief, and necessarily a very imperfect, sketch of Chinese medicine. As yet we know too little of the people to be able to appreciate, with any degree of fairness, the progress which they have made in the healing art. From our author's account it would seem to be in the most rude and contemptible state. In medicine, as in other branches of knowledge, the Chinese appear to have remained for centuries —it may be for tens of centuries—in a state of "petrified fixedness," which nothing has moved. In their physiology, they recognise five elements—fire, water, earth, metal, and wood! They have a vast materia medica, and the physician's prescriptions usually contain not fewer than nine or ten ingredients.

As a matter of course, in a country where anatomy is so utterly neglected, the practice of Surgery must be very rude and ignorant. John Chinaman is not a bold reckless character; and he therefore never goes deeper, in any of his surgical operations, than the surface. A vein is never opened designedly; for there is the greatest horror of shedding blood, except by the hands of the executioner. The moxa is a favourite remedy among the Chinese doctors. Their mode of inoculating for Small-pox—for this practice seems to have been followed in China long before it was dreamed of at Constantinople—is curious:—

"Instead of introducing the virus directly into the system, by a slight incision, they accomplish their object in a circuitous and rather complicated way. The crust of a maturated pock is thoroughly dried, powdered, and rubbed into the mucous membrane of the nostril, or a piece of cotton, powdered with it, is stuffed into the nose. This is the most common method for the common people; but there is one of greater pretension, though probably less effectual, for the rich. A small metallic cup, shaped like the bowl of a tobacco-pipe, is introduced into the nostril of the child, while the inoculator, applying his mouth to the stem, blows the variolous contents forcibly against the lining membrane." P. 247.

The shops of the Chinese druggists seem to be not much unlike those of our own pharmacopolists. They are described as being large and commodiously fitted up with a great array of drawers and jars, arranged much in the same way as in England: glass vessels are rare. Different departments are allotted to separate classes of medicaments; care is taken to keep things in order; and there is a degree of neatness and method in their appearance, which would not be discreditable to a London laboratory. They do not seek notice by party-coloured bottles and cabalistic signs, which make so great a figure in the windows of English medicine venders, but are rigorously plain, and, as far as mere appearance is concerned, appropriate.

With the exception of camphor, rhubarb, and liquorice, there was scarcely anything in the shops that could be recognised. On the counters

is usually to be seen a number of boxes containing ready-prepared panaceas for curing cholera, imparting strength and vigour, exciting love, and so forth.

Here we must take leave of our author. His work is very interesting, and contains a great deal of instructive matter. The observations of so experienced a practitioner as Dr. Wilson are well deserving the attention of all medical men, and of our naval and military surgeons more especially. We trust that his example will be followed by some of his brethren in both departments of the Public Service. How full of interest would a medical history of the campaigns of our army in Cabool, for example, be to the profession at large!

After this article had been printed, we accidently met with the following strongly corroborative testimony by the late distinguished Editor of this Journal, in favour of Dr. Wilson's opinions on the important subject of deleterious miasms often generated from the bilge-water, and other impurities in the hold of a ship. In his review of Sir William Burnett's official report on the destructive fever on board H. M. S. Bann, in 1823 (vide *Medico-Chirurgical Review* for January, 1825,) Dr. Johnson has remarked:—

"It has now been well ascertained, from fatal experience, that while we are seeking for external sources of contagion, we have the enemy in our own bosom. Between the timbers and planks in the holds of vessels, miasmata have been generated that scattered destruction among the unsuspecting crews above. The Pyramus was a recent and melancholy example. To this source we are strongly inclined to attribute the fever in the Bann, aided by the atmospheric exposure and intemperance alluded to by the surgeon. It is not a little in favor of this local origin that the fever began forward in the ship, and came gradually to the after part, till nearly all the officers and men were affected."

The Pyramus frigate, alluded to in this extract, suffered dreadfully from fever in the West Indies. After all means had been tried to check its ravages on board, it was determined, at Antigua, to have all her stores and provisions taken out by negroes. Still the pestilence did not entirely cease. At length, the limber-boards in the hold were taken up, when such an offensive smell issued forth, as caused fainting in some on the spot, and several of the officers attending on this duty were immediately seized with the fever.

"The state of the hold, under the limber-boards, is compared by Staff-surgeon Hartle, to a bog of the most pestiferous nature on shore; and Mr. Comrie mentions that three or four of the slaves, employed in cleaning the ship, were attacked with the fever. I may here observe, that previous to the sailing of the Pyramus from England, she had had her magazine fitted on a new plan, and that the shavings and chips resulting therefrom, instead of being removed from the ship, were unfortunately suffered to remain in the lower part of the hold, and thus mixed with the bilge-water under the limber-boards.

"Dr. Fitzgerald states, that sometime previous to the appearance of the fever, the foul state of the hold was sufficiently indicated by smells of a very disagreeable nature issuing therefrom, and diffusing themselves over the ship; and that a candle, when placed at 'the mouth of the hold,' was immediately extinguished; and I was also informed by an officer of the ship whom I formerly knew, and whom I lately saw at the Royal Hospital at Haslar, that all the captains of the hold had lost their lives."

I. AN ESSAY ON THE TREATMENT OF COMPOUND AND COMPLICATED FRACTURES. By *W. T. Walker*, M. D. Octavo, pp. 100. Boston, U. S. 1845.

II. DE L'AMPUTATION DES MEMBRES PAR SUITE DES PLAIES D'ARMES-A-FEU. Par *J. Lisfranc.* (Bulletin Général de Thérapeutique, 1845.)

On Amputation in Gun-shot Wounds. By *J. Lisfranc.*

THE object of Dr. Walker's Essay is to prove "the powers of Nature in healing compound fractures attended with great contusion, laceration, and loss of substance of the bones, and of the skin, and other soft parts, to be much greater than is generally supposed." To this end he relates six cases which fell under his own notice, and quotes several others from the older surgical writers, whose precepts, he considers, have not been sufficiently attended to in modern times.

"The precepts to which I allude are—1. That all tendinous or membranous structures which obstruct the removal of foreign bodies, or unduly confine or strangulate the soft parts, when swollen by inflammation, should be promptly and freely divided. 2. That such dependent orifices should be preserved, or counter-openings made, as will, when aided by position and dressings, secure the free discharge of all fluids which might otherwise stagnate within the wound. 3. That portions of bone protruding through the integuments, which cannot, easily and without violence, be reduced to their proper place, should at once be removed by the saw; and that all foreign bodies, loose portions or shivers of bone, should be promptly extracted from the wound. 4. That great pain, inflammation, or nervous symptoms, depend rather on peculiar complication than on the extent of the wound. And that they indicate great danger unless rightly understood and appropriately treated and relieved."

The recognition of the vast extent of Nature's restorative powers is one of the just boasts of our actual surgery, and has caused a great diminution of that hasty lopping-off of limbs which not long since disgraced its votaries, both in this country and on the Continent. That this greater caution extends to cases involving bad fractures of the extremities is familiar to all conversant with the deliberation with which our hospital surgeons now weigh the possibilities of being able to dispense with an operation, justly deeming this one of the greatest triumphs of their art; and, indeed, as regards our own country, we are prepared to maintain that the whole history of surgery does not supply a period so characterized by an enlightened abstinence from officious intermeddling with Nature's work. The cases culled by Dr. Walker from the practice of Paré, Wiseman, De la Motte, and others, may easily find their parallels in that of many of our surgeons; they are indeed exceptive, and as a general rule amputation was far more frequently resorted to in such accidents at the epoch when these celebrated men lived than it is at present. However, we do not mean to state that we have yet reached the attainable limits, but it is quite certain there is a disposition to extend these as far as possible;

and Mr. Rutherford Alcock's papers* so recently published have shown us that a class of cases, bad wounds of joints, hitherto excluded, may oftentimes be placed in the category. It is, however, no such simple matter to determine upon the propriety of non-operating, as it would seem; for we have to decide not only that there is strong probability of life being saved, but also that an effective limb will be preserved. The constitution, condition of life, and means of effectually treating the patient, have to be considered quite as attentively as the nature and extent of injury he has undergone; for it would be but sorry humanity to attempt to save the limb of one whose powers were incompetent to the necessary restorative processes, or when it would never prove other than a painful incumbrance and impediment.

An important principle laid down by Dr. Walker is, that whatever interference the injured extremity may require should be resorted to at once, rather than delayed until the inflammatory process is set up. Indeed, where amputation itself is demanded, the author agrees with the great majority of surgeons of the present day, that it should be performed *immediately*, or at least within the first twenty-four hours. This too was the generally received doctrine in the profession until the French Academy of Surgery awarded its medal in 1754 to M. Faure, an army surgeon, who maintained that the operation should be deferred until after the establishment of suppuration. He stated, that of more than 300 persons operated upon immediately after the battle of Fontenoy thirty only escaped, while of 10 operated upon after the subsidence of the first symptoms, all recovered. Dr. Walker believes the opinions of Hunter upon this point, which were published some time afterwards, and which exerted so injurious an effect upon practice in this country, were based upon the authority of Faure. The experience of the last war completely exploded them as regarded military practice; but they long retained a hold upon practitioners in civil life. The fallacy of Faure's arguments were indeed exposed, but unsuccessfully, by M. Boucher, a cotemporary shortly after the award of the Academy had been made. He showed that Faure had reasoned from cases in which the performance of the operation had been undertaken after inflammatory action and fever were set up, whereas the true object of immediate amputation is to anticipate these. Moreover, it seems there was great lack of surgeons at Fontenoy, so that few of the sick could receive due care. Dr. Walker thus extends the application of the above remark:—

"If this be the explanation, and I doubt not that it is, then it is established, that in the days of Faure and of Hunter, as well as in our own, neither amputation, nor other operation of surgery, can be performed upon the human body while suffering under inflammation and fever, without incurring the greatest danger. * * * * If, then, amputation is not to be performed in a compound fracture, from the commencement of the second day after the injury until suppuration has been established, does it not follow, by a parity of reasoning, that we should avoid, during the same period, all acts which may promote or aggravate inflammation? Does it not also follow, that every thing possible

* Med.-Chir. Trans., vol. 23.

should be performed on the first day, and that every act which can, in any way, excite or aggravate inflammation or irritation should be studiously avoided during the intervening time preceding suppuration? Is it not clear that all membranous structures, which, in consequence of the injury, have become so many bands, impeding or even arresting the circulation of blood or the transmission of nervous influence, should be at once divided? Shall we, in compliance with the rules of our art, remove from our bandages both hem and seam, lest they irritate or cause ulceration of the skin, and at the same time suffer rough, angular, loose fragments of bone or other foreign bodies to remain within wounds to irritate, inflame, and endanger the vital structures they may lie in contact with? Shall we any longer talk about the necessity of cleanliness, and busy ourselves with water, soap, and sponge, in washing the external coverings of a broken limb, knowing full well at the time that this very limb is, within, full of all uncleanliness? Shall we recognize any longer as a principle of our art, that in such cases, it is Nature alone which has power, after weeks of suffering and danger, to relieve our patients, by removing the complication of their wounds, whence all their sufferings and danger come?"

Dr. Walker combats the opinion of Sir James Earle, that the serious symptoms of compound fracture are dependent upon the admission of air, an opinion founded upon the apparent analogy of the hectic and other fatal symptoms which supervene upon the opening of psoas abscesses, &c.

"These views are certainly not sustained by the cases collected and hereto annexed, as neither hectic, inflammation of a grave character, or severe pain, or nervous symptoms have occurred in either of them, where the wound was at once sufficiently open, drained of liquid discharges, and freed from all foreign bodies; while severe symptoms have attended some of the other cases, where these precautions have not been sufficiently regarded; yet here, on several occasions, the severe symptoms vanished as soon as the needful treatment was adopted. I will only add that I have, on many occasions, known compound fractures heal readily as by first intention; but this result has only been obtained in cases where the wound has been inflicted on the bones and muscles, without splintering or other important complication; while the severest symptoms have followed such fractures as have occurred in tendinous or membranous parts, where the bone was much splintered, and the skin but little broken. Most English and American surgeons, however, have adopted the views of Sir James Earle, and have endeavoured to convert compound into simple fractures, by closing the wound by means of adhesive plaster and bandage, in the hope of healing by the first intention. Now, although there may be but little objection to gently drawing the lips of the wound together by adhesive plaster, in the simpler kinds of compound fracture, I mean such as are compound by definition, not by complication, yet I apprehend much mischief may flow from the practice, if extended to the more complicated forms of fractures, especially when, in addition, graduated compresses and bandages are made use of to prevent the undue rising of the bone. Under such treatment I doubt not that inflammation and sloughing have often been attributed to the original injury, which would never have existed, had it not been for undue constriction over the wound."

The only other point we have to notice in Dr. Walker's interesting Essay, is the recommendation of early *dilatation of the wound for the extraction of foreign bodies*. All the celebrated older surgeons, such as Paré, Wiseman, Petit, Ledran, &c., recommended that, where the case is such as to require this procedure, it should be put into force at once. They maintained that dangerous symptoms arose, unless tension was removed, and foreign bodies, &c., were extracted prior to the setting up of inflam-

mation. This was quite consistent with their opinion of the preferableness of prompt to deferred amputation. Hunter, Pott, Abernethy, and most other surgeons, however, disapprove of considerable attempts at the removal of foreign bodies being made prior to the occurrence of suppuration, and offer no recommendation of early dilatation of the wound for draining it of its fluids or for the relief of tension; and even Mr. Guthrie,* the able advocate for early amputation, recommends delaying dilatation and the removal of fragments of bone, &c., until suppuration is established. This advice Dr. Walker considers both inconsistent and injudicious, and much prefers that delivered by the older authorities referred to. We believe, indeed, the tedious progress of many of these cases might be prevented by a somewhat more active and earlier interference than most modern surgeons seem disposed to employ; but, on the other hand, great additional irritation would necessarily be excited, and additional suffering caused, if that excessive probing and searching for foreign bodies put into force by the old surgeons were revived.

Connected with this subject, we are desirous of furnishing an abstract of a paper upon *Amputation in Gun-shot Wounds*, recently published by M. Lisfranc. It is gratifying to find a surgeon of his eminence and operating celebrity, expressing such *conservative* views upon this point, albeit he may hold others *destructive* enough upon some other subjects in surgery. Before adverting to those cases in which the bones are fractured, he observes that, when about two-thirds of the *muscular substance* at the upper portion of the thigh have been torn off by a ball, amputation should be resorted to, even although no very important vessels or nerves be injured. Cicatrization in such a case becomes tedious and difficult; and the cicatrix which is formed is thick, adherent, large, and easily torn; while the movements of the part after so large a loss of muscular substance become impaired or impossible. The dangers and unsatisfactory results of such cases always lead to regret when the operation has been declined. A similar wound, occupying the arm, by no means renders an operation so essential.

Some modern works have stated that wounds of *joints* are least dangerous when the ball only makes two small apertures while traversing them. So far is this from being the case, however, owing to the absence of due exit for the fluids which result from the accident, that M. Lisfranc has found the happiest effects follow the enlargement of the wound, as recommended by Larrey; and, providing the loss of substance be not very great; there is therefore less danger in a large than a narrow wound. "I have shown, in a great number of cases, that where the loss of substance is not very large, and appropriate after-treatment is pursued, bad wounds of the shoulder, elbow, wrist, knee, or ankle do not necessitate amputation." He makes the following statements respecting fractures:—

"It is generally believed that amputation is required when the body of a long bone of the lower extremity is fractured in a comminuted manner. I reject this precept. Lombard, Gaultier de Claubry, S Cooper, Percy, Larrey and Guthrie, all state they have seen but few such cases of fracture of the femur recover, almost all dying. Ribes did not obtain one cure out of 10 patients, upon whom

* Med.-Chir. Review, April, 1838, (Appendix.)

every care was bestowed; and among the 4000 old soldiers at the Invalides, he could not find one who had been thus cured of this accident; but it is to be observed these latter cases were treated in the army, where sufficient care cannot always be furnished or due transport obtained. It is said no better success has attended these cases in the Belgian and Paris Hospitals, but I suspect this arises from appropriate measures for the prevention and removal of inflammation not having been put into force. I believe, as a general rule, we should not resort to amputation in these cases before inflammation has declared itself and resisted an appropriate plan of treatment. This, independently of phlebitis and resorption, is all we have to fear in comminuted fracture. I must, however, admit that my opinion is as yet scarcely based upon a sufficient number of facts, and that I have chiefly founded it upon the analogy of what takes place in the upper extremities, in which comminuted fractures produced by fire-arms hardly ever give rise to fatal accidents. Dupuytren, however, cured a case of bad fracture of the femur. I have saved several, and not yet lost one. In 1830, I had two patients brought in, both of whom did well. In the first, there was a comminuted fracture of the femur, and in the other, there was great accompanying crushing of the soft parts. In both cases the *constitution was good and the intestinal canal in excellent condition*—important points for consideration in deciding whether amputation can be dispensed with.

"It has been said that such cases, even when cured, are almost always followed by fistulæ and *engorgements*, which impair the usefulness of the limb, and eventually induce death in a great number of instances. I do not admit this. I have retained my army connexions with certain officers, in whom such fistulous openings manifested themselves. It is true that swellings arising from the presence of pus are formed from time to time, which disappear when it is evacuated. These persons walk pretty well, are but little annoyed at the slight discharge which persists, and are in a far better state than if they had undergone amputation, which is not, be it recollected, unfrequently a fatal operation. The fistulæ may be complicated with considerable *engorgement* of the limb. Sometimes these are the results of the primary inflammation, and at other times they arise from neglected or badly treated phlegmasiæ, produced by the presence of pus in the sinuses. In this latter case, relief is obtained when the pus is evacuated; and in all cases powerful means of combating simple induration have been long known, such as careful local bleedings when there are phlegmasiæ, and resolvents when these are absent.

"Fractures complicated with solution of continuity of the soft parts, and produced by ordinary causes, are less dangerous than those resulting from fire-arms; and are also by far less so when affecting the upper than lower extremities. It is sometimes difficult enough to distinguish the circumstances under which the amputation should be determined upon or rejected. If the locality is a good one, the patient's constitution and digestive canal in good order, if his *morale* is good and his animal economy unaffected by any cachexia, we need rarely operate; for under these circumstances, success frequently surpasses all expectation. I am fond of saying to the young pupils who come to study in the hospitals of large cities, that they must distrust the observations they make there, and recollect that, under other circumstances, the removal of limbs should be much oftener rejected.

"When the principal vessels of the limb are divided, large ecchymoses or effusions of blood are present, the soft parts are largely torn or much bruised, or a large joint is opened, we must amputate; but we need not if the vessels have been saved, or the joint is not open to the air. When the fracture is comminutive, the effusion of blood large, and the muscles torn off to some extent, I have frequently applied my usual measures at La Pitié, and every one is aware of my success in saving these limbs, several of which had been crushed by the wheels of carriages. When the fragments have traversed the skin, and we are unable

to reduce them owing to the narrowness of the aperture, we should enlarge the incision, and remove any portion of bone which is denuded of periosteum.

"There is much difference of opinion among surgeons as to the propriety of amputation in these cases. I believe opinions would be much less divergent, and even all contest terminate, if medical knowledge as well as surgical were more generally diffused, and if preconceived and exaggerated ideas acquired at bad schools, did not so frequently serve as a guide to the practitioner. Read the accounts of cases that have been published, and you will find in the majority of these that true medical ideas have been completely neglected."

Throughout his paper M. Lisfranc frequently reiterates that, if limbs are to be saved from amputation, under the circumstances mentioned above, the patient must be treated upon the plan he has repeatedly made public.* This consists in the observance of a rigid diet, the employment of several small bleedings from the arm (discontinuing these when suppuration is about to occur,) and the frequent application of emollient cataplasms to the parts. We may observe also, that Mr. R. Alcock, in cases of wounds of the joints which he wishes to save from amputation, has recourse to active general and local bleeding, and the application of slightly stimulating fluids. When we consider that these accidents so frequently occur to persons in robust health, we certainly think that enough use of depletion is not, as a general rule, made by our surgeons, who, indeed, seldom resort to it at all.

RECUEIL DE MEMOIRES DE MEDICINE, DE CHIRURGIE, ET DE PHARMACIE MILITAIRES. Vol. 59. Paris, 1845. 8vo. pp. 415.

A Collection of Memoirs on Military Medicine, Surgery, and Pharmacy.

THE present volume of the "Recueil" contains several interesting papers, an analysis of some of which we proceed to lay before our readers.

I. ON THE TREATMENT OF SPRAINS. By Dr. *Poullain* of the Military Hospital at Lyon.

Sprain is an accident of very frequent occurrence among soldiers, especially those of them who belong to the cavalry. During the seven or eight years that Dr. Poullain has been attached to the 1st Regiment of Dragoons, he has had from 70 to 80 more or less serious cases under his care. The object of this paper is to detail the plan of treatment followed in these and other cases, which has been so successful that it has been very rarely found necessary to send the men into the hospital. The usual mode of treatment by the application of leeches and emollient cataplasms is not

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* See Med.-Chir. Review, Jan. 1843, p. 63.

only inefficacious, but even hurtful; for the hospital to which Dr. P. is attached being that to which the soldiers are sent for the benefit of the mineral waters (Eaux de Bagnoles,) he is in a condition to prove that, of every hundred patients sent there, from 25 to 30 are so on account of chronic articular swellings, consecutive to this mode of treating sprains, several of which eventually require amputation. He possesses notes of more than 103 such cases, exhibiting the uselessness or danger of the practice, a few of which are inserted in the present paper. Larrey and other celebrated surgeons have likewise protested against the mode of treatment. Professor Begin observes, in his Elements of Surgery :—

"Some persons having an exaggerated idea of inflammation, combat it in sprain by means of warm topical emollients, repeated local bleedings, and frequently-renewed applications. This plan seems to me defective. It does not allow the parts to remain absolutely at rest, it favours their tumefaction, *engorgement* and suppuration, while the recoveries it procures are tedious, purchased with risk, and less complete than those which follow the use of repercussives."

M. Baudens, senior surgeon of the military hospital of Val-de-Grâce, is likewise of precisely the same opinion.

"Leeches applied immediately after a sprain," he says, "induce an afflux of blood to the part which we should do all in our power to prevent. Cataplasms maintain this fluxion, and after the foot has been macerated 10 or 15 days in such an application, it remains pasty and deprived of its elasticity, and, when not rendered liable to a degenerescence, which not unfrequently leads to the necessity of amputation, the difficulty of walking and persisting *engorgement* at least manifest its debility."

The means which Dr. Poullain and the above authorities recommend in lieu of leeching and cataplasms is the *immediate and continued application of cold* by immersing the part in water. The cure is not only prompt but complete, inasmuch as there is no remaining *engorgement* to lay the foundation for future mischief, and the patient is enabled to employ the joint as actively as heretofore. This would be a great point gained even if the time consumed in the treatment were as great in the one plan as the other, which it is not. Many cases of its success are related in the paper, and although, of course, in the great majority of instances, the ankle is the joint affected, sprains of other joints may be treated in just the same manner, except that in those, such as the knee, in which immersion may be difficult, the application of wet compresses or irrigation may be substituted. The treatment indeed, is not novel, for it was recommended by Boyer, and more recently by M. Begin.

"Of 90 patients whom I have treated by the aid of cold and resolvents, 23 were cured in 6 days, 10 in 8 days, 22 in 11 days, 28 in from 11 to 15 days, 4 in from 20 to 25 days, and 3 at the end of a month. None of these persons have continued lame. Seven felt the effects of their accident for several months, without, however, being prevented attending to their duties, and becoming quite cured. * * * * * * * If this mode of treatment has incurred blame at the hands of some surgeons, it is because it has not been sufficiently promptly and freely employed, and it is therefore necessary to lay down some rules upon this point.

"The immersion should be resorted to as soon after the accident as possible. Recourse may be had to it also three, four, five, six, or even 12 hours after, but

then its sedative effect is less prompt and the cure more tedious. The foot should remain at least *two hours* in the bath, and oftentimes much longer. It may sometimes be left in for entire days; and as a general rule the part should not be removed until it becomes completely cooled, the water being renewed as often as it becomes warm. This prolongation is easily obtained, for, after the first hour or so (during which the pain is sometimes almost insupportable,) the immersion becomes bearable, and the patient is himself very desirous for its continuance. Iced water does not possess any superior efficacy to that of a temperature of 37° or 39°, provided this be equally maintained. As soon as the limb is removed from the bath it must be surrounded by a roller previously moistened with Goulard water or camphorated spirit, some of which must afterwards be applied to it from time to time. So effectually are congestion and swelling in this way diminished, that the bandage usually becomes loose within the 24 hours. It must be re-applied until all swelling and pain has disappeared, which is generally the case in from three to six days. The patient may now be allowed to walk, continuing however the use of a bandage for ten or twelve days.

"If 14, or even six or twelve, hours after the application of the wet bandage, pain still continues, or throbbing is felt by the patient, it must be taken off, and the limb again immersed in the water for a longer period than at first, even for a whole day if requisite. This second immersion is sometimes unsuccessful, but fortunately it is very rarely required, as the first almost always suffices.

"If the sprain is several days old, the limb swollen and painful while nothing has been done for it, a free local bleeding is a necessary preliminary, after which the bandage and cold lotions or perhaps immersion itself, should at once be resorted to. These means are, however, now of far less service than when employed soon after the occurrence of the accident."

When the sprain has been badly treated the joint may become the seat of a chronic enlargement, which is dissipated with difficulty, and only after the persevering use of compression. MM. Begin and Velpeau, indeed, employ this in the earliest stage of sprain as a powerful means of preventing inflammatory swelling. Dr. Poullain employs to this end a starched many-tailed bandage. Whatever means are used the case is tedious, and may also require the aid of stimulating liniments, or, if very obstinate, of the douche as employed at the mineral springs, and even this does not always dissipate the enlargement.

The Editors of the "Recueil," in a note to this communication, corroborate the statement of the efficacy of the treatment of sprain by immersion, provided this be uninterruptedly continued. They state, also, that M. Baudens of the Val-de-Grâce, so far from finding the first period of the immersion to be attended with pain as related by Dr. Poullain, has always observed marked relief from this symptom to occur at once. This practitioner employs this means with great success, not only in sprains, but also in every case of acute traumatic inflammation. Several cases are cited of the beneficial results following the continuous application of ice, cold lotions, &c., too, in cases of bad compound fracture, wounds of the joints, &c., &c.; general bleeding, however, being also generally resorted to in serious cases. In the treatment of sprain, M. Baudens has usually had to prolong the immersion for a longer period than Dr. B. has found requisite. He observes it occasionally happens that, during the immersion, the pain which had subsided is renewed with great severity. This is a sign of threatened congelation of the part, which must be removed from the bath, bathed in tepid poppy-water, and rolled with a starch bandage.

II. ON THE TREATMENT OF ORCHITIS. By *M. Songy*, Assistant Surgeon 3d Chasseurs in Africa.

M. Songy observes, practitioners have long recognized that, however distinctly inflammatory the affection of the testis may be at its origin, its results soon become modified by the nature of the tissues, and that, although sometimes the disease may be a pure inflammation of the terticular parenchyma, at others, it consists of an œdematous condition of the external coverings, or of an effusion into the tunica vaginalis. This view of the case led to modifications in treatment, and compression employed by Fricke of Hamburg, was also adopted by Velpeau and Ricord, and recommended by them as a speedy means of cure. Nevertheless, the majority of practitioners have continued to employ antiphlogistics for the relief of the early symptoms, and resolvents to dissipate the consecutive indurations, which are often-times so obstinate, and frequently form the germs of future attacks. Even supposing these means were more efficacious than they really are, the instances are numerous in both civil and military practice, in which their employment, in such an affection, is attended with inconvenience or annoyance.

"I do not say that any means which has been hitherto found useful should be abandoned, for each may have its value. My object is to introduce a mode of treatment which I believe has not been yet employed, and which I regard as very successful as well as easy of application. This new agent, which has never failed me during the four years I have employed it in our military hospitals, is *cotton*, or, where I have not been able to obtain this, as in some districts of Africa, sheep's wool. * * * * * * I consider that, in orchitis, we may have either an inflammation of the testis itself in which the coverings of the gland may participate, or a phlegmasia whether of the epididymis or the tunica vaginalis, with œdema of the scrotum and more or less extension to the gland; thus, distinguishing these, as does M. Vidal de Cassis, into *parenchymatous orchitis* or inflammation of the substance of the testis, and *membranous orchitis* embracing the varieties known by authors as vaginitis and epididymitis. Is this division of practical importance? I cannot doubt it, and authors have tacitly admitted it to be so. They have described orchitis which required little else than hygeienic treatment, viz., the membranous form; but they have also described orchitis with violent pain and symptomatic effects, such as hiccough, vomiting, convulsions, fever, &c., which all agree arise from the propagation of the inflammation to the substance of the gland, requiring the most active treatment. These cases are exceptional, and we have only to inquire whether antiphlogistics are requisite for the cure of the membranous form which constitutes the great majority of cases, or whether mere hygeienic precautions will not suffice. With few exceptions physicians and surgeons at all periods have prescribed *absolute rest of the organ affected* as an indispensable condition of cure. But we must not confound this with the absolute *rest of the body*, by keeping in bed, which patients are ordered to observe at the commencement of the affection, for they are two different things."

M. Songy then points out the inefficiency of the means usually employed to secure this absolute rest of the organ, viz., the constant application of cataplasms or the strapping up the testis. As soon as the cataplasm is cold and dry, it may become a source of irritation to the part, and, ceasing to be accurately applied to the surface of the organ, it admits

of injurious motion and friction. These objections do not apply to the same extent to strapping the testis. But first the preliminary removal of the hairs of the scrotum is a painful and difficult operation, and their reproduction will yet after a while necessitate the re-application of the plaster, which cannot be removed without exciting an injurious traction. The constriction of the cord by the plaster also may give rise to much pain; while, when the tumour has diminished somewhat, and before the plaster is renewed, an injurious mobility is allowed to the testis, described by the patients as "the nut rattling in its shell."

"It may be objected to me that I insist too much upon the necessity of the absolute repose of the organ in an affection generally regarded as a slight one; but, in external as in internal pathology, nothing should be neglected. To the mildest symptoms sometimes have succeeded diseases almost beyond the reach of art. And, then, is chronic orchitis so insignificant an affection, that the practitioner can venture to neglect considerations mistakenly represented as too minute? Will, then, cotton or wool more effectually maintain the organ in a state of absolute rest, without preventing the patient following his ordinary occupations? Once applied, as it undergoes no change in position, it need never be renewed, and even the diminution of the tumour does not exact this, inasmuch as, by its natural suppleness and elasticity, the cotton always as it were moulds itself around the parts which it envelops. Moreover, these same properties, by deadening any inconsiderate movements of the patients, prevent the effects of pressure or friction of the parts so injurious in an affection in which the local sensibility is so exalted that the slightest touch induces excessive pain. When placed in contact with the scrotum, the cotton at once produces a sense of comfort soon followed by a mild and moist warmth, the cutaneous transpiration being sensibly increased. * * * *

But to obtain the advantageous results which cotton has furnished us with for several years some precautions are indispensable in its application.

"After having chosen a suspender which is perfectly adapted to the parts it is destined to sustain, and fastened it around the body, the surgeon surrounds the scrotum over its whole extent from the perineum to the root of the penis with a layer of carded cotton (or of thick wool so prepared as to present the tomentous aspect of cotton) of about an inch in thickness. It is then to be placed in the suspender, but at this period, if the success of the case is to be secured, the practitioner must gently raise the tumour in such a manner that the testis may become applied as near as possible to the ring, in which position it is to be maintained. Thus placed, the tumour, which from vertical has become horizontal, will not exert any influence upon the *engorgement* by its weight, and the testis, thus bolstered up, is scarcely brought into the horizontal position before the pain diminishes, and ere long disappears."

Some cases are added exemplifying the rapidity with which relief from suffering is obtained, and the ease with which patients so treated can avoid the inconveniences of confinement.

III. ON THE USE OF THE SUTURE IN WOUNDS OF THE NECK. By Professor *Bertherand*, of the Strasbourg Military Hospital.

Most surgeons have objected to the use of the suture except in superficial wounds of the neck, and Dieffenbach rejects it altogether. He observes that, owing to the loose and extensible condition of the subcutaneous cellular tissue of this part, and the consequent facility with which

the skin glides over the subjacent layers, it results that any approximation of the edges of the wound is but superficial, the deeper-seated parts remaining separated. Between these, cloaca are established, pus accumulates and degenerates, and more or less extensive burrowing and other accidents ultimately result. But it is to be observed, that the suture is not the cause of these, but the deceptive security of the superficial union of the parts. A better-founded objection to the suture is that, when it only implicates the skin, the mobility of this, already adverted to, may lead to irritation and ulceration of the apertures, and a clean section be thus converted into an irregular wound of tedious cicatrization.

During the author's attendance upon the *ambulances* in Africa he had frequent occasion to observe the insufficiency of these superficial re-unions, and of the various contrivances put into force to keep the parts in contact. It seemed to him possible, by passing the suture through the deeper-seated parts, to substitute for this great mobility of the skin so incompatible with firm adhesion, very limited movements of the entire parts in their relative positions. A case soon offered itself for putting the idea into practice. A soldier was brought to the *ambulance*, having a wound on the right side of the neck, eight or ten fingers' breadth in length. The sterno-cleido-mastoideus was divided through, but the carotid had escaped, and was observed pulsating in the anterior angle of the wound. Behind, the trapezius was transversely divided to the extent of about an inch and a half. Several large coagula were washed out of the wound, and two small branches of the superior thyroid submitted to torsion. A long curved needle, carrying a double thread, was passed through as great a thickness of the muscular substance as possible, and its point brought out about an inch from the edge of the wound. A quilled suture, penetrating thus deeply, was formed at each extremity of the wound, and two others, more superficial, brought the intermediate parts into contact. Adhesive strips were placed between these, and the whole covered with a "solid dressing." The head was kept bent to the right side, and the greatest quietude enjoined. Three of the sutures were removed on the 6th day, and the remaining one on the 9th. Adhesion went on satisfactorily; and, on the 20th day, solid food was given the patient, he being considered perfectly cured.

Another case is related which, however, did not come under Dr. Bertherand's care until the sutures had already been inserted. A soldier attempted to cut his throat, and inflicted a large incision upon it, a small fragment of the thyroid cartilage being observed to be floating in the wound. Several coagula filled the wound (which had been already strapped up,) and prevented the issue of air by the aperture. The voice was scarcely affected. The edges were united by two sutures and dressed. The patient was bled, and again in the evening, as he was seized with violent delirium. Afterwards he became calm and dejected, and determined to abstain from all food whatever, and would reply to no questions. Dr. B. regarded his self-imposed immovability of parts as highly useful, especially as his former violence had not disturbed the adhesive process. At the end of a week the sutures were removed, and, as two days afterwards the patient still continued obstinate, Dr. B. said aloud, near his bed, that as he was quite well he should order his dismissal, at the same time

that he prescribed for him a better diet. He yielded to this, took food, recovered his strength gradually, and was dismissed the 20th day after the accident—fasting for *eleven days* having been thus borne by one whose digestive organs possessed all their activity.

Dr. Bertherand does not mean to state that sutures are applicable to every wound of the neck. As a general rule, the regularity of the section is of more consequence than the superficiality of the wound. Irregular wounds, with loss of substance, excavations, rough walls, or communicating with the œsophagus, do not admit of the suture. The editor of the "Recueil" observes, that M. Begin employs sutures in some forms of wounds of the throat, and that the necessity of embracing a great thickness of the lips of the wound in the ligatures has been long acknowledged by M. Baudens in his treatment of the wounds which result from the extirpation of cervical ganglions. As a general rule, he has found wounds so treated unite by the first intention, and the patients have been cured in from 8 to 10 days.

IV. REPORT OF SEVERAL CASES OF CEREBRO-SPINAL MENINGITIS WHICH OCCURRED AT DOUÉRA (ALGERIA) IN 1845. By Dr. *Magail.*

Cerebro-spinal meningitis prevailed epidemically in various garrisons of France from 1837 to 1842; and the Report of Dr. Magail exhibits the disease identical in all its characters, but far less extensive in its prevalence, in Africa. Between the 4th and 17th of February, 10 soldiers were attacked, when the epidemic ceased, to re-appear again on the 21st March, between which and the 26th April twelve new cases occurred. The subjects of the first epidemic were robust, healthy men, of from 21 to 30 years of age, all of whom had been in the service for one or more years. Four of them were drunkards, two nostalgic, and one very low-spirited. In explaining various epidemics, much stress has usually been laid upon the bad lodgement and over-crowding of troops, their excessive fatigue, the bad condition of the barracks, and the dissipated conduct of the soldiers. All such causes should rather operate upon the feeble and debilitated than upon the robust, while the condition of the soldiers at Douéra, in respect to air, dress, food, and exercise, is excellent. The invasion of the disease was always sudden, coming on in cold, damp weather, with violent cerebro-spinal pain, delirium, and other symptoms of excitement, and passing rapidly into a state of collapse. The pulse was however small and soft, and ranged from 106 to 114; vomiting and obstinate constipation were always present, while the urine was normal as to quantity, but gave out a very distinct odour of "bat's." The penis was sometimes erect, some patients presented a more or less complete trismus, and others subsultus. Every patient laid exclusively upon his side, usually the left, with the extremities bent up, and the head drawn too much forward or backward, the muscles of the neck and extremities being more or less rigid. Blood drawn from a vein was always found destitute of buffiness, in and possessing but a small portion of serum. The collapse came on from 12 to 48 hours. The prognosis was very unfavorable, recovery never occurring after the period of collapse had commenced. Of the ten patients

six died in from 8 to 23 hours after the collapse had commenced—this generally occupying a third of the whole duration of the disease. Some of those patients died within 21 and 23 hours, and the longest lived only 72 hours after the commencement of the attack, and yet, in all, distinct *suppuration* was discovered. The dura mater, sinuses, and veins of the surface of the brain were found gorged with blood; but the pus presented great varieties as to its situation, consistency, and quantity, the base of the brain being the spot it was oftenest found at. It not only existed at various other parts of the exterior of the brain, but also in the cellular tissue connecting the arachnoid and pia-mater. The ventricles were filled with a lactescent or sanguinolent serosity. Pus was found in the spine only in three subjects, and in these resembled a band covering the posterior portion of the marrow from its origin to the cauda equina. The spinal marrow was however often found softened, even when the condition of the substance of the brain in this respect was normal, which it usually was.

The *treatment* of the disease, to be of any avail, must be vigorously active. General bleeding to syncope is necessary, although for the production of this sometimes the loss of from 40 to 120 oz. was required. While the blood was flowing from the arm, hundreds of leeches were placed upon the head, neck, spine, and anus, and cupping-glasses applied to various parts. Hot water was applied to the legs and thighs, cold compresses to the head, and active enemata administered. Shortly after reaction was endeavoured to be produced by active friction and sinapisms applied to all the extremities. When this seemed of no avail, the actual cautery was applied at various points along the spine and blisters inside the thighs. Quinine was tried with no benefit, and tartar-emetic with some apparently good effect. Croton oil, in doses of three or four drops during the 24 hours, seemed also of some advantage, when it induced copious stools.

The *second* epidemic was somewhat less violent, and only attacked young soldiers who had just joined the regiment, the invasion of the disease taking place, as in the other, constantly at night. In these cases general bleeding to syncope was resorted to as early as possible, and a nasal hæmorrhage maintained by leeches applied to the nares. This latter was continued as long as the symptoms of excitement lasted, *i. e.*, during 48 or 64 hours; free purging by croton oil being also advantageously maintained during the same period. Sinapisms were applied to the extremities, and embrocations to the spinal column. In the present epidemic, however, M. Magail abstained from making any active application, such as leeches, blisters, moxas, cautery, &c., to the spine itself, "experience having too strongly proved to me that to act in this matter on the spinal column is only to augment the mischief by favouring suppuration, and thus leading to a fatal termination." Ten out of the twelve cases of this epidemic were cured, although in eight of them symptoms sufficiently alarming had manifested themselves. The patients who died did not apply for treatment until the disease had much advanced.

M. Guyon, in a note to this communication, observes that a similar epidemic was observed at Douéra, in 1840, when of 13 soldiers attacked only

one survived. Other cases have been observed in other parts of Algeria, but he is unacquainted with the particulars of these.

V. ON PHTHISIS AMONG SOLDIERS. By M. *Godelier*, Professor of Internal Pathology at Strasburg.

This paper, for which its author obtained a gold medal in 1844, was prepared in answer to the following question, proposed for discussion by the Council of Health for the Army—"*What are the causes of the frequent development of phthisis among soldiers, and the most efficacious means of preventing and treating this disease?*"

M. Godelier first endeavours to ascertain in what proportion the deaths of phthisis occur in military life, compared with those occurring in civil life: but he soon finds himself at a loss from the non-existence of any statistical documents which can elucidate the point. He is indeed obliged to have recourse to the Registrar's and Army Reports of this country, and assume that a like proportion prevails in France. We have so continually had dinned into our ears the excellence of the continental arrangements for internal administration and the collection of facts, that it is not unpleasing to find our superiority in this respect cheerfully acknowledged by every writer who has occasion to search into these matters for himself. It is true that, in some of the more despotic States, much information is accumulated in the various *bureaux*, but only for the use of the government authorities themselves, and even in enlightened France itself there is a great backwardness in publishing many documents which yet might prove very useful. The Reports of the Registrar-General and Poor-Law Commissioners, and those of Messrs. Tulloch, Wilson, and Chadwick, diffused far and wide, are spreading useful information among all nations, at the same time that they do honour to that one whence they emanate. We need not follow M. Godelier through his calculation, the conclusions to be drawn from which he acknowledges to be only approximative. He says:—

"From an attentive examination of these documents, it results—1. That of 10,000 soldiers, about from 50 to 60 die of phthisis. 2. This figure being somewhat lower than that which attaches to the males of the civil population of the same age, (between 20 and 30,) leads to the presumption that the soldier is less exposed to pulmonary tubercularization than these. 3. The difference being but slight, and the data furnishing it not presenting all the certainty desirable, it is allowable to suppose that it might be altered by an examination of additional facts. 4. It would be unlogical to assume a doubtful result, and search at present for the causes which render the soldier less liable to phthisis than the civilian—a state of things not yet sufficiently demonstrated, and one which, if true, may be explained by the fact of a soldier being a picked man from the mass of the population."

We have adverted, in our article upon Dr. Thurnam's "Statistics of Insanity," in the present Number, to some *statistical errors* which have obtained currency, and M. Godelier signalizes others. Speaking of an article on the Mortality of the French Infantry in Vol. X. of the *Annales d'Hygeiène*, he observes:—

"This is the only work in France, relating to the army, where the amount of

deaths is compared with that of the effective force. Such a condition is indispensable for the establishment of any true conclusion, and yet it is so generally neglected, that I cannot forbear pointing out some of the errors which result. It is customary to be contented with comparing, in statistical tables, the amount of deaths furnished by a disease with the total number of persons attacked by it, or with the entire mortality. Thus, of 100 cases of pneumonia, 5 died; in 100 deaths, 20 arose from phthisis. The first of these comparisons may serve to show the mortality of a given affection under particular circumstances. The second professes to indicate the part which a disease plays in the general mortality, or sometimes the *frequency of the disease*. I maintain that it is a grave error, to take the relation which deaths from phthisis bears to the other deaths as a measure of its frequency; for the total of deaths being incessantly variable, its elevation or descent may change in a contrary direction the value of the relation, although the number of deaths from phthisis continue the same.

"Suppose, *e. g.*, three corps, in the 1st of which the proportion of the mortality from phthisis to the general mortality is $\frac{1}{10}$, in the second $\frac{1}{30}$, and in the 3d $\frac{1}{50}$, it would be concluded that phthisis had committed by far the greatest ravages on the first of these, while nothing would be less certain than that it had; for, if the effective force of each corps had been the same, and each had lost the same number of consumptive soldiers, their equality in this point is obvious—the difference consisting only in the augmentation of the number of the other deaths. Thus:—

	Effective Force.	Total Deaths.	Deaths from Phthisis.	Proportion.
1st	Corps of 20,000	100	10	$\frac{1}{10}$
2d	Corps of 20,000	200	10	$\frac{1}{30}$
3d	Corps of 20,000	300	10	$\frac{1}{50}$

"This is, however, the error which disfigures and renders sterile the majority of this kind of statistical documents. It has diminished the value of the labours of Benoiston de Chateauneuf, and Lombard of Geneva, upon the influence which professions exert in the production of phthisis. They have accurately counted the number of deaths which it had caused in each of these; but since they were unable to compare them with the unknown total of individuals employed in each profession, how could they draw conclusions as to the relative frequency of the disease? Thus, when M. Benoiston de Chateauneuf confines himself to stating that, of 17,209 deaths in the French infantry, 1,260 were due to phthisis, ($\frac{1}{14}$ or 0·074,) the information is of little importance: but if I learn that these 1,260 deaths took place in 801,748 soldiers, ($\frac{1}{636}$ or 0·0016,) I acquire by this fixed term of comparison a relation of great value, which is of service as an approximate measure. Statistic results, from whatever authority they emanate, require to be controlled by new results before they can obtain the force of laws. All the sagacity of the statistician is often powerless for the correction of the defective elements of his calculations. New series of facts are requisite to palliate or correct his errors."

M. Godelier enters into a long examination of the various circumstances in the condition of the soldier which may predispose him to the disease, and thus sums up:—1. A residence in Africa appears to be favourable to European troops, while one in the West Indies is quite the reverse. 2. The influence of birth-place is not ascertained. 3. The officer is much less liable to phthisis than the private, and he owes his comparative immunity much more to the influence of hygeiene than to that of age. 4. Those branches of the service are least exposed to the disease in which are the strongest constituted men, and which adopt the most regular system of exercises, and the best hygeiene. 5. All things being equal, the constancy, regularity, and variety of exercises contribute most to exemption.

6. Bad conditions of the air, food, lodging, and exercise exert an acknowledged influence in producing phthisis, and as these conditions are in some measure inherent to the soldier's life, they must exert the same effect upon him. And thus the vitiation of the air in his crowded sleeping-rooms, the too great uniformity of his diet, the damp and coldness of some barracks, and insufficient or injurious exercises, all tend to this end. 7. The existence of chronic disease, necessitating a long sojourn in the hospital, predisposes to phthisis. 8. The mode of life the soldier leads exposes him to numerous occasional causes which may act on the predisposed.

The chapter on *prophylaxis* enforces the necessity of the proceedings which the above conclusions obviously indicate; and M. G. also suggests the propriety of allowing valetudinarians an opportunity of re-enforcing their health by a visit to Algeria, the climate of which seems so adverse to the production of tubercle. In regard to the *treatment*, the author speaks of the existence of the power of detecting the early stages of the disease by a combined observation of the physical, local, and general signs, with a positiveness which, unfortunately, facts do not warrant; and he also promises a success from the adoption of general hygeienic precautions and local antiphlogistics, in a far more sanguine manner than we should have expected in so cautious an observer. However, this little avails the soldier, since M. Godelier admits that early or anticipatory treatment is incompatible with the exigencies of the service; and recommends that whenever incipient phthisis is detected, he may be allowed his discharge.

MILITARY MISCELLANY, COMPREHENDING A HISTORY OF THE RECRUITING OF THE ARMY, MILITARY PUNISHMENTS, &c. By *Henry Marshall*, F.R.S.E., Deputy Inspector-General of Army Hospitals, &c. Octavo, pp. 375. London, 1846. Murray.

IN a previous article in our present number, we have alluded to the great and most desirable reform that has, of late years, been effected in the treatment of the insane. Thanks be to God, men's eyes are now opened to the folly as well as the sin of punishing the worst of human misfortunes as a crime, and of attempting to subdue or correct the diseases of the mind by the infliction of suffering upon the body. Language like this may be considered as putting a too charitable construction on the motives of those who could act so; and truly we must confess that, in the majority of instances, it was cruel selfishness rather than want of knowledge, wilful insensibility rather than prejudice or error, in short, the evil passions of man's heart much more than the ignorance of man's head, that were the real causes of those terrible and most revolting enormities that used to be—would that they had utterly ceased to exist in the present day!—perpetrated within the walls of lunatic asylums. How humiliating a reflection it is that human beings, when left to themselves, unawed by the disgrace of detection or the fear of punishment, should treat their fellow-creatures worse than the very beasts beneath their feet! The vilest of mankind

would blush to chain up a helpless brute in a dark damp cell, and there to leave it to wallow in its own mire, without clothing or sufficient food, but with no lack of blows, often and mercilessly inflicted. Yet such was actually the treatment that was too frequently the portion of him that is fashioned in his Creator's image, and made instinct with immortality. The faculties of the poor maniac's mind may indeed be unhinged and perverted, but it would seem to have been forgotten that still the mind is there; and, for ought that we know, the Almighty himself may condescend to hold intercourse with that very spirit, which man, in his impiety, supposes to be wholly eclipsed. It is only, we believe, by taking this high view of the subject, and by contemplating the relationship that subsists between all members of the human family to each other, and to Him who hath made and who knoweth all things, that we can ever hope to obtain a safe and unerring guide in our efforts to correct many of the worst evils of our social system. The science of political ethics has too often set all principles of justice, not to talk of mercy, at defiance. There is no want of instances to prove the truth of this assertion. Take, for example, the history of the criminal code in this or in any other nation of the earth. What, it may be fairly asked, has been the great defect in all legislative enactments upon this most important subject? Is it not the case that the only object and aim have uniformly been to deter by fear, on the one hand, and inflict condign punishment upon the offender on the other? Have the laws ever sought to prevent the commission of crime by duly instructing the people in the knowledge of their duties, and by encouraging them in their exercise? or have they, while they awarded pains and penalties for their infraction, simultaneously endeavoured to correct the evil principles or enlighten the ignorance that led to that infraction? Hitherto little, very little, has been done, or even sought to be done, in either of these ways. A criminal is detected, apprehended, thrust into the company of those more hardened in vice than himself, tried, convicted, and punished—let us suppose by imprisonment. What is the ordinary result? he comes out more depraved and corrupted than before; none will employ or trust him; he cannot starve; he is forced almost by necessity to the commission of a new crime; and thus things go on from bad to worse, until, perhaps, his life is at length forfeited to the laws, or he is driven a disgraced outcast to a penal land. Now something very similar to all this has been, in one respect at least, the condition of our soldiers and sailors; and the plan of treatment, that has been pursued towards them, has been, —until within the last 30 years or so—founded upon equally enlightened and humane principles. A man was found guilty of some (it might be very petty) offence, and forthwith he was ordered to have 100 lashes. The same or a similar offence was repeated; 300 was the number now assigned. A third time, the effect of 500 was now tried; but still without permanent or decided amendment of the offender. Well, what then? Did officers begin to perceive the inefficacy of their invariable panacea? Oh, no; the dose, it was said, had not been sufficient; and forthwith 1000, 1200, nay 1500 lashes were ordered, and inflicted too, upon the wretched criminal. On reading of such cases, the question naturally occurs to one's mind, was this corporal punishment at all likely to correct any vicious habits; to make the drunkard sober, the thief honest, or the

coward brave? What does the testimony of military men themselves say upon this head? All, without exception, who have written or given public evidence upon the subject—whether they be abolitionists, or not—agree on this one point, that flogging never made a good out of a bad soldier. Nor can we wonder at it. A human creature, when treated like a brute, becomes gradually hardened and indifferent as to himself; he loses all self-respect; and *therefore* he is utterly regardless of what others think or say of him. This is a law of man's moral nature; who can gainsay its truth? You may torture his body, you may load him with fetters, and shut him out from the face of day; you may crush his very soul with injuries; but you cannot, by mere force, extract the poison-fang of vice that is rooted in his breast. How, then, is this to be effected? We shall answer the question by a short narrative. A man had been over and over again flogged; but this repeated punishment seemed to have really no effect at all in deterring him from disobedience. At length, "the commanding officer," says our author, "observing that, notwithstanding all his vices, he had some very valuable qualifications, resolved to try another mode than whipping. It was not long before he had an opportunity of putting his scheme into execution; for the next fault, instead of being punished, to the fellow's great surprise, he appointed him sergeant! This opened his eyes, he applied himself diligently to his duty, and became as remarkably sober and good as he had been the contrary before."

Sir Neil Douglas, the gallant colonel of the 79th, understood well the art of managing his men; for he tells us:—

"To this mode of punishment [flogging] I have been a great enemy, having found from long experience that it is hopeless to expect any good results from its infliction, either as a *warning to good men to avoid evil courses, or as a punishment to bad ones* for the actual commission of crime. I therefore gave much attention to the subject, and came to the conclusion, after mature reflection, that under the existing system of military law of England, my only chance of abating it, was by stimulating men's minds by holding out other great advantages for good and regular behaviour, and thus making it their interest to conduct themselves with propriety.' For this purpose Sir Neil established a regimental order of merit in the corps he commanded, the 79th Regiment of Highlanders, 'which,' he says, 'tended more than any measure I ever knew or heard of, to encourage good conduct, and to repress vice and immorality of every description.' Sir Neil Douglas commanded the 79th Regiment for upwards of twenty-two years." P. 215.

It is a remarkable fact, and certainly one that is not very creditable to the intelligence or the good feeling of military officers in general, that of the many (214,) who were examined before the Parliamentary Committee, appointed about ten years ago to report on military punishments, *two* only recommended any thing but physical penalties for the prevention of crime. In answer to the following query—*Are you enabled to suggest any means of restraining or eradicating the propensity to drunkenness, so prevalent among the soldiers, and confessedly the parent of the majority of military crimes?*—a great variety of penal enactments were advised; but no one suggested the schoolmaster's drill, but Sir George Arthur and the late Col. Oglander. The words of the latter breathe as much wisdom as benevolence: "The only effectual corrective of this, as of every other vice, is

a sound and rational sense of religion. This is the only true foundation of moral discipline. The establishment of libraries, and the system of *adult* schools, would be useful in this view." P. 317.

It would be pleasing to follow out this more attractive part of our subject; but this is impossible; we must quit it for the present with this passing notice. What we have now to do with is the physical, rather than the mental, operation of Military punishments. And here it must be confessed, that the whole history of them is a sad and humbling record of cruelty, folly, and wickedness. Those, who may wish to became acquainted with details, will find much instructive matter in Dr. Marshall's narrative. Without alluding to the various modes in which capital punishment used to be inflicted, we shall do little more than merely enumerate some of the most approved of the secondary or minor punishments, many of which, be it remembered, have been in use until the last quarter of a century. These are the *strappado* or *estrapade*, hanging up by the thumbs, riding the wooden horse, lying neck and heels, picketing, running the gauntlet, cold burning, booting, the log, the stocks, the whirligig, slitting the nose, various sorts of mutilations, branding, imprisonment, &c. The *strappado* is thus described: "The delinquent is hoisted up by means of a rope fastened to the arms behind his back, and then suddenly dropped down with a jerk, by which process his shoulder-joints were sometimes dislocated. He was sometimes hoisted up, and again let fall two or three times."

"Picketing" was equally cruel and absurd:—

"The instrument here (Bombay, 1816) employed, consisted of a board, and a peg; the board, or block of wood, was about 12 inches long, eight broad and four thick. The peg, which tapered to a point about the size of a sixpence; was twelve inches in length, and inserted in the middle of the board. The punishment was inflicted as above described, the delinquent's right arm being fixed to a hook, and his left foot resting on the peg, while his left arm and right foot were tied together behind his back. Delinquents were sometimes kept on the peg for a period varying from ten to thirty minutes; and occasionally they fainted either while on the peg, or after they had been taken down." P. 154.

In former times, boring the tongue with a red-hot iron was a common penalty for blasphemy and swearing; the offending organ being made the suffering one, according to the strict principles of the *lex talionis*.

Flogging does not appear to have been used, as a regular form of punishment, in the British army for more than a century, if indeed so long; although manual correction had been doubtless well known at all times. During the rebellion in Scotland (1745) we read of the *cat* being repeatedly used to extort evidence as well as to punish the soldiers. Previously to this period, it would seem that rods were generally employed, and executioners hired for the purpose of inflicting the punishment. The *Fustuarium*, bastinado, or stick-beating, was a common punishment in the Roman armies.

For upwards of sixty years after the introduction of the *cat*, the mere arbitrary will of the commanding-officer appears to have been a sufficient warrant for its use upon a soldier's back; no court-martial was necessary. When our readers learn that it has been only since 1811, that captains of our ships of war have been required to forward to the Admiralty a regular report of the punishments inflicted by their command, they will cease to wonder at the extent of the enormities that were often perpetrated on

board ship. It was not much better on shore; save and except that no soldier could be flogged without (the form at least of) a trial. But this was often a mere mockery; a set of junior officers did as their commander bade them: and forthwith the soldier or soldiers were brought to the halberts. One of the most flagrant instances of the extreme lengths, to which unrestrained authority was apt to be carried in the infliction of punishment, is recorded in Sergeant Teesdale's *Letter addressed to the people of England*, 1835. In October, 1806, the 28th regiment embarked from the Cove of Cork for Germany. At Bremen, where they were stationed for some time, there was a parade every morning and afternoon, and scarcely a day passed without a flogging taking place for some frivolous offence or another. Sometimes 10, 15, and 25 men were flogged, one after the other.

"At one of the above flogging parades, when we had been nearly two hours witnessing the horrible scene of bloodshed, and when the hands and feet of every soldier in the regiment were benumbed from cold, and from remaining for such a length of time in one position; I say, at one of these parades, a brave old soldier, whose character was unimpeachable, happened to cough in the rank. He turned his head a little on one side to discharge the phlegm, and was instantly ordered into the centre of the square, stripped of his accoutrements, and placed in front of the halberts. He went through the mock form of a trial, by a drum-head court-martial. Major B—— swore he was unsteady in the ranks; and on the *ipse dixit* of that tyrant he was sentenced to receive fifty lashes. After the brave veteran was tied up he implored hard for mercy, adding, that he had been twenty years in the service, and was never till then brought to the halberts. The pale, worn, and dejected appearance of this man, from age and length of service, was in itself sufficient to excite compassion and sympathy, even had he been guilty of a crime; his appeal was useless, he had every lash of his sentence, weeping and crying bitterly during the infliction; and although he only received fifty lashes, he never looked up afterwards. It had wounded his best feelings; he was constantly in hospital, and but a little time elapsed before he was discharged." P. 171.

Lieutenant Shipp tells us that, when he was at Jersey in the year 1808, not a week passed over without some flogging exhibition. Several men in the 60th regiment, quartered there, were condemned to receive 1000 lashes for desertion. "This punishment," says he, "was rigidly inflicted, with the additional torture which must have resulted from the number of *five* being slowly counted between each lash; consequently the space of three hours and twenty minutes was occupied in inflicting the total punishment, as though 1000 lashes were not of themselves a sufficiently awful sentence without so cruel and unnecesary a prolongation of misery. Many of these poor creatures fainted several times, but having been restored to their senses by medicinal application, the moment they could move their heads the castigation was recommenced in all its rigour. Numbers of them were taken down and carried from the square in a state of utter insensibility. The spectacle, altogether, instead of operating as an example to others, created disgust and abhorrence in the breast of every soldier present who was worthy of the name of man."

Major Macnamara states that it is scarcely an exaggeration to say that, during the war, at least one-half, if not three-fourths, of the soldiers of almost every regiment in the service had felt "the sting" of the lash. The mean number of lashes inflicted monthly in a regiment, then serving

in India, was for some time 17,000! Strange to say, the reasonings of the West India planters, in justification of their own barbarous conduct to their slaves, contributed—in a somewhat unexpected way, it must be acknowledged—to open men's eyes to the impolicy, not to say the iniquity, of such wholesale use of the lash in our military and naval services. The argument, as urged by "a native of Jamaica," who published a vindication of the treatment of the poor slaves, is so curious, and withal so cogent, that we must give it in his own words:—

"In Europe, among free men, and by a court of free men, a seaman and a soldier are sometimes sentenced to receive 100 to 1000 lashes—*men who have fought their battles and protected their liberty*. A master in the West Indies cannot, without answering to the laws for it, nor can a magistrate, by the settled laws of the country, give, or sentence a slave to receive, more at one infliction than forty lashes. Would not an idiot perceive on which side the *humanity lies?*" P. 183.

In 1812, by an order from the Horse Guards, no *regimental* court-martial could inflict more than 300 lashes at a time upon any offender.* Much credit is due to the late Duke of York for having taken this initiatory step in meliorating the punishment of soldiers. Some officers of the old, unlimited flogging school, thought that this order would destroy the discipline of the army! Dr. Marshall amusingly tells us of one who swore that he would never comply with it; "for," said he, "my conscience would not allow me to award a sentence of 300 lashes when I felt convinced that a man deserved 600."

For many years after the above-named period, much, very much, remained to be done to make the penal code of the army at all consistent with any principles of humanity or justice. Among other enormities, the inhuman practice of inflicting the second—nay, sometimes the third, and even the fourth—part of a sentence upon a man, who had been declared by the surgeon incapable of receiving the whole amount of lashes at once, continued to prevail for many years afterwards; and this too, it deserves to be remarked, notwithstanding the recorded opinion of Mr. Manners Sutton, the Judge Advocate, (1815,) that the practice was clearly *illegal*. We read of one hideous instance, in which a man was sentenced to receive 1500 lashes for marauding. When brought to the halberts, he seized the drum-major's sword, and called upon his comrades to rescue him: they however did not interfere. He was forthwith flogged to the full extent of his sentence; subsequently he was tried for mutinous conduct in the square of the corps, found guilty, and shot! There was surely something shockingly barbarous in all this. The spirit, that dictated the double punishment, is revolting from its very vindictiveness. To torture a man first, and then put him to death, is it not horrible?

The practice in the navy of flogging *through the fleet* is another disgraceful enormity, which should never be tolerated. Hear what a naval officer has said on the subject:—

"I believe no man has ever been known to hold up his head after going

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* It was during this year that, by an Act of the American Congress, flogging was entirely prohibited in the American army.

through the fleet. The heavy launch is fitted with a triangle, to which the wretch is tied, as if to a cross. It takes some hours to row (sometimes against wind and tide) through the fleet. The torture is, therefore, protracted till, to use a sailor's phrase, 'their very soul is cut out.' After this dreadful sentence they almost always die." P. 360.

In 1836, the award of a general court-martial was reduced to 200 lashes, of a district court-martial to 150, and of a regimental one to 100. Before that time, there was no limitation, as we have seen, to the extent of the sentence of a general court-martial: 1000 lashes were not unfrequently, and even 1500 were every now and then, ordered.

We have no intention to discuss the much vexed question as to the propriety of abolishing altogether corporal punishment by flogging; but we cannot pass over without notice some of the observations of Sir Charles Napier, recorded in his Treatise on Military Law, 1837; as they bear upon the duties of army surgeons, as well as upon the medical iniquity, if we may use the expression, of this mode of bodily chastisement. Among the objections which he urges against flogging, he very justly remarks that "it is torture of very unequal infliction;" varying, on the one hand, according to the strength or temper of the drummers employed, not to mention the will of the commanding officer or drum-major; and, on the other, according to the different powers of endurance in different men, whether this difference may depend on original constitution or on the existing state of their health at the time. The following passage, in which Dr. Marshall comments on two of the other objections of the General, contains so much interesting information that we gladly submit it entire to our readers' perusal. The fifth objection by Sir Charles is worded thus:—

"Because the state of a man's health, and the ability of a man to endure severe corporal infliction, cannot be always correctly ascertained."

"This is a most important objection, and one which has a particular reference to the duty of a medical officer. A medical officer of the greatest experience is unable to predict with certainty the effect of a severe, or even a moderate flogging, upon the future health of a man—either upon his general constitution, or locally upon his back. He may be attacked with fever, which may terminate in death; or inflammation and sloughing of the back may supervene, and be followed by a similar result. It is not to be expected that young or inexperienced medical officers should be acquainted with their duties on a punishment parade, more especially as no official instructions have been promulgated on that head. A medical officer ought to be informed in regard to what is required of him in the performance of this duty, and how he should execute it. Sir Charles Napier states that he knew of two soldiers who were flogged at Corfu, by sentence of a court-martial in 1819; both died, and neither had been punished with unusual severity. When an inquiry was made by Sir Thomas Maitland into the cause of their death, it was alleged that both of them had the '*malaria*' fever before they were punished, although neither they nor the medical officer who attended the punishment were aware of the circumstance. This allegation, or conjecture,—for it was only a conjecture,—served the purpose of shielding the commanding officer and surgeon from the consequences of any imputation of blame, in regard to the manner of carrying the sentence into effect. It is sufficiently well known that fever is frequently excited by injuries, such as the fracture of a bone, or a surgical operation, and it may also be caused by the contusions occasioned by a cat-o'-nine-tails. There is much truth in what Sir Charles Napier says on the subject. 'As to pulse,' says he, 'it forms no criterion by which a medical

officer may ascertain the state of a man's health; for what man's pulse would be regular when going to suffer, or when actually suffering torture?' I believe every possible care is in general now taken by medical officers, to ascertain the state of a man's health previous to his being flogged; but medical men cannot do what is impossible,—and it is impossible to say what may be the effect of corporal infliction with more certainty than to predict the consequence of a surgical operation. The most experienced surgeons can only guess; the result is, that when a man is tied up to be flogged, his life depends upon a guess, and that guess, perhaps, made by a young and inexperienced officer.

"Sixthly. Because the danger to life from flogging is greater in a tropical than in a temperate climate.

"The ratio of sickness in a tropical climate is more than double that of troops in a temperate climate; while the mean ratio of mortality in the former may be estimated at from four to six times that of the latter. Every cause of ill-health, including the contusion excited by flogging, is of more importance in a warm than in a cold climate. Fever is more easily excited between the tropics than where the temperature is low.

"The fact is, (says Sir Charles Napier,) that the medical officers are placed in a most unfair and perilous position. The danger to which the life of the culprit and the *life of the surgeon* are exposed, appears to be a powerful objection to this punishment. As to making the surgeon responsible, it is unjust to do so: the law places a man *by force* in a certain position, and orders him to act according to the best of his judgment; he does so, and there is an end of the matter, whatever may be the consequences, unless it can be shewn that he was drunk or mad!" P. 225.

Our author has fully described the manner in which the various military punishments, including flogging, are carried into effect in our army. As a matter of course, we must refer the curious reader for details to the original. All that we shall do is to pick out a few passages, that may be supposed to have some interest in a medical point of view. When a man is to be flogged, he is, after stripping himself to the waist, tied to a machine termed a triangle, his hands being pulled up to the top of it and there secured; cords are passed round the upper part of the thighs, and the ankles. At other times, the delinquent is lashed to a gun-wheel or a tree. The amount of suffering from the punishment then inflicted may depend a good deal upon the manner in which the prisoner is secured. If the ligatures are too tight, the hands may become quite black and benumbed—the numbness continuing for several days—from the stoppage of the circulation; this arises partly from the man's hanging on, as it were, by the hands. If, again, they be too loose, he is liable to move and wriggle about from side to side, by which means the *cat* falls on unsuitable parts, as the ribs, the neck, or even the face and eyes. During the last war, it was very common to flog upon the breech, and sometimes on the calves of the legs, when the back and breech were unsound from former inflictions. Flogging on the breech is said to be more painful than on the back, "probably in consequence of the greater sensibility of the extreme parts of the body."

"The first stroke of the cat," says Dr. Marshall, "occasions an instantaneous discoloration of the skin from effused blood, the back appearing as if it was thickly sprinkled with strong coffee, even before the second stroke. Sometimes the blood flows copiously by the time the first fifty or 100 lashes are inflicted; at other times, little or no blood appears when 200 lashes have been inflicted. During the first 150 or 200 lashes, a man commonly appears to suffer much, considerably

more, indeed, than during the subsequent part of a punishment, however large it may be. The effused blood in the skin, or, perhaps, some disorganization of the nerves of sensation, seems to occasion a blunting of its sensibility, and thereby lessen the acuteness of the pain arising from the application of the cat. Left-handed drummers, whose cats are applied to a portion of sound skin, and drummers who have not been sufficiently drilled to flogging, spread the lashes unnecessarily, and excite an unusual degree of pain. Delinquents frequently call out to the drummer to strike higher, then lower, and sometimes alternately." P. 255.

An ex-drum boy, who subsequently rose to the rank of a commissioned officer, describes the result of his *experience* as a flogger, in the following terms :—

"From the very first day I entered the service as drum-boy, and for eight years after, I can venture to assert, that, at the lowest calculation, it was my disgusting duty to flog men *at least three times a week*. From this painful task there was no possibility of shrinking, without the certainty of a rattan over my own shoulders by a Drum-Major, or of my being sent to the black-hole. When the infliction is ordered to commence, each drum-boy, in rotation, is ordered to strip, for the purpose of administering twenty-five lashes (slowly counted by the Drum-Major) with freedom and vigour. After a poor fellow had received about 100 lashes, the blood would pour down his back in streams, and fly about in all directions with every additional blow of the cat, so that by the time he had received 300, I have found my clothes all over blood from the knees to the crown of the head. Horrified at my disgusting appearance, I have, immediately after parade, run into the barrack-room, to escape from the observations of the soldiers, and to rid my clothes and person of my comrade's blood." P. 255.

Is not this truly revolting? but let us proceed in our narrative. Sir Charles Napier remarks that he has always observed that "when the skin is thoroughly cut up or flayed off, the great pain subsides. Men are frequently convulsed and screaming during the time they receive one lash to 300 lashes, and then they bear the remainder, even to 800 or 1000 lashes, without a groan: the drummers appear to be flogging a lump of dead raw flesh." Water is always at hand for the delinquent to drink, or for the purpose of sprinkling upon his face, should he become faint. Should the ends of the cords of the *cat* become entangled, they are disengaged from time to time; if clotted with blood, they are dipped in water. In former days, even this was not done; and the blood was allowed (ten being counted between each lash) to dry upon the cat, for the purpose of rendering the punishment more severe. It was then also, sometimes, the practice to steep the cat in brine before, as well as during, the infliction, of the punishment: this brutality is now strictly prohibited. The *cat* we may here observe, is usually "made of a thick strong kind of whip-cord, (we quote the description of the ex-drum boy,) and on each lash, nine in number, and generally about two feet long, were tied *three* large knots, so that a poor wretch, who was doomed to receive 1000 lashes, had 27,000 knots cutting into his back, and men have declared to me, that the sensation experienced at each lash was as though the talons of a hawk were tearing the flesh off their bones."*

* The *cat* in the navy is a much more formidable instrument. The cord used is considerably larger: it is usually log-line. Moreover, a boatswain's mate

Will it be believed that the delinquent is actually made to pay for the use of the cat, with which he has been flogged? The charge is regularly entered in his monthly account, thus—"Drum-Major's charge, 6*d.*" This is adding insult to injury with a vengeance!

Let us now briefly notice the duty of the medical officer in reference to flogging, in the army. Until within the last seven or eight years (1838,) the surgeon did not necessarily know that a man was to be flogged until he saw him tied up; and not unfrequently he was ordered to attend at the punishment of men whom he had never seen before. Now-a-days no soldier can be punished until the surgeon has examined him, and certified that he is "in a good state of health, and fit to undergo corporal punishment or imprisonment, solitary or otherwise, and with or without hard labour." On this subject Dr. Marshall has the following very pertinent observations:—

"To certify that a soldier is fit for duty is sometimes attended with some difficulty; but to report a man to be able to undergo corporal punishment is a measure which requires still more careful consideration. In this country the duty in question is comparatively easy; but in tropical countries, where, in consequence of the prevalence of disease, every man is, on an average, two or three times in hospital in the course of twelve months, a regiment will always contain a considerable number of men who are not likely to endure the punishment of either flogging or imprisonment with impunity. The following example will illustrate the difficulty which sometimes occurs in the execution of this duty. A soldier belonging to — Regiment, serving in India, was sentenced to receive 100 lashes, a punishment which was commuted by the approving officer to imprisonment for a short period. The man had been carefully examined by a medical officer previously to trial, who emitted the required certificate of health and fitness to undergo corporal infliction. Within twenty-four hours after this man was confined, he was attacked with remittent fever, followed by *delirium tremens*, and narrowly escaped with his life. Had he received his sentence, 100 lashes, there is some reason for concluding that his career as a soldier would not have been long; and, perhaps, the medical officer might have been unjustly inculpated.

"During the great prevalence of endemic or epidemic disease, or when symptoms of scurvy or ill-conditioned sores appear among the men, it may be highly expedient for a medical officer to recommend that corporal punishments should not in any case be inflicted.

"In addition to the general health of a man, I would strongly recommend a medical officer invariably to pay much attention to the state of his mind. Military crimes, such as desertion and insubordination, are not unfrequently the result of idiotcy, weakness of intellect, or partial insanity, a circumstance of much importance in the administration of military discipline, and one which a medical officer would require carefully to investigate. Fictitious madness has no doubt been treated as real; but, what is of much more importance to my subject, real madness has escaped unobserved, or has been treated and punished as fictitious—a fearful mistake when the penalty was such a punishment as flogging." P. 269.

Dr. M. gives the particulars of a case, which it is very distressing to read. Medical men cannot be too sedulous in carefully and repeatedly scrutinizing every particular, in a doubtful case.

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can *lay on* with much greater force than a drummer. A dozen lashes on board ship are suppposed to be equal, in point of severity, to 50 given on parade.

It is altogether a very painful duty, and one too surely not devoid of degradation, for a medical man to determine the fitness or ability of a criminal to undergo a certain amount of torture. We have already seen what Sir C. Napier says upon this point. In the House of Commons, in 1834, we find that Sir Charles Grey expressed himself still more forcibly: "There are sentences of courts-martial which, if inflicted, would amount to loss of life; and I think when the punishment is to the extent which we sometimes hear of, it is *degrading rather to them who inflict it* than to the sufferer, and especially degrading to the noblest art which human talent can attain—I mean the art of healing—when the attendance of a medical man is rendered necessary, *not to assuage pain and relieve suffering, but to ascertain the extreme limit of human endurance.*" Where is the medical officer that does not feel a sense of humiliation in such a responsibility being imposed upon him? Now, this responsibility is doubly onerous, when the crime, for which the man is tried, is that of "malingering," or feigning, or producing disease or deformity; because, not only is it mainly upon the evidence of the medical officer himself that the soldier must be usually convicted, but also because he is obliged to certify at the very same time that the prisoner is fit to undergo the punishment that is awarded. When we find so experienced an officer as our author admitting the difficulty, often very great, of distinguishing real from alleged suffering, disease or debility, and also of detecting some forms of mental alienation or imbecility, well does it become his younger brethren to consider their ways under the circumstances alluded to. His suggestion in the following passage indicates so much right feeling, as well as good sense, that it gives us great pleasure in making it more public.

"Instead of bringing an alleged malingerer at once before a court-martial, I think, with Dr. Cheyne, that a board, consisting of at least three medical officers of mature experience, would be found the better tribunal in the first instance, and upon their decision the ultimate measures should be grounded. It is easy to infer and to allege, that a soldier is fraudulently feigning a disability; but it is frequently very difficult to prove satisfactorily that he is not more or less affected with disease. No person should be convicted of the crime of malingering, unless it be very satisfactorily established by skilful and impartial testimony. A medical officer who has had the care of an alleged malingerer, and who has reported him to his commanding officer, may have so identified himself with the accusation, as to be somewhat biassed in his opinion of the case; and consequently a court-martial will be required to estimate the weight of his testimony, according to what they may deem its real value. I repeat my opinion, that no man should be punished for a delinquency so difficult to appreciate as feigning a disability, but upon the testimony of two competent and impartial witnesses." P. 270.

Dr. Marshall quotes a case, illustrative of these remarks, from the *Medico-Chirurgical Review*, No. 79, p. 75, in which a man, within not many hours of his death, was saved from an intended punishment by the prudent caution of the medical officer consulted.

There are still one or two points connected with the duties of the medical officer, in reference to corporal punishment by flogging, that deserve brief notice. He generally takes his station a few paces behind the sufferer; but should symptoms of fainting come on, he sometimes moves in front, that he may see his face. Every now and then, he feels the temporal pulse to guide him in forming his judgment. His business is to observe and watch the

effects of the punishment; so that, on the one hand, the delinquent does not escape any part of it by simulating exhaustion or any other alarming symptom; and, on the other, that he is not permanently disabled for service, or have his life endangered by its severity. It is strange that, in the performance of this most responsible duty, the medical officer has no official instructions for the direction of his conduct; the mere usage of the service is his only guide. Should he observe any symptoms arise during the punishment, which, in his opinion, indicate the expediency of suspending its infliction, it is his duty to intimate the circumstance to the commanding officer.

"But," our author goes on to remark, "as a commanding officer sometimes asks the surgeon whether the delinquent is not able to bear a greater number of lashes, he should invariably be prepared to give a suitable answer. A man may be able, in all probability, to endure a somewhat greater amount of punishment, without materially endangering his prospective fitness for the service; but it may be highly inexpedient in the opinion of a medical officer to sanction the infliction of a punishment to the utmost verge of safe endurance." P. 261.

Sir John Macdonald, in his evidence on Military Punishments, says that it is at the *peril* of a commanding officer to continue the punishment after the surgeon has interfered; but it does not appear that the former incurs any great risk, unless indeed the death of the sufferer ensue; for certain it is that commanding officers have frequently refused to attend to the suggestions of the surgeon, without danger to themselves. Dr. M. tells us of one case, where the surgeon was actually put and kept in arrest for 10 days, in consequence of interference: when liberated, he could get no redress. It may be worthy of notice here, that in the celebrated case of Governor Wall—tried at the beginning of the present century for the murder (20 years before) of a soldier, who, by his commands, was flogged at Goree by negro-slaves, and died five days afterwards—the surgeon of the garrison was present during the greater part of the infliction, and did *not* recommend that the punishment should be suspended. Notwithstanding this, Wall was condemned and executed at the Old Bailey.

As to the responsibility of medical officers in respect of the consequences of excessive flogging, the following examples will show how serious it is, or, at least, can be interpreted to be.

On the 5th of February, 1824, at St. Vincent's in the West Indies, two men received 1000 lashes, each, for desertion. One died two days afterwards, apparently in consequence of collapse; the other, on the 14th, after a fit of ague: sloughing commenced on the 12th, and by the following day, the whole of the back and loins had become involved. The backs of the men were not much cut.

When the report was transmitted to the Inspector-General of hospitals at Barbadoes, he forthwith officially recommended that the surgeon, Mr. F——, (who, although he had been seven or eight years in the service, had never attended a punishment parade before) should be cashiered, on the ground, that, although 1000 lashes may be awarded by a general court-martial, it is never expected that the whole should be inflicted in a warm climate; and that to stand by and see 1000 lashes inflicted on men who had served long in a tropical country, evinced great want of feeling and

judgment; it betrayed, he added, neglect or ignorance, or both, to a most discreditable degree.

The result was, that Mr. F. was ordered to England; and, in the following June, he was dismissed the service without court-martial, or his having any public opportunity of defending himself! Such treatment is surely quite irreconcileable with every principle of justice, when we read over and over again that punishment quite as severe, nay more so, has been repeatedly inflicted without any alarming consequences having ensued. Dr. Gordon Smith, in his *Forensic Medicine*, expressly says, "I have seen 1000 lashes received without complaint, and the back healed so rapidly that, in about ten days, the patient was dismissed cured." On the other hand, our author states it, as a remarkable fact, "that the fatal cases on record have, in most instances, been the result of very moderate inflictions, some even under 200 lashes." Dr. Hamilton (*Duties of a Regimental Surgeon considered*, 1794) mentions a case where, after 200 lashes, the sloughing became so deep and extensive that part of the back-bone and scapula was laid bare; the man was in hospital for seven months. Our author alludes to two similar cases of sloughing that occurred in his own experience; one man died; the other was never fit afterwards for duty, and required to be invalided. Need we add another word to show the utter uncertainty of the eventual result of corporal punishment, according to individual circumstances which no medical man can ever fully or altogether be enabled to determine.*

A man under flogging may be seized, or feign to be seized, with fainting or convulsions at the very commencement of the punishment. The mere dread of it has been known to be followed by fatal consequences, although not a single lash had been given. Such a case is related by our author. It is sometimes by no means easy, as we have already stated, to distinguish real from feigned convulsive fits. Dr. Bell, in his work on *Diseases of Soldiers in the West Indies*, relates a case, where a soldier at first escaped punishment in consequence of such an attack, when he was brought to the halberts. On being afterwards convicted of a second offence, it was determined to go through with the punishment. Dr. Bell argued thus: "If the fits were feigned, the pain of the flogging would soon put an end to every exertion of artifice; and, if they were real, it appeared probable that severe pain, to which he had not been accustomed, and the operation of terror on his mind, at the time the fit was approaching, might prevent the attack, and, by breaking the habit, might prove a useful remedy." No ill effects resulted from the flogging; but Dr. Bell leaves us in doubt after all as to the real nature of the case; for he merely says, "whether the fits were real or feigned, impressing the mind with terror, produced the effect that was desired"—*i. e.*, their non-recurrence.

Paralysis of one or of both arms has been known to be the result of flogging. Dr. M. has seen one such case; the man was discharged the service, in consequence of being disabled. In some cases, the constitution

* Persons of a sanguine temperament, with red or fair hair, and of a tall slender frame of body, usually suffer more than others. Fifty lashes have been known to disable a man of this sort for three months.

has been irrecoverably damaged by the extensive suppuration that has ensued. Sir H. Hardinge tells us that the Portuguese method of punishment, that of striking the culprit upon the back with the flat of the sword, is apt to be followed by spitting of blood, and other maladies. The same remark is made by Dr. Kirckhoff in reference to the Dutch army, in which the cane is generally used as the instrument of punishment.

The cruel consequences of second, and still more of third and fourth, instalments of punishment, may well be conceived, when we think of the thin and tender state of the newly-formed cuticle: the first few lashes usually bring the blood in streams down the man's back. We may remark that a commanding officer, it appears, has the power of remitting any part of the punishment awarded by a regimental court-martial, but not of a general one, when the sentence has been approved of by the sovereign or his representative. In the latter case, if the man should die before the entire punishment has been inflicted, (whether this be at once or at several times,) "it must be consummated upon his lifeless and mutilated carcass." So says Major James, in his *Regimental Companion*, 7th Edit. 1841:—we trust that such is not the case.

The usual application to the back after flogging is a weak solution of sugar of lead. Whenever there is very great tumefaction of the parts, there is reason to apprehend the supervention of mortification. This is one of the most troublesome and dangerous consequences of the punishment: the putrid smell, more especially in hot climates, arising from the extensive gangrenous surface, is described as being sometimes almost unsupportable alike to the patient and attendants. We have already seen that no medical man can, with certainty, predict or foresee the degree of liability of the injured parts to fall into a state of sloughing; much, very much, depends on the constitution of the patient, and other incidental circumstances.

When the sloughing and ulceration have been extensive, the cicatrized surface will sometimes remain so tender for a length of time as utterly to prevent the man wearing his cross-belts, far less carrying his knapsack. Several men have been dismissed the service in consequence.

It would seem, from Dr. Williamson's report, (Observations relative to the West India Islands, 1817,) that, in the case of the poor negroes, the integuments of the back sometimes become nearly insensible from repeated thrashings.

"By frequently punishing offenders," says he, "the parts become insensible to that laceration which tears up the skin. When that barbarous consequence is arrived at, its infliction becomes a matter of indifference to the unfortunate negro; and new sources of *torture* must be found out by which the commission of crime may be checked. It can scarcely be necessary to add, that such a condition of torpor in the parts to which punishment has been applied, can never be justified on any pretext; and I blush to reflect that white men should be the directors of such disgraceful deeds.—*Observations relative to the West India Islands*, 1817.)

"Dr. Williamson had peculiar opportunities of acquiring information on this subject, having resided in a medical capacity during fourteen years upon different plantations in Jamaica.

"Allowing that few or none die, which (says Dr. Hamilton) I believe to be the fact, immediately from punishments moderately inflicted, I know, from ex-

perience in the service, that constitutions have been considerably impaired by them. We sometimes find the body melt away into a spectre of skin and bone, from the large suppurations that have followed; nor were they ever afterwards, as long as I knew them, able to bear the same hardships as before; and they must from thence also be more incident, not only to contagious diseases, if they be in the way of them, but to other complaints to which fatigue or hardships of duty may expose them." P. 291.

Before closing our remarks, it may not be amiss to mention a professional epispastic which we find has been tried, and that too successfully, as a *succedaneum* for the coarse and barbarous one in common use in the army. Sir Charles Napier is the narrator.

"The commanding officer of one of the regiments in question, then stationed in Guernsey, where liquor is cheap, determined to try to put a stop to the crime of drunkenness on duty, by an appeal to the honourable feelings of soldiers, and at the same time to make drunkenness as unpleasant as possible, but without the lash. He gave out an order to say that he would not flog, but trust to the soldiers' self-respect for keeping sober on duty. Next day a man was drunk and confined. The colonel, accompanied by the surgeon, went to the guard-house, and felt the drunkard's pulse: he was declared to be in a fever. Nothing could be more true. He was therefore put into a blanket, and four soldiers bore him through the barracks, his comrades all laughing at the care taken of him; on reaching the hospital the patient was put to bed and *blistered* between the shoulders, fed on bread and water for a week, and then discharged cured. He was then brought on the parade, when the commanding officer congratulated him on his recovery from the fever, and sent him to join his company, when he was laughed at and jeered by his comrades during the space of a week. Many others underwent the same treatment; but the joke, though very amusing to the sober soldiers, soon began to be none to the drunkards. There was considerable pain and uneasiness—some bread, plenty of water; but no pitying comrades—no commiseration—no mercy. The experiment was completely successful. Not a man of that regiment was flogged in Guernsey from the time the men were treated with blisters: and after a fortnight there was no such thing as a man drunk for guard or parade. Now *this regiment had been in an infamous state.* 'Observe,' says Sir Charles, 'the consequence of having inefficient means. This same regiment was embarked for the Bermudas. There was at that period much drinking and much illness in these islands, rum being cheap and the blister-plaster scarce. There was no means of confinement, and the lieutenant-colonel, for want of efficient means, was obliged to use the lash, which punished without preventing drunkenness. Now the blister did prevent it in Guernsey. So much for inefficient means.'" P. 160.

Throughout the whole of his descriptions and remarks, Dr. Marshall displays a generous and enlightened feeling of condemnation of the barbarous and *unjust*—because *unequal*—punishment of flogging. There is something in it, too, positively degrading to the medical officers of the army and navy; from being messengers of mercy, they are made little better than aids of the executioner. Already has flogging been abolished among our native troops in India; nor does it exist, we believe, in the French, or in almost any of the European, armies. Without taking upon himself to say that it should be entirely given up in our public services, the general tone of our author's observations certainly tends to this result; and he strongly expresses his concurrence in the sentiments expressed by Dr. Hamilton, published upwards of fifty years ago in the following extract:—

"*I wish, after all, the military laws knew no such thing as flogging, and that in place thereof some other mode of punishment could be devised less ignominious. On this head, however, I dare say nothing, it is out of my line of life, though I wish it, with all my soul, abolished, as an inhuman thing, more suiting the nature of savages, than civilized and polished nations.*" P. 293.

And surely there is no occupation, in which medical men can more profitably fulfil their beneficent *mission*, than in using all their professional influence and authority to plead the cause of the suffering and oppressed against the heartless neglect or tyrannical severity of irresponsible task-masters. Of recent years, the fruits of enlightened medical philanthropy have been gloriously displayed in the changes already effected in our prisons and lunatic asylums, &c., and in the improved general economic treatment of our pauper population. May the same good spirit continue to animate every member of the profession, in whatever sphere he may be placed! It is thus only that our calling may justly claim to itself the proud distinction, that has been assigned to it by the eloquent orator of antiquity, of being "an art almost divine."

A MANUAL OF PHYSIOLOGY, INCLUDING PHYSIOLOGICAL ANATOMY, FOR THE USE OF THE MEDICAL STUDENT. By *William B. Carpenter*, M.D., F.R.S. With One Hundred and Eighty Illustrations. 12mo. pp. 582. London: Churchill, 1846.

THE present volume combines in a condensed form, the matter contained in the two larger works of Dr. Carpenter on Vegetable, Comparative, and Human Physiology; it thus presents an outline of the vital phenomena as they have been investigated up to the present time. Although this manual combines in some degree the scope of the above works, yet the author informs us, it cannot be regarded as a mere abridgment of them. As we have already noticed at some length, and with much approbation, Dr. Carpenter's Principles of Physiology (see Medico-Chirurgical Review, 1845, p. 321,) our remarks on the present occasion will be limited.

The first book, treating of General Physiology, contains a judicious sketch of the *nature* and *objects* of this science, which we would commend to the careful consideration of our junior brethren, with the conviction that it is a matter of the first consequence, to obtain a just conception of the fundamental laws, and of the scope of the inquiry in the prosecution of every branch of knowledge, but in a more especial degree as regards the most abstruse of all sciences—that of life. To him, who neglecting the natural order, commences his investigation with the individual actions of vitality, infinitely varied and complex as they are, all appears peculiar, bizarre, and obscure; and if he is satisfied to be guided by those writers who setting aside, as unconcerned with human physiology, the general laws of matter, of chemical affinity, of vegetable organization, and of comparative anatomy, he will rest in the false assumption that, living bodies in their structure and functions form an exception to the ordinary phenomena of Nature. The student of physiology is particularly prone to fall

into one error which has often led to the most mischievous results; it is this—the belief that more is known of the phenomena and essence of inorganic matter, than of those connected with organized bodies. One of the evils of this misconception has been the attempt, often made and always in vain, to determine that "ultimate fact" of organization, the essential nature of life. Well would it have been for the progress of our science, if physiologists, in place of these barren speculations, recollecting that we only know matter, whether inorganic or organic, by its qualities, had learnt to recognise that line of demarcation, which, separating natural phenomena from their efficient causes, has never yet been passed by the most eager or the profound of human observers.

Entertaining these convictions, we have perused with much satisfaction the following remarks of Dr. Carpenter:—

"All sciences have their 'ultimate facts;' that is, facts for which no other cause can be assigned than the Will of the Creator. Thus, in physics, we cannot ascend above the fact of attraction (which operates according to a simple and universal law) between all masses of matter; and in chemistry, we cannot rise beyond the fact of affinity (limited by certain conditions which are not yet well understood) between the particles of different kinds of matter. When we say that we have *explained* any phenomenon, we merely imply that we have traced its origin to these properties, and shown that it is a necessary result of the laws according to which they operate. For the existence of the properties, and the determination of the conditions, we can give no other reason than that the Creator *willed* them so to be; and, in looking at the vast variety of phenomena to which they give rise, we cannot avoid being struck with the general harmony that exists amongst them, and the mutual dependence and adaptation that may be traced between them, when they are considered as portions of the general economy of Nature.—There is no difference in this respect between Physiology and other sciences; except that the number of these (apparently) ultimate facts is at present greater in physiology than it is in other sciences, because we are not at present able to include them under any more general expression. Thus we find a certain peculiar endowment existing in one form of structure; and another endowment, equally peculiar, inherent in another; but we can give no reason why the structure called muscular, should possess contractility, and why the structure called nervous should be capable of generating and conveying the force which excites that contractility to action. Each of these facts, therefore, is for the present the limit to our knowledge; we can ascertain the conditions, according to which the muscular contractility, and the exciting power of the nerve, are called into operation, and can form some estimate of the amount of the forces which they generate; but we cannot see clearly that they are necessarily connected by any common tie, such as that which binds together the planetary masses, at the same time that it weighs down the bodies on the surface of the earth towards its centre." P. 15.

Another of the fallacies which still lingers in the Schools, and which, as we have said, is traceable to the too exclusive study of vital actions, is the notion that these phenomena are altogether sui generis; whilst the truth is, that the processes of the vegetative existence especially, are to a great extent dependent upon the ordinary energies of matter.

"There can be no doubt whatever," observes the author, "that, of the many changes which take place during the *life*, or state of *vital activity*, of an organized being, and which intervene between its first development and its final decay, a large proportion are effected by the agency of those forces, which operate in the inorganic world; and there is no necessity whatever for the supposition, that these forces have any other operation *in* the living body than they would have *out* of it

under similar circumstances. Thus the propulsion of the blood by the heart through the large vessels, is a phenomenon precisely analagous to the propulsion of any other liquid through a system of pipes by means of a forcing pump; and if the arrangement of the tubes, the elasticity of their walls, the contractile power of the heart, and the physical properties of the fluid, could be precisely imitated, the artificial apparatus would give us an exact representation of the actions of the real one. The motor force of the muscles upon the bones, again, operates in a mode that might be precisely represented by an arrangement of cords and levers; the peculiarity here, as in the former case, being solely in the mode in which the force is first generated. So, again, the digestive operations which take place in the stomach are capable of being closely imitated in the laboratory of the chemist; when the same solvent fluid is employed, and the same agencies of heat, motion, &c., are brought into play. Moreover we shall hereafter see reason to believe, that the peculiar form of capillary attraction, to which the term endosmose is applied, performs an important part in the changes which are continually taking place in the living body." P. 10.

The recognition of the close resemblance, and not unfrequently of the actual identity, thus displayed in the processes of the vegetative life, when compared with the ordinary phenomena of physics and chemistry, naturally suggests the inquiry how far it may be possible to refer *all* the actions of life to properties of matter. In discussing such a question one thing is immediately apparent, namely, that with our present knowledge, we can only argue from analogy and by inference, inasmuch as the *known* properties of matter are altogether insufficient to produce, for example, muscular contraction and nervous power. Dr. Carpenter has a very interesting section "On the Connexion between Vitality and Organization," which should be read throughout to realize the scope of the argument, but we will endeavour to convey in our limited space the views set forth upon this profoundly important inquiry. The author commences by observing that—

"The idea that new properties may be called forth or developed by a new combination of elements, and by a new arrangement of particles,—and that, consequently, the class of properties included under the general term *vital* is dependent upon the peculiar state of matter which is designated as *organized*,—is so perfectly conformable to what is seen elsewhere, and is so fully sufficient to explain all observed phenomena, that it would scarcely seem necessary to use any further argument in support of it. But the notion has been entertained, that Vitality is *a something superadded* to matter, and that it is absurd to suppose that the phenomena of Life can be produced by *any* combinations of matter; and this indeed so generally prevails, that it seems desirable to carry our investigations with regard to the causes of Vital phenomena a little further." P. 33.

Dr. Carpenter, then, shows that the properties of any kind of matter, even those with which we are most familiar, require *certain conditions* for their manifestation; that these properties may be either latent or active, according as these conditions are or are not present; that, for example, oxygen and hydrogen have within themselves the latent property of so combining together as to form water; but this does not manifest itself so long as they are separate; nor does it manifest itself at ordinary temperatures, when they are mingled together; but if through such a mixture we transmit an electric spark, or if we raise the temperature by the contact of a heated body, or if we simply introduce into it a portion of platinum

in a state of minute division, the requisite stimulus or excitation is given, and chemical union of the two substances is the result.

"Now if we apply these views to the phenomena of Life and Organization, we see that they enable us to regard those phenomena as *analogous* in character to those of the Inorganic world, though not *identical* with them; and they lead to a simplification of our ideas of them, which more clearly marks out the path to be pursued in their investigation. We find that the essential materials of Animal and Vegetable structures are the four elements, Oxygen, Hydrogen, Carbon, and Nitrogen; these are distinguished by the extraordinary number and variety of the combinations into which they will enter,—so much so, indeed, as to constitute, in this respect, a group quite distinct from all the other elementary substances. Now we are perfectly justified by what we elsewhere see, in attributing to these elements the property or dormant capability of exhibiting *vital* actions (in addition to the ordinary chemical ones with which we are familiar,) so soon as they are placed in the requisite conditions; in other words, as soon as they are made a part of the living system by the process of Organization. It is only the peculiarity of the conditions required to manifest this capability, which prevents us from recognizing it as an ordinary property of matter, or at least of those forms of it, which we know by experience to be capable of entering into organized structures." P. 36.

"The elements, Oxygen, Hydrogen, Carbon, and Nitrogen, in a certain state of combination and arrangement, form the substance which we term Muscular fibre; and they then manifest certain peculiar properties, which we designate as *vital*. On the other hand, those same elements exist in nearly the same proportions, but in a different state of combination and arrangement, in the substance which we term Cyanate of Ammonia; and they then exhibit a different set of properties, which we call Physical and Chemical. Now we have just as much right to say, that the contractility of muscular fibre results from the peculiar combination and arrangement of the elementary particles in its substance, as we have to say that the solidity, translucency, hardness, and other qualities of the salt (all of which are opposed to the vital properties, and cannot co-exist with them,) are necessarily connected with *its* peculiar mode of combination and crystalline aggregation. If we were *only* acquainted with these elements as they exist in organic compounds, their transposition into a crystalline salt would be almost as marvellous to us, as the opposite change is now." P. 37.

The author, in considering the opposite theory, or that according to which Vitality is regarded as something distinct from and superadded to organized structure, opposes to it the facts disclosed by embryology.

He contends that it seems absurd to suppose that, in a single cell-germ, a molecule almost invisible even with a high magnifying power, a force is concentrated, which is afterwards to be diffused through the whole structure of a vast tree, or through the ever-changing fabric of a complex animal. On the other hand, it is affirmed, that we get rid of every difficulty, whilst at the same time we reason in accordance with the fundamental principles of Logic and Philosophy, which forbid us to assume any agency that is not required to explain the phenomena, if we suppose that Vital properties are called forth or developed in the substance of the germ, whilst this substance is being organized by the agency of its parent; that the germ, in its turn, calls forth or excites the dormant properties of the combining elements (like the spongy platinum, or the electic spark in a mixture of oxygen and hydrogen;) that it thus originates, first a chemical combination, and then a peculiar structural arrangement, the elements

then manifesting peculiar properties in place of their old ones, which last as long as they exist in that condition; and so on.

Such, briefly, is the theory advocated by Dr. Carpenter in the work before us; that it is characterized by the talent and extensive acquirements of its author, our readers will have themselves perceived. But, ingenious and even probable as we admit these views to be, it is necessary to point out that they are not, and, indeed, for the reasons already stated, in the present state of our knowledge, cannot be proved: to do that it would be necessary that we should know all the properties of matter, and the conditions necessary to call them forth into active exercise. In the interval, vast as it probably will be, which will elapse ere this information be attained, we hold that the subject is one fairly belonging to physiological research: and when it is discussed in the spirit which is alone befitting such inquiries, with sentiments that is to say of profound reverence towards the Deity, and with entire faith in the Christian revelation, no evils can result from the attempt to solve one of the most deeply interesting truths connected with our present state of being.

In the chapter on Vital Stimuli, the reader will find a large mass of interesting matter relating to the influence more especially of light, heat, and electricity upon the vegetable and animal kingdoms. This is an inquiry of the deepest importance both in physiology and pathology; and which, notwithstanding the attention that has been directed to it of late years, particularly since the appearance of Dr. Edward's admirable researches upon the influence of Physical Agents on Life, is still not sufficiently appreciated by practitioners. The effects produced by light have been principally investigated in plants; but, as the author observes, there is abundant proof that this agent exercises an important influence on the nutritive processes of animals: thus the appearance of animalcules in infusions of decaying organic matter is much retarded, if the vessel be altogether secluded from light; and it has been ascertained that, if an equal number of silk-worms' eggs be preserved in a dark room, and be exposed to common day-light, a much larger proportion of larvæ are hatched from the latter than from the former. It may be further remarked that—

"Numerous facts, collected from different sources, lead to the belief that the healthy development of the Human body, and the rapidity of its recovery from disease, are greatly influenced by the amount of light to which it has been exposed. It has been observed, on the one hand, that a remarkable freedom from deformity exists amongst nations who wear very little clothing; whilst on the other, it appears certain that an unusual tendency to deformity is to be found among persons brought up in cellars or mines, or in dark and narrow streets. Part of this difference is doubtless owing to the relative purity of the atmosphere in the former case, and the want of ventilation in the latter; but other instances might be quoted, in which a marked variation presented itself, under circumstances otherwise the same. Thus, it has been stated by Sir A. Wylie (who was long at the head of the medical staff in the Russian army,) that the cases of disease on the dark side of an extensive barrack at St. Petersburgh, have been uniformly, for many years, in the proportion of three to one, to those on the side exposed to strong light. And in one of the London Hospitals, with a long range of frontage looking nearly due north and south, it has been observed that residence in the south wards is much more conducive to the welfare of the patients, than in those on the north side of the building.' P. 56.

The vast influence exerted by heat, as a stimulus to vital action, is constantly exhibited in the two organic kingdoms; and, without regarding its operation on a grand scale as displayed by contrasting the progress of vegetation in the tropical and polar regions of the earth, we may often, by observing some of the individual actions of life, obtain a striking illustration of the direct effects produced by this all-pervading and mysterious agent. The process of incubation affords such an example: here the elevation of temperature directly excites the previously dormant powers of the germ into activity, and throughout the whole process of development, sustains the actions essential to the formation of the chick. Again, if the egg be opened subsequently to the completion of the area vasculosa on the fourth day, and be carefully transferred into lukewarm water, and the actions of the heart be watched, it will be found that in a short time the organ beats more slowly and feebly, but that, on again raising the temperature of the water, the contractions become immediately quickened and in the ratio of the heat applied; in this way the stimulating influence of heat is brought directly under the observation of the senses. The following are a few of the many details adduced on this subject by the author. As examples of the adaptation of vegetables to extremes of temperature, it is found that in a hot spring in the Manilla islands, of the temperature of 187°, plants flourish; in one of the hot springs of Iceland, which boiled an egg in four minutes, a species of Chara was found growing; various Confervæ, &c., have been observed in the boiling springs of Arabia and the Cape of Good Hope; and at the island of New Amsterdam, there is a mud-spring, which, though hotter than boiling-water, gives rise to a species of Liverwort. On the other hand, there is the Lichen, serving as the winter food of the Rein-deer, which spreads itself over the ground whilst thickly covered with snow; and the beautiful little Protococcus Nivalis, or Red Snow, reddens extensive tracts in the arctic regions, where the perpetual frost of the surface scarcely yields to the influence of the solar rays at midsummer.

We have only space for one more extract relating to a curious point in the economy of the bee:—

"In order to hasten the development of the pupæ of the Social Bees, a very curious provision is made. There is a certain set, to which the name of Nurse-bees has been given, whose duty it is to cluster over the cells in which the Nymphs or Pupæ are lying, and to communicate the heat to them, which is developed by the energetic movements of their own bodies, and especially by respiratory actions of extreme rapidity. The nurse-bees begin to crowd upon the cells of the nymphs, about ten or twelve hours before these last come forth as perfect Bees. The incubation (for so it may be called) is very assiduously persevered in during this period by the nurse-bees; when one quits the cell, another takes its place; and the rapidity of the respiratory movements increases, until they rise to 130 or 140 per minute, so as to generate the greatest amount of heat just before the young bees are liberated from the combs. In one instance, the thermometer introduced among seven nursing bees stood at 92½°; the temperature of the external air being 70°. We observe in this curious propensity a manifest provision for accelerating the development of the perfect Insect, which requires (as already pointed out) a higher temperature than the larva, in virtue of its greater activity. The nurse-bees do not station themselves over the cells which are occupied by the larvæ; nor do they incubate the nymph-cells with any degree of constancy and regularity, until the process of development is approaching its highest point." P. 74.

In our present notice, we have confined ourselves to the introductory portion of Dr. Carpenter's Manual, which contains, mixed with many details of interest, an excellent sketch of General Physiology. The whole work is executed in the same spirit, and is brought up to the level of the day, all the important facts which have lately been added to our stock of knowledge being incorporated in the several sections. There are two plates and not less than 153 well executed wood-cuts dispersed through the work, which we have much pleasure in commending to the favourable notice of our readers.

SCROFULA; ITS NATURE, ITS CAUSES, ITS PREVALENCE, AND THE PRINCIPLES OF TREATMENT. By *Benjamin Phillips*, F.R.S., Assistant Surgeon to the Westminster Hospital. 8vo. pp. 397. London. Bailliere, 1846.

THIS work reflects very great credit on Mr. Phillips as a most ardent and indefatigable inquirer after truth. The accumulation of so many details must have cost him a vast deal of patient research, and their arrangement much tedious and elaborate study; for his investigations have extended not merely to the public institutions within his reach, and to those at home, but to very many on the Continent of Europe, and even to some in America and in the East Indies. By putting himself in communication with medical men in different parts and regions of the world, and thus collecting information from a variety of sources, he reasonably hoped that he might be enabled to delineate, more accurately than preceding inquirers had been able to do, the natural history of the disease he had undertaken to illustrate, searching out its causes, and shewing what amount of influence such agencies as climate, food, occupation, mode of life, &c., have upon its development, and thus to suggest a more successful plan for its treatment, preventive as well as curative. One of the chief difficulties attending all such wide-spread inquiries, arises almost necessarily from the circumstance that medical men are by no means agreed among themselves as to the essential characters of the *object* or *condition* to which the appellation of Scrofula is to be given. What are we exactly to mean by this term? What are the distinctive features of the disease? and how are we to recognize its existence? Unless all inquirers are agreed upon these points, it is quite obvious that we can never reasonably expect to arrive at anything like satisfactory general conclusions, when the data and materials, from which these conclusions are to be drawn, have been obtained from different sources. Mr. Phillips was fully aware of this difficulty *in limine*, and accordingly one of his first objects was to fix upon certain visible and readily discoverable features or phenomena, the presence of which was to be regarded as positive and trustworthy evidences of the existence of Scrofula. Let us now see what these features are said to be.

At page 9, he "defines the ordinary marks of Scrofula to mean enlarged glands discoverable by the touch or the sight, sinuses or ulcerations re-

sulting from them, and ordinary scrofulous bones or joints." At page 26, it is said that "Scrofula is a disease of the constitution, and that it is most clearly manifested by certain external signs, of which swelling of the subcutaneous lymphatic ganglia is the most conclusive." A little farther on, it is defined to be "a disease, manifested by a peculiar deposit in the subcutaneous lymphatic glands; the mere enlargement of the glands, although affording a strong presumption, being not regarded *per se* as an absolute proof, that the constitution of the individual is scrofulous; for our author expressly says that, "unless the swelling of the gland be accompanied by a deposit of a product, hereafter to be described, known as *scrofulous matter*, the proof of a scrofulous constitution is, in my judgment, wanting." Still more emphatic is the language used in the very next page, where Mr. P. expresses his belief that "diseases regarded as scrofulous, but in which no scrofulous matter is present, are not scrofulous at all, but simply the result of such low inflammatory action as is often set up in a debilitated state of the constitution." In following out his views, we shall find that our author comes to the conclusion that "the accumulation of certain morbid materials in the blood constitutes what is known as the scrofulous diathesis or constitution, and their deposition in the subcutaneous lymphatic glands constitutes what we know as scrofula." Many readers will doubtless at once object to so much stress being laid upon the change in the *subcutaneous glands*, as if the mere fact of their enlargement constituted the pathognomonic sign or evidence of the existence of the disease. Mr. Phillips himself seems to afford more than one argument against this doctrine; for we find him admitting, in one passage, that "the bronchial glands are affected (*with scrofulous deposit*, —Rev.) more than twice as often as those of the mesentery, the latter four times as often as those of the neck, though in many respects less exposed, and the last named glands four times as those of the axilla and groins." If such then be the case, does not this very admission shew how fallacious it may be to take an enlarged state of the cervical glands as a safe or trustworthy test to discover the actual or relative frequency of scrofulous disease among any number of individuals? We must confess that, throughout the entire perusal of Mr. Phillips' most laborious and praiseworthy investigations, this objection has continually recurred to our mind.—His correspondent, Dr. Griffin of Limerick, seems to take the same view; for we find him saying:—

"With regard to enlarged glands, whether visible or perceptible to the touch, I doubt their being decided indications of a scrofulous tendency; eruptions on the skin, and especially on the head, will produce them, and such affections are extremely common in Ireland. Most of the enlargements included in the above Tables were of a trifling nature; indeed the vast majority of them were so, (about 80 per cent., or thereabouts.) But I made it a rule (as you seemed to require a test,) to include all such cases as were distinctly though very slightly visible, as well as all such as were distinctly, though often but slightly perceptible to the touch." P. 318.

That it is far from being easy, even in our author's opinion, to determine the actual existence of Scrofula in an individual, is abundantly obvious from the following passages:—

"In a constitution favourable for the deposit of scrofulous matter, I believe

there are no features, in the absence of the tumour, so constant and so conclusive as to justify a reliance upon them, in pronouncing an opinion whether a constitution be scrofulous or not. It is certain that the ordinary tests are fallacious; I know that the major part of them may be observed, again and again, without any other evidence that the constitution is tainted with Scrofula. We may even have enlarged glands, while no product such as that which I have alluded to, is deposited; although, in the absence of any source of irritation, enlarged subcutaneous glands constitute grounds for grave suspicion that the constitution is scrofulous. Thus, whatever may be the constitutional peculiarity, however marked may be the general physiognomy by what is called the scrofulous diathesis, we have no certain sign of the existence of the disease until sufficient evidence can be obtained that the deposit has taken place. The constitution may suffer long before such a deposit is made, and the glands themselves may be swelled without presenting in their substance a scrofulous deposit; indeed the deterioration of the system proceeds so slowly, that although the tendency be directly onwards from the period when the gland is simply enlarged, to that when the deposit would ordinarily occur—in that interval favourable or unfavourable circumstances may be experienced, and no deposit may take place;—on the one hand, the constitution may improve and the glandular swelling may subside; on the other, the ailing child's life may be cut short by other diseases before the proof of Scrofula is complete.". P. 33.

Again, a few pages farther on, Mr. P. remarks that

"The condition of the economy, then, which is favourable to the formation of scrofulous matter is not Scrofula, but a diathesis, or disposition; it may exist long, it may cause the tumefaction of many glands, but, under favourable circumstances, it may disappear without the deposit of a single particle of that product whose presence in the glandular structure is, I conceive, necessary to constitute Scrofula. Again, I say, the condition of the system favourable to this deposit is marked by *no certain* external signs, up to the moment when the glands become tumid. The child may be fair or dark, pallid or ruddy, well fed, clad, and lodged, or all these may be the worst possible; he may be the child of wealth or of poverty; he may live in town or country; he may be the child of old or young, of healthy or sickly parents; he may be born and live within the tropics, or in the arctic circle; under all these circumstances, as we shall see hereafter, Scrofula may be developed." P. 37.

The Physical Characters of "the deposit whose presence in the glandular structures is (supposed to be) necessary to constitute Scrofula," are thus described:—"To the naked eye, it is presented in the form of an amorphous grayish, buffish, or yellowish mass, irregularly granular, and not unlike moist old cheese."

The Microscopical Characters of this morbid product have been examined by Professor Albers, of Bonn, and other continental pathologists, as well as by our countrymen, Messrs. Dalrymple and Gulliver. Mr. D. has communicated the following description to our author:—

"The whole material is composed of disintegrated tissue, granular molecules, irregular exudation corpuscles, and in which the nucleolus is seldom to be recognized, and a considerable quantity of oil globules, which may be abstracted by boiling in æther, and recovered by evaporation on a plate of glass.

"In acute or chronic inflammation of the glands, in otherwise healthy subjects, in whom no particular morbid disposition exists, the exudation corpuscles, by what appears to be a law of vitality, proceeds to the development of a cyst around the nucleus, or cyto-blast; and this nucleus even splits into two or more, and hence a pus globule is formed. At this point, however, the process stops,

and the pus globule subsequently disintegrates, and is resolved into granular and fluid matter. During the development of the cell and fissure of the nucleus, a pus globule may be said to be an organic and vitalized body, deriving its means of increase from the blastema around.

"The exudation corpuscle, however, is capable of a much higher degree of organization, and under favorable circumstances, the cell germ produces its cell, the cell elongates, and either fibre or filament is produced, as in the healing of a wound.

"In this scrofulous matter it appears that the exudation corpuscles do not possess even that feeble vital power, which induces the further change into pus, and therefore it passes from the nucleolated cyto-blast into an irregular granular body, (disintegrated,) the elements of which, by some further chemico-vital process, resolve partially into oil, or fat globules." P. 41.

Mr. Gulliver says:—

"In the human subject, it appears to me that crude tubercular matter, from whatever organ obtained, differs as little in its microscopical as in its general and chemical characters. When examined by the aid of the micoscope, crude tubercular matter can scarcely be said to present any regular structure, as it is merely made up of minutely granular matter, oily spherules, some shapeless albuminous flakes, or shreds, and a few irregular corpuscles; the latter are probably nothing but effete, or shrunken primary cells." P. 41.

As to the Chemical Characters of scrofulous matter, we need say very little. Prout regards it as albumen, imperfectly developed; Gendrin as a mass of albumen, with an excess of salts. Bredon considers it to be an albuminate of potash or soda. Boiling water and acids coagulate it, when it has become somewhat softened.

Before passing on to other matters, we may remark that Mr. Phillips is of opinion that the deposition of the peculiar scrofulous product is always preceded by a sub-inflammatory or hypertrophic condition of the affected glands; and he maintains that, when this deposit has once taken place, it is never removed by the process of absorption; suppuration and ulceration are believed to be necessary for its ejectment.

Is the Blood altered in Scrofula? and, if so, in what manner? This very important question is discussed in the fifth chapter. Baumes has stated that the constituents of the blood are less intimately mixed in scrofulous than in healthy persons. Dubois, besides alluding to the diffluence and imperfect coagulability of the blood in the former, has also pointed out a change in the form of the globules: he found them "lenticular, the central depression, or spot, being extended beyond its natural limits, and so defined that they looked like wheels;" "many of them being also deformed, notched, or otherwise irregular."

Let us now hear the results of Mr. Phillips' own observations on this subject:—

"I have examined," says he, "the blood in sixty-seven instances of Scrofula, and although I have almost always observed a considerable deviation from the condition of healthy blood, the changes have not presented sufficient uniformity to induce me to regard any particular condition as specially characteristic of Scrofula; the changes are such as seem to belong to a tolerable extensive group of affections, all, it is true, being connected with disordered nutrition and debility."

"In almost every case, the coagulum was relatively small, the serous men-

struum, large, the clot was usually very soft, almost diffluent; in a few instances only it was tolerably firm. In almost all cases, the proportion of globules was considerably under the healthy standard. The fibrin had not usually undergone much change; in a few cases it exceeded, in many more it was below the healthy standard. In most instances there was a considerable increase in the proportion of albumen; in almost every instance the proportion of salts was found to exceed the healthy standard; in some instances it was nearly doubled. At one time I thought I had clearly made out, that the proportion of chloride of sodium was deficient, and certainly in many cases it was so; but in many other cases that deficiency was not evident. If succeeding observations should establish a frequent co-existence, it might be well to carry out Russell's salt-water plan in the treatment of the disease; he conceived it to be efficacious. It may be, that salt water so used acts as a stimulant of the whole economy, increasing plastic energy and thus improving the vigour of the system." P. 57.

The upshot of the whole is, however, anything but satisfactory; for our author frankly confesses that he is not "in a condition to point out any particular state of the blood which is certainly characteristic of Scrofula;" the very same deviations from the composition of healthy blood, that are usually discoverable in that of Scrofulous patients, being present in anæmia and other diseases of debility, where no scrofulous taint can fairly be alleged to exist.

After such an admission, is it not indulging a little too much in mere speculation to assume "that the blood is changed before the deposit is made, that the accumulation of certain morbid materials in the blood constitutes what is known as the scrofulous diathesis or constitution, and that their deposition in the subcutaneous lymphatic glands constitutes what we know as Scrofula?"

Chapter VI. is devoted to the consideration of the question, "*Are pulmonary tubercle or phthisis and scrofula in their nature identical?*" Here we may first ask, why should the comparison be instituted between *pulmonary* tubercle only, and not rather between the formation of tuberculous matter generally, on the one hand, and scrofula on the other?* The only answer we can find is given in these words: "I exclude, therefore, from the present inquiry, disease of the mesenteric, the bronchial, and other glands existing in the thoracic or abdominal cavities, because I wish to consider Phthisis and Scrofula under their most decided characters." Mr. Phillips admits that no difference can be pointed out between the morbid deposit, in its early stage, in the mesenteric and bronchial glands, and that of genuine tuberculous matter in the substance of the lungs; and, although

* At the very close of the preceding Chapter, our author appears to admit the identity of scrofulous and tuberculous disease in general; for it is said: "it is certainly difficult to understand how the necessary, abnormal action, that causes the (*scrofulous*) deposit, can be developed in so many regions of the body, and often in so many different tissues, almost at the same moment of time, as we find in *tuberculous* diseases, unless something more than a local action was at work to determine the deposit." It is curious also, that in the opening paragraph of the following Chapter, the term Scrofula is expressly "extended to all those affections in which a *tuberculous* product is deposited in any of the tissues of the body."

he says that he has very rarely known scrofulous deposit in the subcutaneous glands exhibit the "gray, smooth, translucent appearance," so characteristic of pulmonary tubercles when first deposited, we might almost appeal to his own definition of scrofulous matter, (as given at the beginning of his work,) as sufficient evidence of their close relationship, if not their absolute identity.

Some of the modern continental pathologists have endeavoured to point out certain differences between scrofulous and tubercular diseases; with what success, the following extracts will show.

Lebert (whose work on Pathological Physiology we reviewed in our No. for January of the present year) says:—

"Diseases which are commonly regarded as scrofulous, may be ranged under three categories: chronic inflammations, in which nothing specific can be detected, and which are not really scrofulous; scrofulous diseases, properly so called; tubercular diseases, which, though having a great analogy with Scrofula, ought to be separated from it, as well for clinical as pathological reasons. I believe scrofulous and tubercular diseases have a certain connection, that they depend on a dyscracic state of the blood, which is, in both cases, either identical, or, at least, analogous. I regard scrofulous diseases neither as simple chronic inflammation, nor as a form of tubercular disease; and I believe in the essentiality of Scrofula. Tubercular diseases, on the contrary, appear in many circumstances to constitute rather a form of scrofulous disease." P. 64.

Is this distinction very clear or intelligible? Professor Albers, of Bonn, has addressed a letter to our author on the question of the "identity or non-identity of Scrofula and Consumption." From this we extract the following passage:—

"The most essential results of my investigation are these:—that Scrofula and Tubercles exhibit, in their physiological as well as their anatomical properties, several differences, so that their identity, so absolutely adopted in modern writings, is not to be justified. As you merely desire to know the anatomical differences, I pass over the physiological ones.

"1. Scrofulous swellings, particularly those occurring in the mesentery and the diaphragm, are so closely connected with the lymphatic vessels, that quicksilver may be driven through them and the lymphatic vessels. Real tubercles, even when they are softened, never permit the passage of the quicksilver.

"2. Scrofulous swellings have always around them, as well as in their parenchyma, blood-vessels, which pass from the cellular texture directly into them. The tubercle very frequently forms around itself a caul of vessels, often in a fibrinous layer, which separates it from the lung-parenchyma. Blood-vessels pass but rarely into a tubercle.

"3. The tubercle presents, under the microscope, separate minute tubes, which under a linear power of 550 times, prove to be cells. This is not the case in the scrofulous matter." P. 312.

Now, with respect to any microscopical differences, surely it would be injudicious to attach very much importance to them; for each successive observer seems to have little else to do, than merely contradict the statements of his predecessor on this as well as on several other points in the history of the minute structure of pathological formations. Mr. Gulliver expressly says that "crude tubercular matter, from whatever organ obtained, differs as little in its microscopical as in its general and chemical charac-

ters. The drawing shows how nearly the microscopic elements composing crude tubercle of the lungs and of the lymphatic glands agree."

Professor Albers pushes his peculiar views considerably further than any other writer, as far as we know; for we find him asserting that, "an important difference between Scrofula and Tubercles is presented with respect to treatment. Scrofula proves curable in all forms, Tubercles almost in none; Scrofula lasts a long time, Tubercles hasten much quicker to an issue. Scrofulous swellings decrease considerably after cure, and often disappear seemingly; Tubercles, when stopped in their development, do decrease and ossify, but the decrease of volume is never so considerable as with Scrofula." It is quite needless to comment upon these assertions.

Our author, who evidently leans to the same side of the question as Professor Albers and M. Lebert, appears to rest his opinion, as to the difference between Scrofula and Phthisis, mainly upon certain pathological and statistical data, the truth of which is, to say the least, very problematical. He maintains that inflammation or hyperæmic vascularity of the parts affected invariably precedes the development of the one, and not that of the other.

"The many opportunities I have had for examining lymphatic glands, before and after scrofulous matter has been deposited in them, have satisfied my mind that before the gland receives the deposit, it undergoes considerable change; it becomes enlarged, its vascularity is much increased, and its consistency is almost flesh-like; this change in its condition I conceive to be the result of inflammation. In this respect, then, there is a marked difference between scrofulous deposits in glands and tuberculous deposits in the lung. I am aware that considerable difference of opinion has existed relative to the state of the lung at the point where tuberculous matter is deposited; yet few persons will be prepared to maintain that the deposit is commonly preceded by Pneumonia. The older authors seem to have thought the presence of inflammation necessary for the development of all accidental productions, and this idea was supported by Broussais; few, however, of the more accurate observers of the present day, are inclined to doubt but that tuberculous matter is usually deposited in the lung unaccompanied by any sign of inflammation, or even active hyperæmia. We have clearly no right, then, to assume the existence of antecedent inflammation, when we find the pulmonary tissue around a tubercle apparently quite healthy." P. 67.

With respect to the statistical data, to which Mr. Phillips evidently attaches considerable importance as evidencing a marked difference between Scrofula and tubercular Phthisis, it would be easy to point out some fallacies, which must throw discredit on conclusions derived from this source. We are told that the average number of deaths annually from phthisis, in England and Wales, may be set down at 59,500; while that from scrofula does not exceed 1200. Now, without alluding to the frequent existence of indubitable scrofula in consumptive patients, who for one moment will believe that such a statement as this can be thoroughly correct? Why, very nearly the number last-mentioned die, in the course of a twelvemonth, from Tabes mesenterica alone; not to mention other scrofulous diseases of the viscera, joints, and so forth. In short, the more that we see of medical reasonings built upon the Registration Reports—as hitherto made up—the less confidence are we inclined to place in their truth. This we say, not only in disparagement of the system, but of the imperfect and inaccurate returns that have frequently been received. So completely, however, has our author

trusted to statistical calculations, that he has actually been led to believe, that "where there is a large mortality from consumption, there is a small mortality from scrofula, and *vice versâ*. To bear him out in this somewhat unexpected conclusion, he adduces, among other circumstances, the testimony of Mr. R. Martin to prove that phthisis is much less frequent in India than in our own country, while scrofulous disease is alleged, on the authority of another East India practitioner, Dr. Stewart, to be very frequent there. As we shall revert to the opinions of these gentlemen, when we come to allude to the influence of Climate on Scrofula, we shall not say more at present than that they are strikingly opposed to each other on one very material point.

If we required any other argument to prove the intimate connexion and relationship of scrofulous disease and tubercular phthisis, we should point to Dr. Baly's letter addressed to Mr. Phillips, in reply to certain interrogatories on the subject. The entire letter, indeed, proceeds on the assumption of this very point; and the numerous and very interesting facts, which the writer brings forward from his experience during the last five years at the Millbank Penitentiary, abundantly show that the very same circumstances and conditions tend either to check or to promote the development and progress of Consumption and of Scrofula—the two diseases uniformly increasing or diminishing *pari* (or, at all events, *simili*) *passu*.

"The frequency of Scrofula and internal tubercular disease amongst the prisoners in the Millbank Penitentiary, was one of the first and most important facts which offered themselves to my observation when I commenced my attendance at the Institution in the spring of 1840. I found that the prevalence of the disease had already engaged the attention of the Medical Officers, and that a considerable number of prisoners who were most severely affected with it, had been separated from the rest, and placed in a distinct Ward, and that the system of discipline as it regarded them, had by medical suggestion been much relaxed. The experience of the four subsequent years, and the examination of the Medical Records of the Penitentiary, only strengthened the impression which I had at first received.

"The great amount of tubercular disease engendered by imprisonment in the Penitentiary is proved by the following facts. During eighteen years, 205 deaths occurred amongst the prisoners in that Establishment. Of this number, however, 31 deaths arose from the Asiatic Cholera, which was epidemic during the years 1832–4, and only 174 from ordinary causes. Now of the 174 deaths, 75 were caused by Consumption, and eight by other forms of tubercular disease. Again, during the same period of eighteen years, 355 prisoners were pardoned on the ground of illness, and of these, 90 laboured under Consumption, and 78 under external Scrofula. So that very nearly half the deaths, and the same proportion of the pardons on medical grounds, were due to external or internal Scrofula, or tubercular disease." P. 362.

Mr. Phillips lays a good deal of stress, in the working out of his position that Scrofula and Phthisis are in many respects different forms of morbid action, on the circumstance that the subcutaneous lymphatic glands are diseased in only a very small proportion of those who die from phthisis. But what does this prove?—nothing more, as far as we can perceive, than that when, from a certain cachectic state of the system, one organ is more particularly the seat of disease, another is generally less seriously affected with it at the same time. Dr. Renton of Madeira puts the question

thus:—"May not your views of Phthisis being rare, when enlarged glands are common, arise from the same identical constitution wreaking its vengeance on the more external parts." Although few of our readers, we should suppose, will be inclined to adopt Mr. Phillips's views, it may be but fair to him, and not uninteresting to them, to present, in his own words, the following summary of his views:—

"I apprehend," says he, "it has now been shown, by abundant evidence, that with the exception of the deposit itself, which, whether found in the lungs, or in a cervical gland, whether examined by the naked eye, by the microscope, or by chemical analysis, is very similar, the circumstances attendant upon the development of Scrofula and Phthisis are widely different. In Scrofula the gland undergoes considerable change, inflammatory in its nature, before the matter is deposited in it; in the lung we commonly find the tissue around a recent simple tubercular deposit unchanged by inflammation. We find, further, that in districts where the causes of Phthisis act with most intensity, those of Scrofula fall lightest; that the age when the ravages of Scrofula are most keenly felt is precisely that when the visitation of Phthisis is least to be apprehended; that the sex which suffers most severely from one of those diseases is least affected by the other. And beyond all this, there is the fact that, among the numerous victims of Phthisis, at least eighteen out of every twenty exhibit no marks of having suffered from Scrofula. It seems to me, therefore, that these facts constitute so clearly marked a difference between the two affections, that it will be most convenient, most conducive to scientific correctness, to consider them as affections possessing a certain general similarity of character, but no identity. It may be that they belong to the same family, so do Pleurisy and Pneumonia; but every one deems it desirable to make as clear a demarcation as possible between those diseases. I say the same of tubercular disease generally and Scrofula, between which the points of resemblance are strong, in so far as concerns the deposit; but in all else they are weak." P. 78.

Despite this attempt at distinction, Mr. Phillips himself, in many subsequent parts of his volume, seems to regard Scrofula and Phthisis as members of the same family; for, at page 203, he actually uses the former as the *generic* term, and includes the latter, and Tabes Mesenterica, as different species.*

In Chapter VII., Mr. Phillips endeavours to show that Scrofula is by no means so common or prevalent a disease in England, as in most other countries of the world. After giving a variety of statistical particulars, he says:—

"It is thus seen, that though derived from so many and such different sources, there is a striking concurrence in the results of the evidence I have collected, and that agreement constitutes a strong reason for believing that my data do, very nearly, represent the actual prevalence of the disease. We see that the Returns

* It may be worthy of notice that M. Lugol, in his lately published volume,—of which we gave a copious review in the Number of this Journal for January, 1845—has devoted a Chapter expressly to prove the identity of Scrofulous and Tubercular diseases. The same opinion is held by MM. Rilliet and Barthez, whose admirable work on the Diseases of Children we still more recently brought under our reader's notice. Let us, however, not forget to mention that the subject named, last year, by the French Royal Academy of Medicine for the Portal prize was, "The Analogies and the Differences between Tubercles and Scrofula."

of cases of Scrofula, found among our ordinary population, are singularly confirmed, not only by the Returns of Hospitals and Dispensaries, but also by the examination of Recruits and Convicts; and I think we are thus justified in regarding as near the truth our estimate of the prevalence of Scrofula, such as we have defined the disease. That is to say, that scars are apparent in about 1¼ per cent.; that the subcutaneous glands are enlarged, so as to be perceptible on simple inspection in less than 3 per cent.; and that the glands may be detected by the finger in 24½ per cent. of those of the children of the poor who are under sixteen, and in 8 per cent. of those above; or taking the whole population, in 10 per cent.; and that something less than 3 per cent. of the people are under treatment for the disease in its various forms." P. 84.

He then adduces numerous details respecting the prevalence of the disease in other countries of Europe and of America, and alludes particularly to the numbers of recruits in the French army that are yearly rejected for having marks of Scrofula. He closes his remarks with these encouraging (would that they were *proved* to be true) words:—

"It appears, then, that comparing Paris with London, the deaths from Scrofula, when compared with the population, are six times as many in the former as in the latter capital; and that for the whole of France, the marks of Scrofula presented by recruits are twice as many as among our own recruiting population.

"Is it not, then, abundantly proved, that the notion that Scrofula is eminently an English disease, is incorrect; and am I not warranted in stating that there is no country, so far at least as our information extends, in which the people are more free from the disease than in England and Wales?" P. 91.

Is Scrofula on the increase in this country? Without adducing the data which our author has collected together on this point, we shall merely give the conclusion to which he has arrived:—

"Tried, then, by such tests as I have been enabled to apply, which though not strictly accurate, are the best we possess, and which, when used with caution, constitute a fair body of evidence on the point, the conclusion seems a fair one that Scrofula is much less prevalent in the present day than it was in the seventeenth and eighteenth centuries." P. 98.

The subject of the Etiology of Scrofula is treated at great length in Chapter IX. Nearly 150 pages are devoted to its consideration. With respect to the hereditariness or transmissibility of the disease from parent to child, Mr. Phillips, after exposing the extravagant absurdity of many of M. Lugol's hobby-horsical views on this subject, as we had previously done in our review of his work, propounds some peculiar notions of his own. He will not, for example, admit that "a scrofulous mother does *ordinarily* produce a scrofulous child."

"Nay," he says, "it is not apparent that the influence of a scrofulous parent is more efficient to induce Scrofula in the child than the influence of equal constitutional debility in a parent, originating in other causes than Scrofula. In other words, an ailing parent is less likely than a healthy one to give birth to a vigorous child, and a weakly child is more susceptible than a vigorous one of Scrofula, as well as of other diseases. But in the sense of a direct specific tendency in a scrofulous parent to reproduce in the child the same disease from which the parent suffers, in virtue of an agency of a different nature to that which would be exercised by simple constitutional debility in the parent, it seems to me we have no satisfactory proof." P. 238.

According to his researches, the influence of hereditary transmission does

not appear to be quite 4 per cent.—a very low ratio, it will be admitted. As bearing him out in his views, he appeals to the testimony of M. Louis in reference to tubercular phthisis; for this gentleman remarks that "in reality I have observed nothing decisive in favour of the hereditary character of phthisis." After examining the opinions of various other writers on the general question of hereditary influence and predisposition,* Mr. P. comes to this conclusion respecting Scrofula:—

"The result of the facts now offered is, that a feeble constitution is a favourable, though not a necessary, or indispensable, condition for the development of the tuberculous cachexia; that a feeble constitution is more likely to be the offspring of diseased than healthy parents; but that we are not in a condition to point out the causes in the parent which tend most to entail feebleness upon the child, nor what kind of disease or feebleness in the parent tends most to induce the development of Scrofula in the child." P. 127.

We need not extend our remarks on the various alleged causes of scrofulous predisposition in the child, as derived from the age or affinity of the parents, from being suckled by a scrofulous nurse, and so forth,† as our attention was so recently occupied with all these matters in the review of Lugol's work; and there are no new facts adduced by the present author to throw light upon these somewhat obscure topics. There is, however, one opinion that may deserve to be noticed *en passant*, as it is decidedly at variance with common belief."

"I doubt whether we have any clear evidence of the bad consequences, either on the mind or the body, of frequent intermarriages. I know that, in the management of stock, a notion adverse to breeding in and in prevails, but I know also that it is most extensively practised, especially with the best bloods and breeds, and therefore I conclude that the apprehended evils are not realized.

"In so far as concerns the human race, this point is not easily elucidated, and even with reference to brute animals the evidence is very inconclusive. The only mode of satisfactorily examining the question in the human species, is to observe people who are more or less isolated, and are therefore obliged to intermarry within a narrow circle. For if the principle be operative, it should be found under these circumstances. The classes most favourably situated for the inquiry are the inhabitants of small islands, having little intercourse with other islands and main lands; and on our own coasts, the people of the Isles of Portland, Man, and of the Channel Islands best realize those conditions, but I do not find that Scrofula is more prevalent among those Islanders, than in places where the

* The following truly characteristic and instructive anecdote of John Hunter well deserves to be generally known:—

"On the trial of Donellan for the murder of Sir Theodosius Boughton, he was asked: 'Is not apoplexy sometimes apt to run in a family?' To which he replied: 'There is no disease whatever that becomes constitutional, but what can be given to a child; there is no disease that is acquired, and becomes constitutional in the father but can be given to a child. The father has a power of giving that to the child by which means it becomes hereditary. There is no such thing as hereditary diseases, but such a thing as hereditary disposition." P. 122.

† "Dupuy asserts that 'the milk of a tuberculous cow may contain a greater quantity of phosphate of lime than that of a healthy cow. Labillardière has proved that there may be seven times more phosphate of lime in the milk of a cow affected with pulmonary tubercle than in that of a healthy animal." P. 141.

What should cause this inordinate amount of the earth of bones in the milk of a diseased animal?

circle is much larger. The Quakers, again, have for years married within a comparatively narrow circle, certainly much narrower than other classes of people in Britain; and I do not find more Scrofula among them than among other people of a similar social condition. The Jews, again, marry within a narrow circle, and in their early history near intermarriages seemed to be sought for; and we have no authority for saying that Scrofula is more than usually prevalent among that isolated race. And with respect to noble and royal families, I know of no evidence which favours the belief that frequent intermarriages tend to the production of Scrofula." P. 138.

From this debatable point, we pass on to notice some of the remarks of our author on the influence of *Alimentation* in the production of Scrofula.

It is almost unnecessary to say that those infants, who are deprived of their mother's milk and are brought up by hand, not only have a much more slender chance of life than those that are suckled as Nature intended, but also, if they should survive, are generally more feeble, ailing, and more frequently affected with Scrofulous maladies. No one is likely to dispute either the fact—for it has been proved over and over again—"that Scrofula is least prevalent where children and others are best fed," or the position "that food exercises a more important influence than any other agent in the production of the disease." In reference to these points, Dr. Baly mentions a very conclusive fact, derived from his experience of the prisoners in the Millbank Penitentiary, which in itself speaks volumes: "A marked difference in respect of their general health and the number of them affected with scrofulous disease, is presented by the convicts sent to the central prison of Millbank, from different parts of Great Britain, preparatory to their transportation. By far the thinnest convicts, *and those having the largest proportion of unhealthy and scrofulous individuals* amongst their number, come from the Scotch prisons, in which the diet consists of a sparing allowance of vegetable and farinaceous food."

As showing the paramount influence of diet on the production of scrofula, we have only to attend to the state of things among the poorer classes in Ireland. The disease is more than twice as common among the Irish as it is among the same classes in England; we need not say that the food of the former is vastly less nutritious. The following communication from Dr. Martin of Waterford to our author, attests the distressing amount of scrofulous disease among the children in the Portlaw district:—

"In 100 children under thirteen years of age, of people of the poorest class, not employed in factory labour, 17 had fair hair and blue eyes; enlarged glands were to be felt in the necks of 83, and two had suffered from necrosis of the tibia. In 50 children under 13, in the class of small farmers and petty shopkeepers, 28 were of fair complexion and gray eyes; enlarged glands were detected in 18 of the 50; 1 suffered from morbus coxæ. In 60 children under 13, belonging to the parents of the poorest class of factory operatives, 8 had decidedly fair complexion and blue eyes, the majority had light brown hair and gray eyes; enlarged glands were to be detected in the necks of 37; 1 had a scrofulous cicatrix, and 1 was suffering from white-swelling. In 60 children, under thirteen years, of the more comfortable class of factory operatives, 8 had decidedly fair complexion and blue eyes; enlarged glands were to be detected in the necks of 26. In 100 girls, between fourteen and eighteen, employed for more than a year at factory labour, 9 were of decidedly fair complexion and blue eyes; enlarged glands were to be de-

tected in the necks of 79, scrofulous cicatrices in the necks of 2; marks of necrosis in 4, namely, in 2 instances affecting the femur, in 1 the tibia, and 1 the metacarpal bone. In 100 boys of the same ages, 14 were of decidedly fair complexion and blue eyes; enlarged glands were to be detected in the necks of 71; none had cicatrices in the neck; 3 had necrosis of the tibia, 1 of the humerus. Altogether 470 children under eighteen years of age; of whom 315, or 67 per cent., had marks of scrofula—14 having scars." P. 178.

Mr. Phillips appears to attach much less importance to the influence of *impure air* from crowding, imperfect ventilation, and so forth, in favouring the production of scrofulous disease than most writers. According to his inquiries, "neither the general mortality, nor the deaths from scrofulous diseases, bear any definite relation to the closeness with which the population is crowded together, whether the comparison is made between one Town or District and another, or between different portions of the same Town or District."

The remarks of M. Carmichael, however, subsequently quoted by our author, very forcibly illustrate the paramount importance of exercise in the open air in a hygeienic point of view:—

"From some observations I have made on other Institutions, for instance, St. Thomas's Parochial School, and the Bethesda School, to which I was medical attendant, I came to the conclusion that depriving children of that active exercise in the open air, which is so necessary to their health and development, is almost as injurious as improper nutriment. Let a healthy child have sufficient exercise, and his powers of digestion are so sharp, that he will perhaps assimilate the most inappropriate diet: otherwise the majority of the children of our poor would become scrofulous: deprive him of his liberty, and his nutriment will remain undigested, and occasion all the symptoms I have mentioned. The children of both schools were fed, clothed, and taken the best possible care of, with this exception, that from the want of play-grounds, they were prevented from the enjoyment of active exercise; and although free from disease at the time of admission, near one-third of their number was found exhibiting the symptoms of Scrofula. They were marched out, no doubt, when the weather permitted, once a day, in a sober funeral-like procession; but let no person imagine that such dismal, boarding-school exhibitions, are sufficient for the health of children." P. 253.

The section on the Influence of *Occupation* upon the production of Scrofula contains some very curious details, the results of which will surprise most readers. That the sort of employment and the mode of life generally have much to do with the mortality of a district, independently of its local salubrity or otherwise, is abundantly proved by the well-ascertained fact that the value or expectation of life, at the age, say, of thirty years, varies a good deal in different classes of the community: it is *least* among the nobility, and *highest* among the provident regularly-employed artisans of our towns, and agricultural labourers! "Among the latter it is 40·6 years; among the members of Friendly Societies, including all trades, it is 36·6; among professional men it is 33·9; among the gentry it is 31·2; whilst among the Peerage it is only 30·9. The average for England and Wales being 34·1, Scotland 33·1, Ireland 31·7; but the expectation of life at thirty in the poor fork-grinder of Sheffield is only seven years. Thus a town of fork-grinders, however well it may be built and drained, and however well the people may be fed and lodged,

if judged of by the mortality, would be pronounced a very unhealthy district. So those districts of our country in which the bulk of the population are miners, might in the same way be regarded as insalubrious, although the real cause of the excessive mortality was mining, which is unfavourable to longevity. Again: there are districts where the population is mainly composed of agricultural labourers, of all occupations the healthiest, and if we suppose two hamlets, in the same district, the one the abode of miners, the other of agricultural labourers,—although in all else than the occupation of the people, these hamlets are similar, yet, at the age of thirty, the expectation of life will differ to the extent of seven years, and this although the miner will be better fed and clothed than the agricultural labourer."*

Although, however, the value of life is considerably higher in rural districts than in large towns, it would seem, from our author's investigation, that Scrofula is decidedly more common among our agricultural than among our manufacturing population. This may arise from the circumstance of the food of the former being less animalized, and of a poorer description altogether, than that of the latter.† The greater amount of mortality among the inhabitants of towns is due not to Scrofula, but to other diseases, induced by dissipation, intemperance, residence in a vitiated atmosphere, &c.

It is a very general belief that employment in large Factories tends strongly to develop scrofulous disease, and thus to shorten life very materially. Upon inquiry, it would seem that there has been a good deal taken for granted in arguing upon this head. Mr. Phillips has collected a

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* Dr. Guy remarks that, in the Peerage,

"At 20	the expectation of life is	38·47 years.
30	"	30·87 "
40	"	24·45 "
50	"	17·92 "
60	"	12·56 "

While, at corresponding ages, among the general population, a very large proportion of whom are deprived of advantages enjoyed by their more elevated countrymen,

At 20	the expectation of life is	40·69 years.
30	"	34·09 "
40	"	27·47 "
50	"	20·84 "
60	"	14·58 "

According to the experience of Friendly Societies, the expectation of life in rural districts, at 30 is 38·4 years; and in cities 32·8 years.

† "The dietaries of Union Workhouses, as contained in the Poor Law Circular, having been compared with the food of the agricultural labourer, and the prevalence of Scrofula in each condition having been shown, we arrive at this result, namely, that the Workhouse child is better fed, and less subject to Scrofula than the child reared in the cottage of the peasant: and when it is considered that many pauper children are received into the Workhouse when suffering and destitution have probably developed disease, and have almost certainly produced debility, the comparison is even less favourable for the child of the independent labourer, than the result which is shown by the numbers actually examined."

great deal of very interesting information, that throws considerable light upon the subject. After alluding to the cases of Manchester and Liverpool—the one a factory town the other not—where the mortality is found to be highest in the latter, he continues: "Take again Bristol and Leeds, the one with a Factory population, and the other with a population not engaged in Factory Labour, and we find the mortality in the former 2·91, in the latter 2·59 per cent.; while at Bath, famed for its salubrity, with no Factory, and with a smaller juvenile population than Leeds, the mortality is 2·35 per cent. Still it is certain that the mortality of large Factory Towns is generally large; but it must be borne in mind, that it is not uniformly larger than that of similar towns in which Factories do not exist. If we take an equal number of Factory and Non-factory Towns—each group including a population of upwards of two millions, we find that the relative mortality is as 2·5 to 2·4; and this although the influence upon the general mortality of a rapidly increasing population is usually much greater in Factory Towns than in others, because the proportion of children to adults is greater in a rapidly increasing population, than in one where the population is stationary, or where its progress is less rapid."

The result of all his tabular statistical comparisons is, that "if there be anything peculiarly injurious to life in Factory towns or Factory labour, it is not then made apparent by a register of deaths. The injurious effects of Factory Labour, when compared with other laborious occupations, are not apparent in our Mortality Tables, or in the Returns of Friendly Societies. And the mortality in Factory Towns appears to press just as heavily on masons, shoemakers and tailors, in those towns, as upon those persons who are employed in Factories, and it would therefore seems improper to refer to manufacturers an evil, which is not peculiar to them."

With respect to the question whether Factory labour tends to induce or promote the development of Scrofula, Mr. Phillips frankly admits that it would not be difficult to establish very opposite results from the evidence taken before different Factory Commissions. So much, we may again remark, *en passant*, for the reliance to be placed upon many of the results of statistical inquiries, is hitherto conducted. In France, MM. Baudelocque and Villermé regard Factories as, in general, seminaries of scrofulous disease; whereas M. Lugol tells us that Scrofula is by no means common among the workmen employed in these establishments, even when they are, at the same time, badly fed, housed, and clothed. "In fact," says this gentleman, many of whose assertions are to be received *cum grano salis*, "in my own practice, I see most of it (Scrofula) where the comforts of life are possessed." But M. Lugol is not singular in his opinion as to the effects of Factory labour upon health. Dr. Carbutt, physician to the Royal Infirmary at Manchester, addressed the Factory Commissioners, some years ago, to the following effect:—

"The fact is, that Scrofula is almost unknown in Cotton Factories, although the climate of this town and neighbourhood is particularly cold and humid. In a very extensive examination, which I and some other medical men made a few years ago, we found, to our surprise, that the Cotton Factories, instead of producing Scrofula, are in some sort, a kind of means of cure. The late Mr. Gavin Hamilton, who was for thirty-six years Surgeon of our Infirmary, and who,

previously to that, had been Surgeon in the Queen's Bays, said in my hearing, after examining the Cotton Factory, 'Gad, we found the Cotton Factories to be a specific for Scrofula.'" P. 229.

The remarkable absence of scrofulous disease among those employed in factory labour, Dr. C. attributes "to the dryness and warmth of the Cotton Factories, to the lightness of the work, and to the superior food and clothing which the superior wages of the work-people enable them to obtain." The testimony of several other professional men is brought forward, in confirmation of the same views. For example, Mr. Poyser remarks of the work-people in Mr. Arkwright's mills:—

"The result (of the examination) was extremely satisfactory to me, and confirmed me in the opinion I have maintained and before expressed to you, that the employment in these Factories, so far from producing or aggravating Scrofula, has a tendency to prevent it. The light, dry, and airy rooms in which the hands are employed, the constant exercise they take in moving from one spindle, &c., to another, and the good food and clothing their wages enable them to procure, are I apprehend among the causes which induce this immunity from Scrofula." P. 360.

That all medical visitors of Factories have not been able to report so favourably as these gentlemen, will not surprise any one. Mr. Phillips acknowledges that "the evidence collected by the gentlemen who were employed on the 'Children's Employment Commission,' is less uniformly favourable; and although the preponderance is in favour of the better relative condition of Factory Operatives, when compared with other classes of the labouring poor, yet that evidence is of so contradictory a character, that it might be plausibly used in support of either view of the subject."

It is on the whole, however, pleasing to find, from the testimony alike of French and English writers, that the establishment of large Factories has, on the whole, tended to improve, not to deteriorate, the physical condition of the working classes. That much might be done to make that condition better in respect alike of mind and body, will be denied by no one; but let us not meanwhile be led into error on the general question, from confining our attention solely to the many evils that may still exist. M. Villermé's remarks on the past and present condition of the factory labourer in France apply, with equal force, to our own country:—

"Whenever large numbers of people are collected into narrow spaces, unless there be any counteracting influence in operation, their health suffers. If we would extend this assertion to the manufactories, the facts which have been made known are far from always confirming it. There is, perhaps, no disease which belongs to a particular factory, but there are diseases which are more frequent, when the conditions of life of the labourer favour their development. But almost every disease will prevail in a crowded Non-factory Town, in an equal degree with a crowded Factory Town; and even in Factory Districts, it is upon those who are not employed in Factories, that the mortality falls with most severity. And this seeming incongruity is easily explained; those who are not employed in Factories, are equally exposed with those who are, to noxious local influences; but the earnings of the Factory Labourer are such, as to give him the means to overcome many of the influences of noxious agents, under which the idle, or the irregularly employed, or very poor, would sink." P. 234.

"There are," he continues, "many Factory Labourers whose gains are so small, that they are scarcely sufficient to procure strict necessaries. Are they,

however, more wretched and proportionally more numerous at present than formerly? There is no proof that they are. It is well that the labourer should know that his present condition is better than it has ever been. The documents from whence we may deduce a knowledge of his condition at different periods furnish the proof. I have been struck at different places, which I have visited before, to see the workmen eating better bread, wearing stockings where formerly one only saw naked feet, shoes where there used to be sabots, cleaner, lighter, better furnished rooms. I found, in fact, in all these places, not what I could have wished, but much less that was bad, than twenty or thirty years ago." P. 235.

So much for the influence of occupation in the production of Scrofula; let us now turn our attention to another topic, which of late years has excited much interest.

It affords a striking—and we cannot but add, a *humbling*, as respects the value of medical evidence—example of the discrepancy of different writers on the exciting causes of Scrofula, if we compare what Lugol says of the influence of *Prison Confinement* with the experience of Dr. Baly, as recorded in the letter to which we have already referred. The great (as some folks imagine) continental authority says:—

"We know that houses of detention are generally very humid; that they unite, if not all, the greater number of the things which are regarded as occasional causes of Scrofula, misery in every form, privation of air, of light, of exercise, the influence of heat, cold, and of damp, of coarse food, and in too small quantity, dirty and insufficient clothing in winter, a bad bed to lie on, and the deepest demoralization. The union of these causes occasions many diseases in prisons; Scabies, Prurigo, Dysentery, Chronic Enteritis, Putrid and Jail Fevers; but to this list we must not add Scrofula: for not only is it not endemic in prisons, but we scarcely see a case there, and it is equally rare in the dampest and most unhealthy workshops." P. 237.

How different is the evidence of the Medical Superintendent of our Millbank Penitentiary, as we have already shown by the extract from his letter, which we have given a few pages back!

"On examining the statistical Reports of the Penitentiaries of other countries, I have found that in them also the scrofulous or tubercular diseases have been the principal cause of death, and that the mortality from these diseases has been twice or three times as great among the prisoners in those situations, as amongst persons of the same period of life in the general population of the respective countries." P. 363.

As to the influence of *climate* on the production of Scrofula, the general impression hitherto among medical men has been that cold damp regions, exposed to frequent alternations of temperature, are, *par excellence*, its favourite abode, while such as are warm and dry are comparatively exempt from it. But now all this is alleged to be a mistake.

We have already seen what Mr. Phillips says respecting its *alleged* prevalence in our own country; he recurs to the subject in a subsequent part of his work:—

"England is always pointed at as an illustration of the first condition, whilst the plains of India have been regarded as a good example of the second; it being assumed that there is much Scrofula in England and but little in India. England and India may really exemplify those opposite conditions of the atmosphere or climate; but it has been already shown, that the prevalence of Scrofula is great-

est in India, where it has been assumed to be least, and least in England, where it has been assumed to be greatest." P. 207.

A little farther on, he again vindicates the cause of his country :—

"It has been usual to point to Holland and to England, as cold and damp countries, in which Scrofula is more than usually prevalent; and the prevalence being assumed, a cause was also assumed, and that cause was humidity. But although we may admit that those countries are comparatively cold and damp, we deny the unusual prevalence of Scrofula, at least in England, because it is now demonstrated, that in no European country do the people suffer less from Scrofula than in England." P. 213.

Mr. Phillips admits, however, that cold weather favours the development of the disease; for we find it subsequently stated that "whoever considers the question fairly, will be struck by one fact, which cannot be denied, that, in European countries at least, scrofulous diseases are evolved or aggravated, during the cold of winter."

As we mentioned before, there is no little discordancy among Mr. Phillips's correspondents as to the prevalence of Scrofula in the East Indies. Mr. Martin expressly states, as the result of very extensive observations, during many years, on the natives of Bengal, both civil and military, that "Scrofula, as an idiopathic disease, is seldom seen amongst them." He goes on to remark :—

"At the Native Hospital of Calcutta, of which I was Surgeon for ten years, I saw, however, many cases of scrofulous disease amongst the poorer Bengalees, caused, as it appeared to me, by the abuse of the rude preparations of Mercury and Arsenic, so liberally administered by the native empirics. Arsenic is given liberally in every form of fever, carefully avoiding evacuants, and as carefully excluding ventilation. It will be no matter of surprise then, that the survivors from such treatment should be troubled in after-life with various glandular enlargements. Of Mercury a rude sort of Chloride is prepared by the native doctors, containing a goodly proportion of Bichloride. This mineral is quite as freely exhibited as the first-mentioned, and with fully as evil an effect on public health amongst the indigent natives.

"In all rheumatic cases, in eruptive diseases, as well as in every chronic ailment that puzzles the empiric, this horrible preparation is given in large quantities, and often alternated with Arsenic, while salivation and enlargement of the glands, consequent on this treatment, are never considered as reasons prohibitory of bathing in rivers or tanks even during the cold season. It is needless to describe the lamentable consequences, or the frequency with which protracted suffering from glandular disease and premature death ensue.

"Now as to European residents in India; the civil and military inhabitants of the better classes are almost exempt from Scrofula, and so are their children. This exemption is equally true of parents and their offspring in Bengal, whose families in England are notorious sufferers. During an extensive observation of twenty years in the capital of British India, I do not remember three instances of scrofulous disease declaring itself, though numberless persons were known to me in whom the disease remained latent; and, as it appeared to me, solely through the influence of climate." P. 339.

He then compares the mortality arising from "every disease that can fairly be considered strumous"—viz., phthisis, hæmoptysis, scrofula, hydarthrus, and atrophia—among the British troops (European and Native) in the East Indies, with that among our soldiers at home; and what is the result of this comparison?

"The comparative exemption from pulmonary disease of those serving in Bengal in particular, is very remarkable; the ratio of admissions per 1000 being only 1·8 per annum, while in England, it is 6·4; and the annual deaths, which in England average 5·3 per 1000 by this disease, are all over India 'too small to calculate into Ratios.'

"The ratio of death by all descriptions of scrofulous disease in India generally is 1·6 per 1000 annually, whereas in England it amounts to 5·7." P. 340.

Mr. Martin concludes his letter to our author with these truly practical remarks, which, be it remembered, perfectly coincide with the opinions held by the profession generally.

"On the question of the general influence of the climate of India in scrofulous disease, I would observe in conclusion, that it is pre-eminently beneficial. The equable determination to the surface relieves from glandular obstruction and disease, while the phlegmatic of habit, with dyspepsia, languid circulation, and cold extremities, improve under a residence within the Tropics. The weak chested, as they are called in England, and such of them especially as are of scrofulous habit, are saved by going to India; and I have known instances without number in the curable stage of consumption, that is, labouring under the preceding stage of 'Tubercular Cachexy,' to enjoy good health in Bengal, and to survive their brothers and sisters at home. The fate of those, on the other hand, who go to India with suppurative Tubercles, or even in the stage immediately approaching to it, is only precipitated." P. 341.

We thus see that Mr. Martin is clearly of opinion that "scrofulous disease is *not* very frequent in India." Drs. Stewart and Spry have however come to a somewhat different conclusion. The former of these gentlemen observes:—

"In short, all half castes in Bengal may be said to be scrofulous, though the disease does not develop itself so early in this climate as at home, in the forms you describe, but in cutaneous troublesome sores, weak eyes, mesenteric diseases, spleen, &c. What is very striking is, that 3 half-caste children will exhibit these symptoms for 1 English one, though all 4 be equally carefully brought up, fed, clothed, and tended.

"The climate seems to have a favourable effect in retarding, if not even arresting entirely the development of Scrofula in English children, while it has exactly an opposite effect on half-castes. I will not pretend to account for this, though it would be easy to theorize, but I am sure that I have seen the lives of several English scrofulous-looking children saved, by keeping them in Bengal instead of sending them home, and I have known many scrofulous-looking half-caste children turn out stout fellows who were sent home very young, and must have grown up to be consumptive striplings had they been kept in India." P. 343.

After giving some figures, he adds:—

"Thus it is evident that the scrofulous constitution is the prevailing one in Bengal, a fact well known to all Indian practitioners, and that among those who have comparatively soft skin, thin hair, and light brown eyes, the proportion is great." P. 343.

Dr. Spry examined 75 children of mixed parentage, of whom *all* had swelled cervical glands; 4 had open scrofulous sores; 136, of pure English parentage, of whom none were scrofulous; and 504 native children, of whom no fewer than 300 were scrofulous. Dr. Jackson's report is to a similar effect:—

"I have examined, at different schools, indiscriminately, under ten years of

age, 100 boys, all born in India, and of Creole extraction, of whom 80 may be called of dark complexion, eighteen have flaxen hair and olive eyes, 2 very fair, with gray eyes. Of the 80, a *majority* are subject to glandular disease; of the 18, none are free from glandular tumours, but not suppurating. The 2 fairest are also scrofulous." * P. 90.

We cannot reconcile these and such-like discrepancies, without supposing that different medical men are not agreed as to what diseases are to be considered as scrofulous, and what are not. It seems to us, therefore, that it would have been judicious on the part of our author, had he hesitated more in placing much reliance on some of his statistical positions. Can we, for example, after what we have just read, yield our assent implicitly to the assertion, that "in India, in China, in Russia, in Greece, hot, cold, and temperate countries, Scrofula is unusually prevalent? It is comparatively rare in Barbaboes with a warm climate, in New Brunswick with a cold, and in the Bermudas with a temperate climate."

Many other instances might be quoted where, by trusting too unreservedly to reports of unsufficient authority and trustworthiness, Mr. P. has been led, we think, to adopt conclusions that will not stand the test of future scrutiny. Nevertheless, the highest praise is due to him for the great labour and earnest zeal that he has expended upon the object of his inquiries. The very difference of opinion elicited cannot fail to do good, by directing more general attention to the subject.

The concluding chapter of the present treatise is occupied with the important subject of the *Treatment* of Scrofula. Our readers will not expect that we do more than merely glance at some of its contents, as we had occasion very recently, while examining the works of M. Lugol and Dr. Smith, to discuss the various remedies and hygeienic means that are generally employed for the prevention and cure of this cachexy. Mr. Phillips is too sound a practitioner to have any novelty to propound, or any favourite remedy to recommend. In all his remarks, he displays much good sense and discriminating judgment.

He is far from thinking so highly of Iodine and its preparations, as most practitioners, we fear, do.

"What the exact influence of Iodine is in Scrofula, it is difficult to determine —I mean when not administered in combination with other substances than Potass. I am satisfied, however, that in many cases under the influence of Iodine, the tongue will become much cleaner, the appetite will improve, and the secretions will acquire a healthier character. And the impression left on my mind is, that the good which may be experienced from the use of this medicine, is not owing to any specific influence which it exerts over Scrofula, but to its occasional power of modifying the mucous surfaces, so as to enable them to assist in producing healthy nutrition.

"Whatever good may be derived from Iodine when uncombined, I think that, when associated with particular substances, with Iron, for instance, its power

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* "I think 8 out of 10 half-caste children scrofulous.
5 out of 10 native kind.
4 out of 10 English.
1 out of 10 Mussulman."

over the disease may be increased; but it would be difficult to prove that there are not other forms of Iron which act as favourably as the Iodide in cases of Scrofula. If that impression be correct, as much, if not more, of the benefit may be owing to the Iron as to the Iodine." P. 276.

As to Iodine having direct anti-scrofulous virtues, we quite agree with Mr. Phillips in his negative conclusion.

The muriate of Barytes is regarded by him as little, if at all, inferior to Iodine, as a discutient of scrofulous glandular tumours. The muriate of Lime is believed to have very little, if any, efficacy. Alkalis are often useful, *if* their use be associated with a tonic regimen, and exercise in pure air; but then, how much of the benefit is to be attributed to the medicine? According to our author's experience, Alkalis have been mostly useful in cases "in which much acidity pervaded the secretions, and acted upon the general economy." Cod-liver oil unquestionably produces good effects in some cases: its *modus operandi* is then ascribed by Mr. P. to its power of improving the digestion and nutrition, and not to its having any direct or specific anti-scrofulous virtues from the Iodine which it contains, or otherwise.

With respect to the effects of residence at the sea-side, bathing in the sea and drinking sea-water, our author will not agree in the extravagant praises that have been bestowed upon them by some writers:—

"I am by no means convinced," says he, "that the sea-side is more desirable for the residence of persons suffering from Scrofula, than healthy inland situations. I have been accustomed to send scrofulous patients to the sea-side, because it is usually a thorough change of air, and on their return home, I have commonly found a certain improvement in their general health; but the glandular tumours, though reduced, were usually still present. I do not mean to say that, in the majority of the patients sent, the tumours still remained, but this has certainly been the case in a large minority; and even in those patients where the cure has been apparently most complete, the tumours have frequently re-appeared during the following winter or spring."

After alluding to various other remedies that have, on the whole, been far too highly praised, he sums up in these words:—

"If change of air, good food, and exercise, cannot be procured, the difficulty of treatment is in a ten-fold measure enhanced, and the chances of cure infinitely lessened. For good food, pure air, and proper exercise, the vaunted anti-scrofulous specifics are a poor compensation; we may try one after another, and often find all fail. All that is left to us in such cases, and unhappily they are many, is to endeavour to improve the mucous surfaces and the blood by alteratives and tonics. In this way we can do some good; but it can avail but little to labour by medicine to make the stomach fitter to digest good food, when the patient cannot procure such food." P. 305.

In closing our notice of this volume, we cannot but again express our very cordial admiration of the industry, talent, and zeal—philanthropic no less than professional—displayed by its author. We have not agreed with him upon several points, and we have frankly expressed our difference of opinion. This is indeed nothing more than what Mr. Phillips expected himself; for he candidly acknowledges that several of his positions might be found wanting in precise accuracy. We trust that Mr. Phillips may be encouraged, by the success of his present work, to apply his active mind to the elucidation of some other subject of medical inquiry.

Etudes de l'Homme dans l'Etat de Santé et dans l'Etat de Maladie. Par *J. H. Reveillé-Parise*, M.D.

Observations on Man in Health and Disease. By *J. H. Reveillé-Parise*, M.D. 2d Edition. 2 Vol. pp. 1000. Paris, 1845.

The perusal of any production of the pen of this accomplished physician and able writer always affords us great pleasure. His just appreciation of what should be the noble objects of our profession and the attributes of its members, the importance he attaches to the psychical as distinguished from the mere material investigation and management of disease, the great extent of his professional and classical erudition, the kindness of heart which pervades his pages, and the elegance and ease which mark their composition, combine to render the present two volumes highly attractive, especially to contemplative readers. Several of the papers they contain have before appeared, but are modified by the results of subsequent reflection and experience. "This book," says the author, "is the result of my researches, endeavours, and reflections for more than twenty years, although the fragments which compose it have but very indirect relations with each other, by reason of the variety of the subjects treated. While endeavouring to avoid falling either into scientific common-place or into paradoxical subtlety, I have been desirous of boldly and freely stating my opinion upon many points relating to our art, and then submitting it to the good sense and equity of the public—a tribunal generally just (of course the professional public is here only alluded to by M. P.) and often without appeal. To intolerant and exclusive affirmation, I have preferred calm and reflecting examination, and the search for and legitimate interpretation of facts; being also fully persuaded that it is sometimes advantageous to bring down scientific questions from the loftiness of theory to the reality of application, and sometimes, on the other hand, to mount up by the aid of facts to a rational certitude. Science should not become too materialized, nor should it be allowed to evaporate amid vague principles."

The following are the titles of the various subjects. "The Condition of Health"—"Eclectism in Medicine"—"Management of the Convalescence of Acute Disease"—"On the Imagination as a Cause of Scientific Progress"—"On the Employment of Plates of Lead for the Cicatrization of Wounds"—"On the Science and Profession of Medicine"—"On Moral Medicine"—"On a New Method of Hastening the Cure of Recent Wounds"—"On the Existence and Cause of the Melancholic Temperament"—"Hygeiene of the Corset"—"The Basis of the Progress of the Science of Man"—"The Medical Gallery," comprising able sketches of several modern celebrated medical men. Our limited space will prevent our doing justice to all these, besides which, some of the papers hardly admit of compressed notices at all.

On the Condition of Health.

Two opposite errors are usually committed by mankind in reference to their health, the one being, the paying insufficient attention to its preser-

vation, and the other the paying too much. By far the greater numbers, however, fall into the first of these errors, and the various fallacies by which they seek to defend their inattention are passed in review by the author; but, as these are capable of refutation at the hands of any professional man, we need not advert to them. "After a long exercise of my profession," says M. R.-P., "I am convinced that health is that of which men talk most and care least. *Business before every thing* is, as I have said, their ruling maxim. We are all of us engaged in a cause against Nature, which must be decided before long, and yet scarcely any of us will take the trouble of properly examining the documents upon which so important a decision must rest. Why is this? It is because we eternally forget the grand principle that, it is a hundred or a thousand times more easy to prevent, than to cure diseases."

It may be laid down as a fundamental principle that "*health consists in a normal equilibrium of organic excitement and excitability;*" and, whenever the conditions of the economy exceed or fall short of the limits of such equilibrium, health is menaced. However various organs may be, they all agree in this, the possession of an inherent *excitability*, which would however remain inert and powerless if not developed into action by the agency of another power, usually of external origin, which in its *ensemble* may be termed *excitation* or *excitement*. Although, however, each organ possesses its special stimulus, yet all are united in one consentaneous chain of action, which constitutes that balance between excitement and excitability whence health results. The numerous relations and varying proportions of excitability and excitement, then, constitute the different conditions of the human body, and consequently of its health. In order to better appreciate them, we must not lose sight of the fact that, excitability may be accumulated in any organ if the stimulant be defective. But after the normal relation is once established, if the stimulus is continued or increased, the excitability becomes diminished and exhausted, and the organ, and eventually the general health, becomes impaired. In the first of these cases, excitability being in excess, and excitement defective, we have the *direct* debility of Brown: but where there is diminution and exhaustion of the excitability by the excess and continuance of stimulus, there is *indirect debility*—a more dangerous condition of the economy than the former, since excitement may always be reproduced, while, when excitability is exhausted by age or excess, life becomes extinguished. Among other examples cited is the following. If a man possessing good eye-sight passes from the light into the dark, his eyes fall into a state of extreme excitability or debility: they then become sensible to the slightest ray of light: gradually they become accustomed to this, and support the degree of light suitable to their actual condition. There is a due relation. But let the light be increased in intensity, duration and activity, and the eye will soon become fatigued: its excitability is exhausted by excess of stimulus, its structure undergoes alteration, and its functions are disturbed or cease, unless such excitement is interrupted.

"In reference to the great influence which this relation exerts on the economy, it may be observed that, however numerous the causes of disease may be in appearance, they may yet be reduced to three principal ones, *wounds*, *poisons*, *and moral or physical organic super-excitement*. These causes may act singly or

in concert, but the last must be considered as the most dangerous and most frequent. The reason of this is simple and evident. Man avoids the two former as well as he is able; but he frequently advances to meet the other, without reflecting that, in multiplying the intensity of excitement by pleasure or labour, he loses the secret of maintaining the equal proportions of his organic capacity. The allurement of pleasure is especially the rock upon which he splits. Man, great baby as he is, seems continually to be crying out 'give me too much.'

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Nature furnishes us with a warning of the violation of the physiological law, whose hygeienic consequences we are now considering, viz., in *satiety*. The power which the economy possesses of accommodating itself to different impressions, acts as a remedy held in reserve by nature for the majority of the accidental evils which external agents can induce. Sometimes this is the natural remedy against pain, of which the perception becomes less vivid according to its duration: sometimes it is a kind of counterpoise (although too often an insufficient one) to that perpetual desire for stimulation which exists in man. However this may be, when excitement has taken place to a certain point, and is repeated, sensation or emotion becomes blunted. What is called the piquancy of novelty and the freshness of impression disappears; but, if the excitement continues, indifference, repulsion, and then disgust and the *surgit amari* of pleasure soon begin to manifest themselves. Now, one of two things takes place: either we desist, and conforming ourselves to the physiological law await the return of excitability, that is we recal the appetite by abstinence—a safe, excellent method, the true economy of life and happiness; or we imprudently continue the excitation. The organism is soon brought into relation with this, and can no longer do without it—without the production of a painful sensation, which is again sought to be relieved by renewed stimulation. This is *habit*, the continued source of unperceived and involuntary acts—a singular, capricious, and incomprehensible condition for those who have not studied the laws of life. Prior to engendering this disposition, man, from an excess of excitability, perceives the necessity of a stimulant, which is *a natural want;* but if he continue this, or augments its quantity or intensity, he is obliged to have recourse to it almost against his will. This is a *factitious want*, the forms of which are infinitely multiplied in our social state. The one disposition dominates over man at least as much as the other; so that the habits form, so to speak, the tissue of our lives woven by us around ourselves. It is a second nature added to the first, and just as tyrannical and as powerful. In fact this second nature, which becomes general in the economy, assumes the condition of an *acquired temperament*, and frequently leaves no power whatever to reason. This factitious, importunate, exacting want is renewed every instant by virtue of the physiological law, that an organ being excited, and become in consequence more excitable, solicits the frequent return of the excitation, and that in an infinite progression. But if the power of a superior will, or external circumstances, do not vanquish this want which has sprung from habit, indirect debility or exhaustion from excess of stimulation, will occur, especially when the grosser instincts of animal life have been resorted to."

The author particularly points out that this excessive excitement may not only result from the indulgence in gross and sensual passions, but also from the inordinate pursuit of objects every way proper in themselves, or indeed of the most elevated character. "The austere pleasures of science themselves are in nowise guarantied from the effects of excessive excitement, and are just as dangerous as the others, when prudence opposes no boundaries or limits their strength or duration. The passion for knowledge or for art is as devouring, although a far nobler passion than these; and the agitation induced by it produces the same appearance and effects

as that arising from love or excessive ambition." The passion which seems to predominate in the greatest excess at our own epoch, "and which seems to gnaw away and waste our existence, enfeebling and exhausting it, is the poignant desire for the rapid acquisition of riches, even at the risk of not being able to enjoy what has been gained or accumulated."

"Never was there seen to the same extent as now the ardent desire to gain, accumulate, and bequeath riches. So is it to be remarked that, certain diseases, as aneurisms of the heart, cerebral congestions, morbid affections of the nervous system, mental alienation, &c., are infinitely more frequent at the present time than formerly, especially in large towns, where they reach a frightful figure. At least in some descriptions of excess prudence struggles, and old age intervenes, and with a man endowed with a little good sense, reason does not entirely loosen hold of the reins, although she sometimes holds them lightly enough. But when ambition, honours, gain, and avarice are in question, the too-much is never enough. Age never tempers it; illness scarcely arrests it; and death alone can say 'here are your limits.' But whether we deliver ourselves up to unbridled pleasures, and expose ourselves to the gross slavery of the animal passions, or whether we abandon ourselves no less foolishly to excessive labours of mind and body for the furthering our interests and the acquisition of lucre, it is no less true that, in transgressing the normal type of excitement and excitability, our health becomes compromised in proportion to the degree of excess and constitutional peculiarity."

The following are the applications deducible from the adoption of this equilibrium between excitement and excitability as the law of health. 1. Moderation in every thing. 2. Active exercise of organs. 3. Careful self-study. 4. The special study of preponderating organs. 5. The avoidance of a too uniform life. 6. There should never be several vivid and simultaneous causes of excitement in operation. 7. After vivid excitement the equilibrium should be re-established by intervals of repose proportioned to the intensity and duration of the excitement, and the individual disposition upon which it is exerted. 8. Let habits be acquired or discontinued gradually. 9. The influence of slight causes in the production of important effects on the health must not be lost sight of. 10. Whatever may be the social position of the person hygeienic laws must be observed. All these positions are illustrated at considerable length, and with great felicity.

On the Characteristics of Eclectism in Medicine.

"Eclectism is one of the happiest terms which medicine has borrowed from philosophy, because it so perfectly expresses the end in view—*to choose;* and to choose with discernment, after consulting experience, reason and reflection. It is the philosophy of the well-intentioned; of men of a free and pure judgment, who regard every spirit of sect or system as a form of tyranny. The eclectic school is the positive school of our art, the *realism* of medicine. Well then might we be astonished on reading in the *Annales de la Médicine Physiologique*, that eclectism is the opprobrium of medicine. This is the language of a man who has the interests of a system to fight for and defend. Eclectism has indeed always been the cause of dread to the most opposite doctrines, and alike to those systematizers who wish to change the entire face of science, and those who obstruct its progress, or who obstinately deny this. Both agree in repelling this method: they maintain that eclectism is a word void of meaning, and incapable of definition. In spite of this assertion we shall endeavour to propose one.

According to our view, '*Eclectism is the art of estimating the extent and value of proofs;*" and if we mistake not, this definition unites the two characters necessary for a good definition, brevity and clearness. In adopting it we see at once the immense advantages of this method—the sole basis of medical philosophy."

The eclectic method is an impartial one, flattering neither the prejudices or vanities of systematizers. It examines what is true and what is false, rash, or unproven in a system. It demands evidence. It is on its guard against the seductions of talent and imagination, and is anxiously occupied in indicating the means of detecting such, by the collection and comparison of facts, independently of the influence of great names and reputations. It admits no doctrine as perfect or exclusively true, but gives its preference according to the extent and value of the proofs that can be adduced. The inventors of systems adopt a different course. They first lay down a general principle, according to which the various facts and consequences are adjusted with more or less success. They all profess to have consulted facts, the very same facts are adduced in defence of the most opposite hypotheses, and twisted and turned to suit the purposes of the preconceived construction. And it is also true that every system may count its cures and its victories; for, first, all is not false in any system; next, we know with what ingenuity reverses are dissimulated or explained; and, lastly, a vast number of patients are so happily organized as to be enabled to resist the effects of whatever treatment they are subjected to. The following parallel may be perused with advantage:—

"The systematizer, like the sectarian, rejects every thing which does not proceed from the source he has chosen; there is something bigoted in the plenitude of his conviction. He first asks for the true, then for the probable, and at last admits the absurd. The eclectic endeavours to proceed from the doubtful to the probable and thence to the certain, fortifying himself whenever he can by evidence. Do not ask him under whose colours he serves, to what master he attaches himself, or what is the ensign of his school. A passing visitor, he does not remain wherever he may be carried by a system, but only where reason, experience, and the love of truth, conduct him to.

"The systematic having always his master and his doctrine, its progress and its obstacles, before his eyes, struggles in but one direction, and for him the integrity of the dogma is all and all. *Ipse dixit* is the maxim of a slave, says the eclectic, and being neither for Apollos nor Cephas, his enthusiasm is excited neither *pro* nor *con*. He welcomes the truth from whatever quarter, but he must be sure of its recognition. He must see it, examine every fraction and particle, and convince himself that it is really the truth and not its phantom.

"The fanatical systematic praises or criticizes without reserve, according to the interests of his system; but the eclectic according to the interests of science, and without exceeding a just moderation. With him, pale envy never conceals herself under the mask of the critic. He can recognise important truths in Brown, and delights in attributing to Broussais a rare sagacity in pathological research, and in the art of generalizing facts; but he will not allow that this last author has created medicine as some of his fervent disciples yet love to state.

"The systematic always proceeds from doctrines to facts, while the eclectic does just the contrary, endeavouring to follow the method of the mathematicians, *certum ab incerto, inventum ab inveniendo*. The systematic listens and adopts; the eclectic reasons and deduces: the former engages his obedience and medical faith; the latter never yields his reason to any usurper: the one thinks through an intermedium; the other is always the author of his own judgments. What I believe is the truth, says the systematic. That only is true which has been

demonstrated, replies the eclectic. My master declares it is so, says the systematic. What says experience? rejoins his adversary. But, the systematic will exclaim, cannot I see and observe as well as yourself, cannot I apply my faculties to the examination of controverted questions, and conclude in favour of the doctrine I have adopted? No, the eclectic may reply, you neither do nor can see as I do; a systematic prejudice perverts your understanding, and it is impossible for you to judge with completeness, coolness, and full knowledge of the cause. A secret inclination, a certain tendency, leads you always to see in facts other than they contain. By the aid of subtlety you can extort from them whatever you please and what even they do not yield. The best disposition for finding the truth is to commence with the destruction of every prejudice, and to become persuaded of one's entire ignorance. Is your judgment pure and disinterested enough to allow of this?

"We may see from this parallel the vast difference that may sometimes exist between two practitioners, supposing them equally well instructed and of equal good faith. And let it not be believed that such a picture is the mere work of fancy; it is the history of the last epoch of science, it was but recently the subject of the most exciting interest in the daily practice of medicine; we read of it every day in our books and in our journals, we have heard it in our schools and our academies. We may also predict which of these two adversaries will eventually triumph. The systematic has on his side the piquancy of novelty, the facility of explanation, and the enthusiasm and number of his adepts. The eclectic relies on time, his most powerful ally, and seldom in vain. The records of science present the most manifest proof that every doctrine, every exclusive system, eventually disappears, in a variable period of time, which upon an average may perhaps be stated at twenty years. We can scarcely except any other than Galenism, and that because it prevailed during the barbarism of the middle ages. The eclectic will be justified by the progress of science; his triumph is founded on the nature of things. He judges but is not judged: because he only clings to that which is demonstrated, and remains in doubt as regards the rest. If he affirms a thing he may be believed, not because he always knows the truth, but because he has taken every pains to assure himself of it, and only affirms what he knows for certain. He may sometimes employ the *perhaps* of the wise man, but at all events he does not seek to impose his own opinions and ideas. And why should he? Does he not know that such opinions must sink or flourish according to the degree of value; and that, as the truth only extends by slow undulations, the circle, enlarging more and more, will in the end excite the attention of enlightened men? Time and perseverance only are wanted.

The systematics declare, that however well-looking eclectism may be upon paper, it is destitute of practical applications. The author replies to this by exhibiting several instances in which such have been advantageously made. Thus, although they allow that in many cases essential fevers are but phlegmasiæ with resulting general irritation, the eclectics maintain that an intermittent fever is something very different from this. They admit the localization of many fevers, but reject the Broussaian doctrine of gastro-enterite as a general explanation. They have indicated also that chronic gastritis, which recently created so much attention, has often been mistaken for gastralgia. In reference to tubercles, again, eclectism is as impartial, for while denying their origin in the irritation of the pulmonary lymphatic ganglions, it admits that inflammation performs a certain part, as yet not to be appreciated, in their development, progress, &c. Eclectism acknowledges the importance of the organic lesions observed after death, but it will not allow that pathological anatomy should be made the sole basis of medicine—to the neglect of the primary changes which the

fluids undergo. Eclectism exacts, besides a spirit free from all servitude, great power of discernment, for the seizure of the truth when it offers itself to view; great knowledge, for the employment of terms of comparison; patient research, in order to examine and probe everything to the bottom; and untiring, inexhaustible attention, because nothing must be rejected without a thorough examination. Such acquisitions are rarely found united in one person; but the habit of seeing patients, the exertion of good sense, and a kind of intuitive evidence, do much towards forming the eclectic practitioner: while the accumulated experience of many such, forms a powerful instrument for the overthrow of any exclusive system, even while retaining the truths it may have contained and perfected. Academies and learned societies are in the essence and object of their institution Eclectics.

Eclectism, it is said, leads to scepticism, but surely doubt in so uncertain a science as medicine is no fault. It has been declared, also, that it is too exacting, neglecting important facts because but little known. In truth, however, it neglects no fact, however slight, that can be proved to be one, but declines receiving hypothesis and conjecture in lieu of demonstration. Of what use is eclectism? say others, it invents nothing! This is true. The eclectic does not plant and sow, but reaps and sifts. And is not the proof of the veracity of a fact as useful as the discovery of a new one? The eclectic confers another advantage, by becoming the mediator between the most opposite sects. He is suspicious of all extreme opinions alike, but opposes none with bitter hostility. He is equally averse to the obstinate adherence to exploded doctrines and practices, and to the ardent unreflecting adoption of new ones. Experience is his only law and medical creed. The virulent opposition of the advocates of the various systems is the less justifiable, since eclectism is often the only means of preserving the truths they have really acquired, and maintaining their reputation for the progress they have facilitated. From the impartiality of eclectism alone can such justice be obtained. The eclectics, without being alarmed at the overbearing language of Broussais, have always done justice to his merits, although they have deemed it necessary to submit his various doctrines to the test of experience, and not to receive them upon his mere affirmation.

We have transcribed our author's statements at some length, not only because they are in themselves interesting, but because they point to the characteristics that so pre-eminently distinguish modern English practice, which has always shown itself averse to succumb to the requirements of any exclusive system. In advocating eclectism, however, M. Reveillé-Parise by no means undervalues the legitimate uses of theory and hypothesis, but accords to them a very high value. He says:—

"The anathemas launched on all occasions against systems, appear to us but as mere declamation. Our object should be to appreciate, not proscribe them. There must be systems in medicine, for nothing will ever be discovered but by endeavouring and by *imagining*; for to leave the field of science at rest is to condemn it to sterility. Little does it signify to us that sectarians declare their doctrine as the only true one, and consequently the only durable one, for this is the common sophism which only deceives him who desires to be deceived. In fact, who is ignorant that a system in medicine is a tissue of errors and truths more or less logically disposed, all seeming united by a general principle, that

favourable and approving facts are alike collected, forcibly arranged within the frame-work, and attached to the theory, and that their interpretation is usually arbitrary, often contradictory? What matters this? If the evil is great, it is temporary, while the good will remain."

In a subsequent essay, consisting of two letters "*Upon the Imagination as a Cause of Scientific Progress*," the author pursues this argument at great length and with varied illustration. He shows that, without the power of imagination and invention, no great discovery or progress in science ever has, or ever will be, made; and that all great improvers of science, as Bacon, Newton, Linnæus, Harvey, Davy, &c., have been pre-eminent in this respect. Our space prevents our following him through his demonstration, but we will extract one passage:—

"I am not here speaking of blowing mere soap-bubbles termed hypotheses, but of theories with sufficient basis, the true expression, as near as possible, of observed phenomena. Nevertheless, we must not be frightened at every attempt. For my own part I plainly state again that, without hypothesis, the terror of little minds, science would be eternally confined to a narrow empiricism; if systems were not invented, which, without being the truth themselves, are a preparation or preliminary to it; and if we must only see, smell, feel, measure, and rely upon our senses, we deliver ourselves up to insufficient guides, stop at the semblance, and desert the philosophy of science. And yet the highest abstractions of the science of man are not to be neglected. In fact, to idealize or abstract, is to comprehend the reality better than vulgar minds, for it is to seize the hidden connection of things and facts, of causes and manifestations. Nevertheless, this word *imagination* inspires distrust, because its value in science is not known. It is believed that it is concerned only with absurd visions; but there cannot be a greater error. I allow that nothing can be more injurious to science than systematizing at first view, and generalizing by supposition; but not generalizing at all, and remaining content with material facts, has also its peril. I have not denied that utopias, chimeras, and dreams belong to the family of systems, but so also do enlarged views, principles, fundamental axioms, action, movement, and progress. Imagination sometimes wanders too far in the field of conjecture, but with experimentalism alone, its self-infatuation, its confined logic, more or less supported by statistics and sophisms, truth is not discovered but disguised, and is lost in uncertain deductions, contradictory assertions, and columns of figures."

On the Management of the Convalescence of Acute Disease.

M. Parise observes, that most medical histories terminate with the entrance of the patient upon the convalescent stage; but convalescence is a very different state to that of health. After a serious and prolonged illness, every organ suffers more or less from exhaustion, and their functions are feebly and inharmoniously performed. This condition of the economy results from the violent excitement produced by the disease, whether this has been febrile or not, and the privation of food. The former of course no longer operates, and food may now be administered for the purpose of replenishing the supply of blood, and hence recruiting the forces of the economy. The *stomach* is therefore the organ with which we have to do.

When an organ has been long deprived of its accustomed stimulants, *its tone becomes diminished and its sensibility increased*, supposing no organic lesion be present; and so it is with regard to the stomach of convalescents, and that in proportion as the disease has been prolonged and grave.

Much more so is this the case when the disease itself has been principally located in the digestive organs. We may lay it down, then, as a fixed principle, that in every convalescence *the sensibility of the stomach and intestines is augmented, and their contractility diminished.* The indications are therefore obvious enough. Diminish the sensibility of the stomach, augment its tone, and consequently enable digestion to be properly performed: but our means of accomplishing these are not so efficacious as might be supposed. If the subject be young and robust, with vigorous digestive organs, health is soon restored, and digestion established; but even in such a one, if the tormenting hunger which accompanies convalescence be sought to be appeased by injudicious supplies of food, various disturbances of the digestive organs ensue. "But if the convalescent is naturally delicate, nervous, or irritable, as often happens in the case of literary persons, artists, &c., if his digestive powers are none of the strongest even in health, if he has reached a certain age, or has been worn by anxiety and sorrow, we must expect a tedious and difficult convalescence. You believe you have gained your object, and suddenly find yourself far removed from it. During this period, the economy of the invalid suffers and languishes, his impoverished blood is slightly or badly replenished, and he continues pale, serous, and devoid of plasticity. The more this condition is prolonged, the more pronounced is the gastric sensibility, as also even his individual sensibility; for the old axiom of practical medicine, 'the blood is the regulator of the nerves,' is verified by every day's clinical experience."

At first sight, the mere *fortifying* the stomach by the administration of tonics would seem to be the simple means of procedure: but every reflecting practitioner has been in the condition to observe numerous cases in which the irritable and sensitive condition of the stomach renders the establishment of its digestive power a matter of nicety and difficulty. If we employ soothing or calming means and a debilitating regimen, the tonic power of the organ is gradually diminished; while, if we have recourse to stimulants, uneasiness, gastric irritation, thirst, dryness of the mouth, &c., prove we are exciting the organ to excess. *Diarrhœa* occurring during convalescence oftentimes deceives much. For we may have difficulty in determining whether it depends upon the remains of irritation of the digestive tube, augmented by injudicious diet, or upon simple atony of the canal. Tact is oftentimes the practitioner's sole guide; but, as a general rule it may be stated, that the diarrhœa of convalescence is connected with a defective tonicity, which will be yet further diminished by the abstinence and local depletion too indiscriminately resorted to by some practitioners.

Our means for establishing the digestive powers must in general be neither of a debilitating nor of a too stimulating character; but adapted, in their nature and quality, as near as possible to the degree of sensibility of the organs. The following indications may then form the basis of the convalescent regimen.

1. *Allow only as much food as the stomach can digest.*—Above all, we must not proportion the amount of this to that of the hunger of the convalescent. The *appetite* here continually provokes the admission of more

food than the stomach can dispose of. The patient ought not to experience weight at the stomach, flatulence, but a sensation of comfort, during digestion. The defæcation should be especially attended to. If the matters are excreted in small but well-formed masses, or if there is even slight constipation, the convalescence proceeds favourably. But when all these favourable circumstances are not present, we have to fear the manifestation of serious accidents. "We frequently observed this in the tedious and difficult convalescence of cholera morbus. Supposing even there be no relapse, a repetition of laborious digestions impresses its peculiar mark of weakness and uneasiness. *Cacochylia*, (the old and very just term for an ill-elaborated chyle,) replenishes neither the blood, the strength, nor the organic energy."

2. *The patient should eat little and often.*—Trite as this rule is, it is one of the highest importance. A feeble and irritable stomach can neither endure entire abstinence nor repletion. So that, while you do not gratify a ravenous appetite with a too large quantity of food, if you condemn the convalescent to the torments of hunger, you engender such a susceptibility of the stomach that even a very moderate quantity of food cannot be digested. Immediately that the stomach vigorously seeks for aliment you must furnish it with this, even in the night, but only in small quantity. The exact number of times food is to be given, must of course depend upon the conditions of the stomach, age, stage of convalescence, &c.

3. *Submit the food to effectual mastication.*—The importance of this rule for securing easy digestion is generally acknowledged—"He who chews little and swallows rapidly, should have a stomach of iron. Frequently, however, the convalescent, urged on by hunger, allows scarcely any time for mastication; whence result indigestion, diarrhœa, and an interminable convalescence."

4. *Let the patient be maintained warm, and especially in his feet, during digestion.*—Delicate persons frequently experience a rigor after meals, and this is especially the case with the convalescent, in whom the sensibility is great and the quantity of blood small. Cold feet are especially dangerous for delicate and irritable persons, and the convalescence may continue long after severe diseases before the animal heat is generated with sufficient energy to penetrate to the extremities of the body. Until then, it must be artificially assisted by friction, warm yet light clothing, double shoes, &c.

5. *Let the nature of the food be adapted to the sympathies of the stomach.*—No organ is so capricious in its likings and aversions as the stomach; and the diet, therefore, requires adaptation to individual peculiarities, the habits and tastes of the patient being always consulted when these are not essentially hurtful. The choice of food from its containing the greatest amount of nutritive matter in the smallest space cannot be established as a rule of universal propriety. Some persons cannot bear fatty, saccharine, or herbaceous articles of diet, while these suit others exactly. Some de-

mand light aliments, and others prefer those of a substantial, or even heavy character, giving a sense of fulness and support to the stomach.

"In 1810, the regiment to which I was attached in Spain arrived early one morning at a little village in Arragon. As there was neither time nor means for the preparation of the coarse ammunition bread, a requisition was made upon the inhabitants, and double rations of a very beautiful white bread were distributed. In the evening, however, the soldiers declared they were dying with hunger. 'How is this,' said the colonel to a grenadier, 'did you not receive a double ration this morning?' 'That is true enough, colonel,' replied the old soldier, 'but this *muslin bread* has passed through us so quickly, that we do not know what has become of it.'"

6. *Variation of the diet.*—Much contributes to the facility with which it is digested: but it must be managed with prudence, for wo to the convalescent, who, guided only by his gluttony, delivers himself up not only to his appetite, but even to the caprices of his appetite. All that flatters taste and sensation is not suitable for the stomach.

6. *Change of air.*—Sometimes in spite of every measure, however methodically put into force, the convalescent gains no ground. The digestive canal remains inert and feeble, without any cause manifesting itself. In such cases, change of air is of marked efficacy, "even where the atmosphere in which the patient resides possesses every condition of salubrity. I have seen meliorations produced by the patient but changing his abode from one part of Paris to another." Of course country air is far preferable in the great majority of cases.

7. *Let vivid moral emotions be avoided.*—The injurious effect which sudden moral emotions, whether pleasurable, or the contrary, exert upon the digestive powers is well known to the practitioner:—

"This effect is far more marked in the state of convalescence, when morbid nervous susceptibility being in its highest condition of exaltation, demands long-continued and well-directed precautions. There is not a practitioner in Paris, who, during the epidemic of Cholera in 1832, had not occasion to observe convalescence impeded by moral causes. Observe, also, that these causes were sometimes slight, and bore no relation to the accidents they gave rise to. I have seen mortal relapses induced by the breaking of a porcelain vase, an annoying noise, the falling of an insignificant letter into the fire, &c. Moreover, we may remark that, in some subjects these moral causes operate secretly. The patients are enabled to smother their grief, and restrain their tears, but the pathological influence is no less certain and dangerous. It is for the physician to discover the secret causes of this precordial anguish, to remove them, or at all events to endeavour to diminish their effects. But to attain this end he will have to combine with his knowledge, an exquisite tact and a practical acquaintance with the human heart. Hold out hope! incessant hope, be prodigal in this, it is the veritable charm for disease, *incantatio malorum.*"

Whatever skill and care may be employed, the progress of the patient may still be impeded by the occurrence of various accidents. Only two are alluded to by M. Parise, and that because of their frequency, viz., *diarrhœa* and *gastro-enteralgia.* When the first of these is present, we must endeavour to discover whether it is produced by errors of regimen, a chill, or by some moral cause. Is there any inflammation or disorgani-

zation, or simple atony of fibre? Even where from the symptoms we believe inflammatory action to be present, we must still be very cautious in recommending complete abstinence and leeching, which may produce a degree of exhaustion or contractility of the parts, that may require years for its reparation. Diminution of food will, however, then be required, as well as mild counter-irritation applied in the form of dry-cupping, sinapisms, and frictions to the abdomen. When the irritation has subsided, mild tonics and a gradually improved diet are indicated. When the diarrhœa is *passive*, tonics combined with anti-spasmodics may be given in very decided doses. M. R.-P. speaks highly of the utility of *theriacum*, and especially when united with calumba. *Diascordium* is often useful, but sometimes too stimulating. Alumnized and laudanized waters, or small doses of acetate of lead, also cure the affection. The following formula is recommended by the author. ℞. *Mucous Extract of Opium*, gr. j.; *Powder Acacia*, grs. xij.; *Powder of Calumba*, ʒ ij.; *Mint Sugar*, ʒ iv. *mix for six doses*. The employment of light wine diluted with sweet infusion of lime or orange leaves or camomile; the drinking but small quantities of fluid; the selecting aliments as dry as possible, as crusty or toasted bread in preference to crumb; and the taking food instantly craving is felt, are among the practices recommended.

Gastralgia or enteralgia is of far more frequent occurrence after severe disease, especially when the alimentary canal has been implicated, than is generally believed. The irregularity of its attacks forms one of its most distinguishing features. The poignant feeling of hunger, sometimes so acute in this disease, suddenly changes into a state of insupportable "gastric languor." In this disease, a superabundant nourishment is injurious, but in a far less degree than a too scanty one. Abstinence cannot be borne, nor do the epigastric pain and traction cease until a certain quantum of food has been swallowed; while, if its administration be too long delayed, digestion does not take place, and diarrhœa is produced. "Wo be to the patient whose attendant mistakes a gastralgia for a gastritis!" First among medicines adapted for gastralgia may be named the *sub-nitrate of bismuth* (indeed an admirable remedy in this class of affections) given alone or with opium or calumba. External revulsives, as blisters, cupping-glasses, or sinapisms, should be applied to the abdomen. The endermic use of morphia is often very useful; as are also equitation, gymnastics, travelling, &c.

Whatever means we have recourse to for the restoration of the energy of the digestive process and the establishment of the normal equilibrium between the sensibility and contractility of the canal, a prolonged employment of a variety of measures is demanded. "Perseverance and variety must never be lost sight of by the sagacious and prudent physician in an obstinate, troublesome convalescence, during which health and disease are constantly vibrating."

On the Employment of Plates of Lead in the dressing of Wounds and Ulcers about to cicatrize.

No one has oftener than the military surgeon occasion to exemplify the truth of the proverb, that necessity is the mother of invention; and our author, finding himself destitute of numerous appliances at the siege of

Saragossa, 1808-9, was obliged to tax his ingenuity to discover appropriate substitutes. The want of *lint*, however, was his most serious privation, for although moss and heath served to fill the pads for compound fractures, and tow for the dressing of large and abundantly suppurating wounds, yet there was no means of supplying the place of fine lint in dressing granulating, sensitive sores. He was, in fact, obliged to leave such uncovered, until the happy idea struck him that thin leaden plates might serve as a protection for the surface. Balls there were in abundance, and the author and several soldiers set to work with all their energy to hammer these out into plates, some of which were no thicker than a leaf of paper. To his surprise and satisfaction he found that these not only did not oppose but favoured cicatrization; and since that period he has continued to derive the greatest advantage from their employment.

A wound which is disposed to heal requires only that its surface should be protected while Nature conducts the healing process. Lint is an excellent protection of this kind, maintaining also a mild temperature of the part favourable to its union. It is also an excellent means of absorbing the pus, when this is secreted in too large a quantity. The moderately stimulating effect which some suppose it to exert upon the wound, thereby hastening its healing, is very doubtful. Among the inconveniences of this substance is the difficulty of obtaining it of sufficient good quality, at least when large quantities, as in armies and hospitals, are required. If it is too coarse and hard it irritates and inflames the wounds, while, when too fine and thin, it adheres to them so tenaciously, as to be removed even by soaking only with difficulty, often detaching the tender cuticle with it. The edges of the wounds are kept bleeding and irritated, and cicatrization is retarded. The lint sent to the army, from bad packing or selection, often proves a source of great irritation to wounds, while it has not unfrequently, when washed again to be used, been the means of maintaining hospital gangrene and other epidemic affections. The increased and increasing price of this article renders the discovery of a cheaper substitute also very desirable, when very large quantities are in request. The adherence of lint to the edges of wounds has been sought to be prevented by the intervention of strips of cerate. But the thin cicatrizing edge of a wound is eminently sensitive, and the reiterated application of greasy bodies to it often induces pruritus, or almost even erysipelas, especially when the ointment is at all rancid. Moreover, the removal of the layers of these fatty bodies which cake on the edges is often very irritating. Prepared tow has of late been much substituted for lint, but it very imperfectly supplies its place.

The employment of leaden plates seems much more fitting for favouring the cicatrization than any other means. They are to be retained *in situ* by compresses and bandages, or by adhesive plaister. Their suppleness and flexibility, and the ease with which they may be cut or shaped into any form, render them a very convenient and expeditious mode of dressing wounds. The action of the lead is purely mechanical, for the same effects may be obtained by plates of tin, gold, or silver, but its ductility and cheapness render it the most eligible substance. As the lead contracts no adherence with the edges of the wound, fatty bodies are not required, the dressing is far less painful, and the newly-formed skin is in nowise irri-

tated or torn. The dressings are also required to be renewed only every two or three days, according to the quantity of the discharge and irritability of the wound. As Nature is thus secured from interruption, the work of cicatrization proceeds much more rapidly. Infection, moreover, can never be communicated, as it may be by the employment of old lint; for the same plate, if not too thin, may be used for successive patients with perfect safety, if the precaution be taken to have it well cleaned and slightly polished. Many practitioners have tried this plan of treating wounds since it was first proposed by the author, and have confirmed his statements of its efficacy. Some, however, believing he had recommended it only for ulcers, have failed in deriving advantage from it. His object was, however, different.

"From the title I have given to this essay it is easy to see that I allude to wounds and ulcers only which are disposed to cicatrize, that is to large, superficial, red, granulating, painless wounds, covered with a fibro-purulent layer, and surrounded by a deep, rose-red margin. On the contrary, when the wound is deep, when it does not present any of the conditions favourable to a speedy cicatrization, if there is pain, whencesoever this may arise, if there is abundant suppuration, or if the wound is kept open by a virus, and it is deemed desirable to treat it by local applications, such as cataplasms, &c., I do not think this mode of dressing is ever attended with success, at least such is the result of the numerous trials which I have made. Nevertheless, it is proper to have recourse to it whenever, the obstacles to cicatrization being removed, the solution of continuity tends to a cure. In spite of these numerous exceptions, this mode of dressing will thus prove suitable to the great majority of wounds; and farther than this, there are cases in which it has succeeded, although in a different manner to that already alluded to. Thus, in certain examples of abscesses forming in encysted tumours, after the contents have been evacuated through a small aperture by the aid of the cupping-pump, a strong leaden plate bound over the part, has in a short time effected a cure, by producing adhesion of the walls of the abscess. To return to our proper subject. Whenever a wound is brought into a simple condition, and the obstacles to its cicatrization no longer exist, this is the best of all dressing, and its success is certain. However, *large and superficial wounds* are those in which it has seemed to me to be most suitable. *Burns* offer a remarkable example of this. If they are slight we may at once resort to the lead; but if they are deep we must wait until the consequent suppuration has sufficiently diminished. In both cases, the leaden plates, applied at more or less long intervals, will effect a cure of the wounds, as they will also those resulting from the application of *blisters* in irritable subjects, or at the end of long diseases. Many practitioners have observed that patients in these cases bear the application of lint or cerate to the ulcerated surface with great difficulty. Every dressing is accompanied by adhesions, bleeding, excessive suffering, loss of rest, fever, and delay of cicatrization. A simple leaden plate, which is occasionally removed, at once remedies all inconveniences of this kind.

"This mode of dressing succeeds equally well in wounds situated upon certain parts where cicatrization is usually obtained with difficulty, as over the elbow, the shin-bone, the ankle, and the tendo-Achillis. Every surgeon must have experienced the difficulty and anxiety which sometimes attend the endeavour to produce cicatrization of even small wounds situated on these parts. This new plan effects this with certainty. * * * * There are wounds *with loss of muscular substance* which no means will cicatrize, and the leaden plates offer no particular advantage in this respect; but the dressing by their aid is far more promptly and conveniently performed than with lint, while they offer a kind of artificial integument as a substitute for the natural one.

"No one is unaware that *large cicatrices* are very easily torn, and that the healing of the part is subsequently obtained only with great difficulty. In these cases the dressing with lint and ointments irritates the newly-formed skin, and re-opens the wound again and again, so that this is at last sometimes deemed incurable. But under the use of the leaden plate the healing is completed, although requiring a longer period than under other circumstances. Moreover, a new cicatrix may be rendered more firm, and is seldom then torn, by protecting it with one of these plates."

The wounds, ulcers, or erosions which form upon *œdematous extremities* are also advantageously managed in the same mode. So also the spreading ulcers which follow the application of *blisters in children*. Sometimes, after an *issue* has been made, it cannot be kept open by reason of an erysipelatous redness which surrounds it, and which is usually increased by ointments, &c. The application of a leaden leaf in a short time allays all irritation. As regards *ulcers*, properly so called, the lead will not accelerate cicatrization if they are kept open by any special local or general cause which first requires removal. In *varicose ulcers*, however, combined with pressure, the leaden plate is highly useful. To secure prompt success in the use of the lead, the plates must be properly selected, so that, without being too thick, they will not become torn upon the least movement. The tin-foil which surrounds chocolate, has been endeavoured to be used as a substitute; but even when trebled or quadrupled it offers scarcely consistence enough for repeated use. The plate must of course be accurately adjusted to the wound, just as any other dressing requires to be. Some surgeons have advantageously employed the leaden plates in even abundantly *suppurating wounds*, piercing them with a large pin in many places to give exit to the matter.

As connected with the foregoing subject, we may here notice the author's other paper upon the treatment of wounds. It is entitled—

A new Method of expediting the Healing of recent Wounds.

As the method has already been published in one of the French journals and copied into our own, we need here only recal the principal points set forth. The great object in the management of wounds is the *prevention or diminution of inflammation*. Wounds only of a very slight extent and depth, unaccompanied by complication, have hitherto been considered as capable of union by the first intention. This arises from the presence of a foreign body, which, whether solid or fluid, must be eliminated by the suppurative process before union can take place. Foreign bodies, however, impeding union, do not always proceed from without, nor are they always perceptible to the senses, and capable of removal by instruments, &c. Effused or infiltrated blood, the little clots closing up the divided mouths of small vessels, fragments of skin, cellular tissue, or torn and bruised muscle, may produce all the effects of the ordinary foreign bodies. A *detritus* of this kind induces irritation, inflammation, and suppuration; and what the older surgeons termed the *detersion* of a wound is only an expression for the elaboration of these foreign bodies by the efforts of Nature, and the consequent placing the wound in a condition for healing. In proportion as the wound is made by a sharp instrument, and is of a clean uncontused character, the amount of *detritus* will be smaller, and

the chances of adhesive inflammation greater. Thus fire-arms, by the great amount of contusion they induce, produce the description of wound least favourable for prompt union :—

"In scientific investigations we should never lose sight of the principle of proceeding from what is real and proved by experience to possible melioration. Experience here proves that the more considerable is the *detritus* of a wound, the less hopes we can entertain of a cure without inflammation or suppuration. The primary indication is then, at first, either to seek a cure by the first intention, or at least to diminish, as much as possible, the inflaminatory action which is to follow. The approaching the edges of the wound, the removal of atmospheric influences, the extraction of external foreign bodies, the support of any flaps that may exist, occasionally the use of compression, and, especially in recent times, the more or less prolonged employment of refrigerants, are the means daily resorted to with more or less success. For my part, I believe that, before having recourse to measures for diminishing inflammatory action, and especially the employment of cold water, there is a more fundamental indication which requires great consideration, viz., the *immediate and complete removal of the detritus*. Nothing is so likely to prevent or diminish inflammation as the consequent suppuration. But how is it to be accomplished? The best means is by the exercising a strong aspiration upon the surface of the wound, whether this be done by means of *suction* as employed by the ancients, or by the application of the *cupping-pump* as I have practised it myself. By this proceedure the effused blood is *pressed out* from the wound as well as from the extremity of the small vessels, and from the cellular tissue. The same occurs also with regard to a number of small, almost invisible, fragments, furnished by the tearing of the parts. When no foreign body any longer exists, and the wound remains clean and pure, if we can so speak, no cause of irritation being present, there is no inflammation, or, if this cannot be entirely prevented, it is at least much limited in its intensity and consequences—of course taking into account the obstacles to union which the gravity of the wound and the nature of the organs affected may offer."

The suction of wounds was employed in the most ancient times, and even a class of persons devoted to its performance existed. Their offices were chiefly required for the bites of serpents, but were also sometimes employed after the wounds of arrows, spears, &c.; and Percy states that, in the last century, there were men in the French army whose duty it was to perform this suction. At Amsterdam, a treatise on the "Art of Sucking Wounds" was published in 1712. There can be no doubt that suction and cupping-glasses have been advantageously used in the management of poisoned wounds; and the observations of Celsus in favour of the practice have been amply confirmed by modern observers. In simple wounds, also, the author has convinced himself, by experiments on animals, that the application of the cupping-glass much hastens the curative process.

In employing this means the cup used should always be rather larger than the surface of the wound, so that this latter be completely submitted to the action of the pump. The application, too, should be resorted to as quickly as possible, for if enough time have elapsed to allow of the setting up of irritation or inflammation, the operation is contra-indicated. The pain occasioned is very slight and bearable, and any bleeding which may be produced ceases after the glass is removed. It is almost needless to add, that this means does not exclude the employment of any of those

ordinarily had recourse to. M. Parise cites several instances, in which the healing of wounds of various degrees of gravity, as regards the accompanying contusion of parts, seemed to have been much accelerated by the employment of this means. He suggests the following extension of the application.

"I have not had the opportunity of employing this means after serious and important surgical operations, but I am disposed to believe in its utility in diminishing the intensity of the inflammation, and hastening cicatrization in these cases. Probability, which ordinarily foresees more than it sees, here is increased almost to a certainty, for the same causes produce the same effects. I am indeed convinced, that if, after the amputation of a large limb, the vessels being tied or submitted to torsion, a large cupping-pump were applied to the surface of the wound to remove the *detritus*, prior to dressing the stump, the subsequent inflammation and suppuration would be much diminished. Perhaps, too, purulent infection, that deplorable accident which carries off so many patients, might be thus prevented. Where would there be the danger in the experiment?"

On the Melancholic Temperament.

Notwithstanding the revolutions which medicine has undergone, the temperaments have remained much as the ancients described them, and so faithful to nature have the description of these been, that the efforts of Stahl, Cabanis, Hallé, and other celebrated modern physicians, have only succeeded in impressing certain modifications upon these. "The ancients founded their doctrines upon the condition of the fluids—the moderns have considered only the arteries, veins, and lymphatics: the one founding their doctrine upon the contents, and the other upon the containing parts: they were humorists, and we are solidists: and this is all the difference. We always must refer to their groups of the characteristics of temperaments, modified however by the progress of science." The *melancholic temperament* has been however among the moderns a subject of much controversy. The doctrine of Galen which prevailed in the schools for 16 centuries explained it by the existence of *black bile*, without, however, explaining what was to be understood by this. Cabanis and others look upon it as only a variety of the bilious temperament, without shewing in what it differs from this. Tissot and some others denied its existence altogether, while Broussais referred it to a chronic inflammation of the digestive organs.

The author believes the characters of this temperament to be well-marked, and that the denial of its existence because its cause has not been ascertained, is a very unphilosophical procedure. He has frequently met with it in Italy and Spain, and it is not uncommon in France and England; but, as a general rule, it most frequently occurs in hot and dry countries, as India, Spain, Arabia, &c.

"Political and religious institutions may develop or exaggerate it, but they cannot produce it; for we must not confound *melancholy* with the *melancholic* temperament; the former may occur in every variety of organization, while the latter only predisposes to this singular neuralgia.

"Slender forms; muscles slightly developed, their fibres being however dry and rigid; a narrow chest; a yellowish-brown skin covered with hairs, and generally brown ones; large veins coursing over the cutaneous surface: compact

cellular tissue; a thin, bony, pale, or olive-coloured face; eyes sometimes blue, but usually black and deep-seated, having the conjunctiva of a yellowish colour; a timid, but sometimes sparkling look; slow, circumspect, and embarrassed movements—are the external characteristics. The internal ones are marked by a quiet, small, contracted, and sometimes unequal pulse; feeble respiration, inactive hæmatosis, and scanty secretions, especially that of the skin. The digestion is very uncertain, while some epigastric tenderness is often present.

"The cerebral system and intellectual faculties correspond to those conditions of the organism. The will is at first uncertain, then obstinate and inflexible; there is an extraordinary perseverance in habits, and a distrust which only increases by acquaintance with mankind; sometimes there is a great feebleness of intellect, and at others great power and depth of thought and observation. The imagination always runs to extremes, and, capable of producing chimeras and chefs-d'œuvre, it takes on a double aspect peculiar to this temperament. It is at once vivid and capricious, and yet able to strongly concentrate itself in objects submitted to meditation, so as to be able to follow a fixed idea to the extreme limits of reflection."

As the *sanguine* temperament is the result of the predominance of the *arterial* or red-blood system, so is the *melancholic* produced by the predominance of the *venous* or *black-blood* system. Upon opening the body of a man of this last temperament we are struck with the brownish-yellow colour of his tissues, and the slight development of the various organs. *Venous plethora* is observed in the chest, but particularly in the abdomen, the portal system being especially loaded. The blood contained in these engorged vessels is also remarkably black. The physiological characters already given as those of this temperament, are readily explained by this predominance of the venous system, *e, g.*, the colour and difficult transpiration of the skin, the defective hæmatosis, small and slow pulse, abdominal uneasiness, &c. The predominance of the sanguine and lymphatic temperaments in *women* explain also the rarity with which the melancholic temperament is observed in them: so, too, in reference to *age*, none of the temperaments depend so much upon the influence of this in exhibiting their characters; and where these are witnessed early, so are other signs of old age prematurely observed. Fernel fixes the 65th year as the period when the melancholic temperament is observed. Cullen believes that venous plethora commences between the 35th and 40th years. The period when it manifests itself doubtless differs according to original constitution. Its advent is announced, besides the external signs already adverted to, by two anatomical changes consequent on the progress of age, viz.: the diminution of the capacity of the chest with ossification of its cartilages, and the diminution in the size, elasticity, and suppleness of the arterial system, the veins becoming at the same time enlarged and distended. All men, therefore, in the progress of age, acquire this temperament, but in very different degrees.

The melancholic must not be confounded with the *bilious* temperament.

"In the latter, the chest is broad and ample, giving rise to great development of the lungs, whence an active hæmatosis, a voluminous, active heart, and an ample arterial system. The muscles are thick and well defined. The secretion of fat increases abundantly with age in the bilious but not in the melancholic. The *hepatico-gastric* system always plays a large part in the former, while in the latter, there is rather an abdominal venous ingurgitation. In the one, there is

bilious vomiting and the class of diseases attributed by Stohl to *polycholia*, while, in the other, hæmatemesis, melæna, hæmorrhoids, and passive hæmorrhages in general, are far more frequent.

"The moral contrast is as strong. The man of unmixed bilious temperament is prompt, active, decided, fiery and irascible, he can conceal nothing: resistance is odious to him, and contradiction insupportable. He attacks obstacles with directness and hardihood. His language and manners announce the impetuosity and violence of his organic movements."

It is equally distinct from the *lymphatic*.

"The physiologico-anatomical considerations we have stated, show that the melancholic temp. is never allied with the lymphatic. The vascular system containing the white fluids is only very greatly developed in early life, its activity diminishing with age. It is completely and in everything the opposite of the venous system. We see *lymphatico-sanguine* and *bilio-sanguine* temp., but the characters of the melancholic and of the sanguine are never confounded. They are mutually exclusive; only one of the two great arterial and venous trees can predominate in the economy."

M. Parise enters at some length into the modifications produced upon this melancholic or venous temperament by different degrees of development of the *nervous system;* and then proceeds to consider some of the corroborations of his views derived from pathology and clinical experience. The influence which the venous predominance exerts upon the vital actions must be, in some degree, extended to morbid affections; and thus we find in the melancholic temperament acute affections are less common and less severe, and chronic ones more common and more obstinate than in other constitutions. Inflammations are often latent, and may be attended by disorganizing processes not discoverable until after death. *Melancholia*, which is dependent on a certain condition of the nervous system, may occur in any temperament, but it is the melancholic which especially predisposes to it. The frequent connexion of this affection with the melancholic temperament is shown by the marked relief which frequently follows the disgorging the abdominal venous system by hæmorrhoidal flux or other discharges, either natural, or artificially induced by leeches or purgatives.

"It is a remark made long since by practitioners, that each age has its hæmorrhages. In infancy and childhood they are manifested in the upper parts of the body, in manhood and old age in the lower parts. We see now how easily this is accounted for. We see also why hæmorrhoids are so salutary to a man predisposed to apoplexy. Whenever, in such a constitution, there is no varicose dilatation of the hæmorrhoidal vessels, the hæmorrhagic effort is concentrated upon the encephalon, instead of being directed towards the abdomen and lower portions of the body; hence apoplexy, partial paralysis, &c. Let us not forget that the impulsive force, acting upon the venous blood, is almost of no amount, and diminishes progressively. This is particularly the case in the brain, notwithstanding Nature's contrivances for facilitating the circulation of the venous blood by multiplying the sinuses; and thus cerebral sanguineous congestions are very frequent, especially in old age, whether on account of the abundance of black blood, or the weakness of the very extensible and dilatable venous tissue. Veins have a general tendency to become varicose, owing to the slight elasticity of their walls. When this occurs to the superficial veins, we apply appropriate measures, especially compression; but when such dilatations occur in the viscera, and especially the brain, where venous plethora may so

easily arise, is so common and so dangerous in mature age, it is impossible but serious consequences must result. In this way we explain the occurrence of apoplexy in old men, and why thin, non-sanguineous looking persons sometimes are liable to cerebral congestions."

The same condition of the venous system influences the diseases of the nervous system, and may give rise to hypochondriacism, monomania, &c. The venous temperament likewise predisposes to *gangrena senilis*, and the passive dropsies so common in advanced age.

The author concludes his paper by a few observations upon the utility of the study of the Temperaments.

"It contributes to the establishment of the foundations of clinical medicine. Temperaments are seldom mentioned in books, but much cared for in practice, for they must be considered in the diagnosis and treatment of every disease. Organic tendencies are in fact the primary conditions of diseases: they determine their nature and form, regulate their course, and augment or limit their intensity. It is upon these tendencies that the physician bases his estimate of probabilities of the progress and issue of the disease, and the effects and doses of appropriate medicine. All predisposing causes, *i. e.*, those which it is most essential to be apprised of, originate in the different temperaments of the economy. How are we to oppose the development of these causes, if the very principle of the temperament is unknown to us? The important point is to arrive at the *idiosyncracy* by a practical and searching knowledge of the temperament. The practitioner who joins to the experience of things that of persons, possesses a safe basis for appreciating the unequal proportions of vital energy we remark among most men. The sagacity of judgment, the certainty of tact, the almost infallible experience of some practitioners, oftentimes only depend upon their precise knowledge of idiosyncracies and temperaments."

We regret that want of space will prevent our offering any analysis of the author's papers on *Moral Medicine*, and the *Basis of the Progress of Medical Science*. The former, however, was noticed by us on its first publication (M.-C. Rev., No. 70.) We will occupy the remainder of this notice with a few passages selected from the "*Galerie Médicale*," which contains some masterly sketches of the characteristics of several celebrated members of the profession, as Alibert, Desgenettes, Broussais, Marc, Richerand, Larrey, Corvisart, Boyer, Dupuytren, &c., &c. We should feel much pleasure if it were in our power to re-produce these *in extenso*, for they seem to have been very conscientiously written. "This gallery," says the author, "I make bold to say, is a true study of man, for in the lives of some celebrated practitioners of our epoch, I have sought less for details of interest than for lessons of profit and examples for instruction." The absence of a collection of biographies has always seemed to us a great and leading defect in our medical literature. Many of our practitioners know little or nothing of the lives and doctrines of their predecessors, and are unable to take encouragement or warning, as the case may be, by their example. A volume of well-written medical biographies would, we doubt not, prove an attractive as well as a useful work. Nothing worthy of the name has been attempted in this, or indeed in any country.

Dupuytren.

The determined and overbearing character of this celebrated surgeon manifested itself from the very commencement of his career. When, at

17 years of age, he was appointed one of the Demonstrators at *l'Ecole de Medicine*; his poverty was so great that he was obliged to mend his own clothes. So far from despairing, however, he had such a complete conviction of his own superiority, and presentiment of its successful exertion, that he had already in view the surgical dictatorship which he afterwards attained. The renown of Bichat was a source of continual vexation to him, and upon hearing of his death, he exclaimed, "I begin to breathe." From this period, too, his reputation began to increase rapidly.

"Knowledge, judgment, indefatigable industry, untiring patience, address, energy, and resolution, were all employed by him in the manner characteristic of a man who is determined to succeed—one who is never at ease but at the head of the crowd, who proceeds straight to the end in view, in spite of all the obstacles and difficulties of rivalry. This is in fact the period at which Dupuytren really worked, when he amassed most of his scientific labours, although it was long before he reached his eventual eminence. There was then to be observed in him the decided desire of increasing the riches of science, of extending its limits, united with that burning ambition which destroys rest, and which is the foundation of that strength which characterizes the man impatient of all control and jealous of every influence."

His avowed determination in 1812, to obtain the professorship held by Sabatier in *l'Ecole de Medicine* raised him numerous enemies and opponents; but he triumphed over all, in spite of an evident desire of the judges to favour some of these. Nothing now impeded his progress.

"Arrived at the apogee of his renown, the genius of Dupuytren was quite equal to his position; and he was enabled to struggle with the torrent of occupations by which he was surrounded. There was indeed something really gigantic in that intelligence which provided for everything by its prodigious activity, indefatigable industry, determined ardour, and by a sort of ubiquity which allowed him to accomplish as well as to desire everything. For twenty years he was always one of the earliest at the *Hôtel-Dieu*, however inclement the season. He passed more than five hours there in the severest bodily and mental exertion. No sooner had he left these, than new duties and crowds of patients called him to all parts. * * * * * * Whoever wishes to give unrestricted praise to Dupuytren, must consider him in his professor's chair. There he triumphed—there he excelled. It was a remarkable thing to hear this illustrious surgeon delivering his beautiful lectures on Clinical Surgery, amidst three or four hundred silent and attentive pupils. With what order and method he enchained the attention of his auditors! how accomplished was he in instructing them, exhibiting the light of evidence with a skilful and vigorous logic, and inculcating good and sound information! His clear, calm, strong delivery, lucid and expressive, fluent, easy without prolixity, elegant without effort, always fortified by things and facts; the clearness of his views; his rare talent for exposition and deduction; his astonishing facility in defining, classifying, and expressing his ideas, in rendering objects sensible and evident, and in arresting, fixing, and directing the attention, gave to his lectures a singular attraction. No idle detail, declamation, or ornament was there: all was clear, precise, intelligible; all tended to fact, to its object, and to instruction. * * * * * * Nevertheless, the pupils sometimes reproached the professor with dwelling only with predilection upon those cases in which success had crowned his efforts, and disguising his reverses with skill. Another, and perhaps better-founded, reproach is, that this illustrious professor never quoted any one else, and French surgeons still less than any. To speak the truth, he had a marked and exclusive disdain for all that had not proceeded from himself; and this indifference, or rather scepticism

for everything and everybody, when the talent of his rivals was in question, had its root in his naturally haughty character—completely despising the opinions of others—those of the past as useless, and those of the present as inferior.

"What I have said of the professor exhibits his qualities as a surgeon. It is allowed by all that Dupuytren was the most remarkable man in the profession of his epoch. Few have possessed that vast and rare assemblage of qualities which constitute a great surgeon. He had especially an admirable strength of judgment, which rendered his diagnosis so true and affirmatory, that it struck every one with astonishment. Moreover, in the observation and application of precepts, there was an exactness of analysis, a certainty of practical good sense, and a depth of examination, which are certainly rare. Although bold, without the least trace of pusillanimity, he never operated until the last moment, when he was certain that this furnished the last resource of art. * * * * * * * * Arrived at the height of his reputation, Dupuytren desired to be not the chief but the tyrant of French surgery; and the most exclusive and bitter individuality dominated his entire being, dictated his thoughts, and governed his actions. So, in his social relations, he much preferred unexacting friendships and passive unconditional devotion.

"Justice obliges us to add, that the high faculties of this great surgeon were not those which may be termed transmissible. He had not a genius for discovery, but for application. This leaves but little harvest for succeeding generations to reap. From such great talent and renown one would expect an imposing succession of works which would add to the acquirements of science, and long leave their traces there. Alas! what remains to us of Dupuytren, what his biographers will carefully collect, is not to be compared with that which we had a right to hope for. It is this which has given rise to the saying that, after all, this great man was only a famous man. * * * * * * * In studying this celebrated surgeon with care, we may, to a certain extent, explain this defect of accordance between his labours and talent, and the insignificant amount of results. He possessed much more of the talent for teaching, and all that required the aid of speech and action, than for slow, continuous labour. Next, he had embraced the whole circle of surgery. He was not one of those who devote their lives to the production and realization of an idea. He wished to know and distinguish himself in every part of his profession, whence it resulted that he only contributed here and there to the progress of some portions of surgery. Add to this, that, desirous of becoming the first in all, he wished to accomplish this by the aid of a large fortune, and the amassing this occupied him incessantly, especially as this was a natural bent of his disposition."

A lofty pride seemed to have been his distinguishing feature, which accompanied him in every act and word, and the inordinate ambition hence arising doubtless was one of the causes of his merit and renown. To serve this ambition, however, he would condescend to almost any meanness and duplicity, so as to impart to its character, observed for any length of time, the most strange contrasts. When his object was to be answered by it, it was painful to see "the insinuating air, the wheedling suppleness of so proud and irritable a man." He was surrounded by flatterers, but he was also exceedingly susceptible to the attacks and criticisms his own disdainful conduct provoked.

"Criticism irritated him as an injury, while blame tormented him as an intolerable novelty. Admirers and acquaintances he had, but few or no friends, and found the surgical crown, like others, was not without its thorns. His genius, talent, and dignities were envied, but no one was ignorant that beneath this brilliant surface were anguish and chagrin. He was looked upon as a sort of power whom one must prevent from becoming an enemy, but with the impossi-

bility of ever becoming his friend. Every one stretched a hand to him, but no one shook his cordially. He was at once admired, feared, and blamed—a singular destiny for a man of such incontestible superiority. Let us add that, like other proud, susceptible, and irritable beings, he was unequal and changeable. Literally, to use the vulgar expression, one did not know which end to lay hold of him by. He did good and evil by starts and caprice. If, in some cases, he might be taxed with falsehood and harshness, in others, he exhibited an exemplary loyalty and unheard-of disinterestedness. Although he felt little disposed to oblige those who surrounded him, we find him one day rising at four in the morning to go to Bicêtre to solicit the favour of Murat for a friend. This strangeness of character equally influenced his general bearing and even personal appearance. Who does not recollect his strange, old-fashioned gaiters, his everlasting, worn-out green coat, his seldom-shaved beard, his apron up to his armpits, and the roll which he munched while driving from place to place in a shabby hackney-cab?"

With all this affectation of simplicity he coveted scientific distinctions, and was especially fond of his title of *Baron*. Probably from the obstacles to his progress which he found his poverty to offer in early life, he acquired an insatiable love of money, and became immensely rich. Having heard that Astley Cooper possessed six millions of francs he would never rest until he had accumulated the like. To this end he sacrificed all the time that might have been devoted to the advancement of science; and even his Clinical Lectures would not have been preserved but for the care of some of the auditors. Dupuytren himself published nothing, except a wretched letter in the newspapers of 1832, announcing decoction of poppy-heads a cure for the cholera! His riches, however, brought no satisfaction, but only the desire to add to them; he was not organized for happiness and never possessed it. He died aged 58. His will, M. Parise says, does high honour to him, but he does not state its dispositions.

Broussais.

Entering life amidst the turbulence of the French Revolution, the early part of the career of Broussais was stormy and uncertain. Soldier, corsair, hospital-clerk, navy-surgeon, student, civil practitioner, and lastly, military surgeon in the Napoleon campaigns, his existence was destitute of repose. Nevertheless, robust in body, firm in determination, and gifted with great aptitude for work, he pursued his studies in spite of every fatigue and difficulty. He eventually came to Paris and became a pupil and an intimate of Bichat, and an ardent admirer of the opinions of Pinel. His treatise *Des Phlegmasies Chroniques*, which had cost him immense labour, fell almost still-born from the press; but, although he felt the disappointment in the keenest manner, he concealed it, and pursued his indefatigable researches in Spain and Italy, examining thousands of bodies, accumulating a vast mass of facts, and meditating over them for years.

"It was in 1815 that he commenced his private course. It was only two paces from the Faculty that he cast forth his haughty defiance and raised altar against altar, doctrine against doctrine. From the first he announced himself as a reformer of science, proceeding by physiological demonstration to a complete rebuilding up of the structure of medicine. And let it not be supposed his course resembled any other of this kind, that is a simple and calm exposition, methodically elaborated, of the principles of the art. No, it was literally a true arena

where the professor fought alone and to the death. It was on his part an exhibition of harshness and frankness, an aggressive and ill-boding discussion of received theories, a continued explosion of recrimination against the science of formerly and its constructors, a constant affirmation of his own doctrines, a haughty and disdainful treatment of those of others—the like of which had never yet been seen. Then, the choleric argument, the passionate tone, abrupt gestures, the thundering voice of the professor, and his animated countenance, for he always seemed as if illumined with inspiration, added much to the effect of his addresses. The numerous and crowded auditory kept the deepest silence, the more as their attention was always, so as to say, enslaved, because it was continually excited. * * * * Broussais joined to a great penetration a very cultivated mind; and thus we may remark in his lectures, in his reiterated appeals to the progress of science, a skilful mixture of truth and error, a certain art of presenting the latter with the probabilities of reality. Although exclusive in his ideas he announced solid principles. Not content with observing and collecting many facts, he knew how to compare them, and deduce from them consequences often strained, but at other times undoubtedly just. To consider science under aspects until then unknown; to open a new horizon to ulterior researches in the study of diseased organs; to state his principles in an exact and precise manner; to reduce them to the rigour of a theorem; such was the end he proclaimed, and which he flattered himself he attained. So much knowledge and application, such high pretensions, united to the fire of conviction which seemed to animate him, to that ardour of proselytism, to that perseverance in self-glorification, to that fanatical esteem of his system, perpetually announced as the complete *resumé* of the entire range of medical truths, gave to the lectures of Broussais an astonishing popularity. Auditors crowded and pushed their way to them as to a theatrical amusement, and, more than once, pupils, who were unable to gain admission, might be seen taking their notes in the sun or rain when the loud tones of the professor allowed them to seize the purport of some of his observations."

In 1815 his *Examen* appeared. In this, the opinions of his adversaries were handled with the bitterest sarcasm and irony, and his own doctrine developed with surpassing skill. His success was prodigious. Not only were his dogmas received with enthusiasm by the schools, but even the convictions of elder practitioners were much shaken; so that the advent of a new era of medicine was proclaimed on every side. The deceptive simplicity of the system, and the magical words "progress" "advancement of science," "new doctrine," contributed immensely to its popularity; and those who dared attempt to oppose its reception were covered with terms of reproach and contempt. It is not surprising that Broussais became intoxicated with the incense constantly poured out before him, and imagined himself exalted into supreme judge of what should or not be received as sound doctrine. Yet, at the end of a few years, it was discovered that he, like many other reformers, was more potent in criticizing and destroying present structures than in raising new ones; and that the system of the man who had been so ready of visiting those of others with sarcasm and contempt, was not itself proof against the shafts of the critic. Remarks, criticisms, exceptions, and objections now multiplied on every side; but we need not follow the exposure of the errors of a system which never obtained much currency here, notwithstanding the Gallomania which some of our Journals endeavoured to foster. M. Parise exhibits concisely not only the unscientific character of the principles laid down, but the bad effects which too frequently flowed from their practical adoption; for

a practice which had been borne by the robust young soldiers coming within the sphere of Broussais' observation, was ill-suited to the feebler constitutions so often met with in civil practice. Once submitted to a rational examination, the system rapidly declined in estimation; and even when in the possession of an official professorship its constructor could not raise it from the neglect and disfavour into which it had fallen. His vehement denunciations against the older systems no longer drew the crowded auditories of heretofore, and the few pupils who attended his lectures seemed to do so more as a curiosity of by-gone days than as a reality of the present. Whether from faith in its truth, or from the desire of drawing upon himself the public attention which had so notoriously flagged, Broussais next became a propagator of phrenology. Again he had a large and applauding, if not a judicious auditory: "but the concessions he made to his adversaries were so numerous, that Gall would hardly have recognized his own doctrine in his discourses." He adopted for the defence of this the same paradoxical arguments, the mixture of error and truth by which he had sought to defend the doctrine of irritation. It will not seem surprising that Broussais only attained a rank in the practice of the profession very disproportionate to his merits and reputation. His exclusive mode of viewing cases so limited his range of therapeutical indications, although the force of circumstances sometimes obliged him to make concessions. One of those related of him is amusing enough. A patient having declared to him that he could support his regimen no longer, and that he was literally dying of hunger, Broussais reflected for a moment and replied, "Very well, you carnivorous animal, I will satisfy you," and ordered him a spoonful of broth in a glass of water. However, concessions of any kind were wrung from him with difficulty, for he prided himself upon his inflexibility to objections or adversaries. Nevertheless, after venting the most bitter sarcasms on places and riches, he allowed himself to accept some of these, and the great agitator was found lecturing in the professor's gown, and placing six lines of honourable titles after his name in one of his works!

Regarding Broussais with impartiality, uninfluenced by the excessive praise of his admirers or the bitterness of his detractors,

"We shall find he was a man of rare capacity and incontestible merit. It will be allowed that he has rendered services to science and has proclaimed truths which will endure. Nevertheless, if he appears great, the narrowness of his views is striking when compared with those of Bichat. This is because he was too exclusive, too absolute, in his ideas, because he has forcibly referred facts to two or three principles only, that is to either too few or too many, and because he has, with a view to his system, proclaimed the true and the false with an equal ardour. In the place of pruning, adding, and extending, he was desirous of radically destroying and re-constructing; and, at a period when doubts were raised, he struggled for the most formal materialism. Thus, although he has shed a luminous ray on science, although he proclaimed himself its master, his opinions have been attacked, his pedestal shaken, and his titles to true glory strongly contested. What remains of him now? Has Broussais done what he ought and could? Has he fulfilled his mission worthily and completely? Posterity, that high court of appeal for reputations, will decide in time to come. She will be just, yet severe; for, according to Scripture, 'much is required from him to whom much is given.'"

Chervin.

Nicholas Chervin is one of the men of science of whom France has just cause to be proud, and we regret our limits forbid our availing ourselves at any length of the glowing picture of his devotedness furnished by M. Parise.

He was intended for commercial pursuits, but happening to accompany a friend at Lyon to an anatomical lecture, he was so delighted with the lucid description furnished of the difficult bone, the *sphenoid*, that he at once resolved to devote himself to the study of medicine. He entered the army, but at the Peace came to Paris. At a breakfast party consisting of medical men, *the contagiousness of the yellow fever* became the object of debate, and Chervin observed that, as far as he had read on the subject, he disbelieved in its contagion. "What can you know about it?" replied one of the company. "I can furnish some proofs in support of my opinion," said Chervin. "Truly," added his adversary, "these will never be obtained on the banks of the Seine." Chervin felt himself hurt by this sally, and to such an extent that, from that moment his money, time, labour, and entire life were devoted to the elucidation of this question. At a period when the yellow fever was an object of dread amongst medical men, he at once sailed for America to visit the enemy in its sources and fastnesses, and the narration of his labours, exertions, and dangers, would occupy pages in their recital. Wherever the disease had prevailed or was prevailing he bent his steps, no matter at what cost of time or risk of health, and took up his temporary residence in even the most pestilential districts. Frequently he tasted the black vomit, and he arduously pursued anatomical investigations under a tropical sun, with a temperature of 106°. Nothing alarmed, nothing arrested him. He accumulated an immense mass of authentic documents properly testified, and returned to Europe. The yellow fever, however, then breaking out in Spain, he at once repaired thither, and thence returned to Paris, bearing with him his documents, and the unalterable conclusion, to the practical carrying out of which he devoted the rest of his life, "that the yellow fever is of paludial origin, its violence and gravity resulting from severity of climate; that it is not contagious; and that lazarets and quarantines should be considered onerous to governments, and especially injurious to commerce."

His desire was to bring his documents enforcing this doctrine before the public, and to enable him to do this he was content to submit to any privation. He took a wretched apartment in Paris for *six months*, and inhabited it for nearly *nineteen years*. He knew the interests and prejudices he had to contend with, but never despaired of ultimate victory. He was always prepared for every adversary, and overwhelmed him with documents, proofs, and authorities, whose refutation was not a little difficult. The truth before everything was his maxim, and he refused to bend to any authority in opposition to this. A man of this stamp, active in the pursuit of what he considered a duty to humanity and to the well being of his country, and who advanced his arguments with the bold tones of one convinced of their unalterableness, of course met with numberless opponents. He was denounced as a visionary, an innovator, almost as an anarchist. "I am strong enough," he proudly exclaimed, "to brave injustice, ingratitude, ill-nature, and even poverty, that implacable enemy to social

man. I can support all this." This was no vain boast. He spread his essays, reports, letters, memoirs, &c., on every side; and eventually he was compensated by the Academy, after a long and vehement discussion, affirming his propositions. A few years after, the Monthyon Prize was decreed him, and in 1832 he was chosen a member of the Academy by a large majority, the editors of several journals also freely opening their pages for the propagation of his opinions. He had the pleasure, also, of finding the Government modifying their quarantine system in 1835 and 1839, and the still greater one, of finding opinion setting in stronger and stronger against the system altogether. But he was not the man to be satisfied with half success, and continued his efforts as perseveringly as ever to the last. He lived to witness no further results of his labours, nor even to present his various documents and publications in any luminous body of doctrine, his time being, indeed, chiefly occupied in its active polemical defence.

"Chervin was, indeed, one of those models of severe morals of ancient times. His moral organization was quite different from that of the ordinary type. We may say he was an honest man in all the greatness and magnificence of the expression; for he possessed that golden flower of good faith, rectitude, and judgment which forms its base and root. When he was seen to labour, demand, solicit, even intrigue, to spare neither distance, trouble, writing, or expense, who would not have believed he was acting for himself and his own affairs, in order to obtain some reward or lucrative employment? Nothing of the kind: the object of so much care was the public good. He was concerned with the advancement of science, the enlightenment of authority, the economizing the finances of the State, and the saving of millions to commerce, while he himself was in want of everything! So will his life always be a great example for men who fully devote themselves to the good of the human race, and the culture of science. There will be found an example of self-abandonment, of devotion the more striking as it is original, and so to say *sui generis*. * * * * * Having neither patrimony, place, practice, nor powerful protectors, simple in his tastes, content with little, uniform in his life, he had no other affection, no other passion than that for the yellow-fever, which he had, as it were personified, and which was called his companion or *heroine*. All else was equal or indifferent to him. Thus he wanted for everything, for the small pension from the Academy was his only resource. During his latter years, he had to struggle with the three greatest afflictions of humanity—disease, old age, and poverty, triple Euminides, against whom men are usually devoid of energy. But Chervin was always the same. Adversity found him armed with strength and patience; there was nothing changed in his views, his habits, or his labours. No complaint or recrimination escaped him, whether in depreciation of what is, the elevation of what ought to be, or against men the most favoured by fortune. Never did he manifest jealousy or envy of his brethren, or cast a longing eye upon an opulent position. Never was he exposed to the torture of that *medical bile* so acrid and so corrosive in some cases. Excepting as regards the scientific point of the *non-contagion of the yellow fever*, he knew how to suffer and be silent."

He resided in a miserable garret without even the necessaries of life; but who among us does not envy his honourable poverty, with his table and few chairs, his books arranged upon two planks, and his *enormous dusty trunk*, containing his precious documents, "collected at the price of his fortune, his rest, his health, and his life in every part of the globe," rather than the prosperous celebrity of some of his contemporaries? He was attacked with hemiplegia in 1842, and died the following year.

Larrey.

The character and services of Larrey are so well known and appreciated in this country, that we need borrow little from the sketch here drawn by his friend and colleague. It is always, however, pleasing to accumulate instances of the excellent conduct of the members of our profession; and we question if the *beau-ideal* of a military surgeon was ever more realized than in the character of this celebrated man.

"At the battle of Eylau he remained 30 hours without eating; and so hurried and ardent was he in dressing wounds and giving orders, that paralysis of the bladder was nearly induced. Friends, enemies, every one, might claim his services. He saw only suffering creatures imploring the succour of art. Degree of rank in nowise decided his preferences. No matter what the epaulette, he only saw the wounded man, whose blood, poured out for his country, was equally noble and precious. After having operated on Marshal Lannes, and visited Duroc, the intimate friend of Napoleon, he would dress with equal eagerness the wounds of the lowest soldier or of the conscript of yesterday. His colleagues even, always his dearest friends, never met with preference. Our honourable *confrère*, M. Tanchou, tells us that, when wounded in the battle of Montmirail, he was taken to Larrey's *ambulance:* 'Your wound is a slight one,' he said, 'we have only room and straw here for bad accidents. However, they may put you in yonder stable.' The gravity of a wound was in fact the only ground of preference in his eyes. * * * * * * * The more severe and doubtful an action became, the more the activity and resources of this great surgeon multiplied—his first and only thought being the succour of the wounded. During the highest raging of the battle of Waterloo, when the fire was continuous and murderous, the author of this sketch and Larrey met, when he exclaimed, 'My dear colleague, look to your wounded soldiers. Pay attention only to them.' He added, 'the firing is hot indeed, but let every one do his duty and all will go on well.' * * * * * Larrey had so little dissimulation, that one of his defects, perhaps the only one, was the high opinion he entertained of his own talent and works. He praised himself with such a good faith and confidence, but his weakness was the more excusable, as it was not without foundation, facts answering to words. Frequently a deep colossal pride, peculiar to celebrated men, is disguised without, and hidden with care; but Larrey concealed nothing. All his thoughts and words were without disguise, for he knew not how to conceal his pride at the bottom of his heart, like some who are termed modest. Oftentimes he raised a pedestal to himself with the same simplicity, the same confidence, as if he had performed the most virtuous action or the greatest sacrifice. We may sometimes smile at his simple and frank want of modesty, and at the same time admire his noble heart full of rectitude, honour, probity, and courage, a stranger to the manœuvres of personal interest, concealed under the veils of philanthropy and love of science. What he delighted in, above all things, was to protect and assist others by his counsels and his credit; so that he always regarded surgeons whom he had known, and who were a little younger than himself, as his pupils. In writing to the late M. Coutanceau, then professor at *Val-de-Grâce*, Member of the Academy, and author of different works, he never omitted concluding with 'your affectionate brother-in-law and former professor.' This was not pride or the desire of superiority, but a bond of patronage, goodwill, and reciprocal attachment, which he delighted in tightening and continuing."

In perusing the accounts of the lives of late and present members of our profession in France and in our own country, we have often been struck with the far larger proportion in the former nation who have raised

themselves from indigence and comparatively low conditions to the highest position of professional eminence. And in the facilities which that country affords to her industrious and enterprising youth, destitute of family connections, pecuniary resources, or hospital influences, for attaining the highest offices in her power to bestow, we see an example well worthy of imitation in our own country, where a very different system of selection, or rather of succession, prevails.

An Anatomical Description of the Diseases of the Organs of Circulation and Respiration. By *Chas. Ewald Hasse*, M.D., Professor of Pathology, &c., at Zurich. Translated and Edited for the Sydenham Society, by *W. E. Swaine*, M.D. London, 1846. 8vo. pp. 400.

Elements of Pathological Anatomy. Illustrated by coloured Engravings, &c. By *Samuel D. Gross*, M.D., Professor of Surgery in the Medical Institute of Louisville. 2d Edition. Philadelphia, 1845. Royal 8vo. pp. 822.

Inasmuch as it is our vocation to bring before the notice of our brethren whatever is of interest and importance in connection with the medical literature of the day, we have always considered that our readers may fairly claim from us some notice of works which, though not published, are issued by a society established expressly for the purpose of benefiting the profession by the diffusion of sound medical literature. We do not, however, consider it our duty, nor is it our intention, to criticize the proceedings of the Council of the Sydenham Society—that they have a delicate and difficult task to perform all must admit, and few will be disposed to refuse them full credit for an anxious desire conscientiously to discharge the onerous duties that have been confided to them. We hail with heartfelt satisfaction the present flourishing state of so important a Society.

The present work we have carefully examined, and hesitate not to express our conviction of its great intrinsic value and practical interest. Although our continental neighbours have led the way in the study of Pathological Anatomy, there has of late been no lack of successful labourers in this department of medical science among ourselves. But hitherto no work of a precisely similar character to the present has issued from the British press. None which appears so calculated to make morbid anatomy subservient to its true and only really useful object, the history of disease —a knowledge of which is essential to all rational treatment. Professor Hasse's work is not a mere dry detail of the morbid appearances presented by the various tissues and organs of the body, nor, as his editor states, a mere descriptive catalogue of curiosities in morbid anatomy. The author's aim has been to make the actual knowledge of pathological anatomy subservient to an "anatomical history of disease," and thus to render it of practical utility at the bed-side.

The advantages which Professor Hasse has possessed for the preparation of such a work as the present, appear to have been considerable, and of these he has manifestly availed himself to the utmost. As a diligent student in the hospitals of Paris and Vienna, and subsequently as clinical assistant to Professor Carus, and pathological prosector in the principal hospital at Leipzig, he possessed the means of observing and collecting materials for himself, whilst, at the same time he was forming that "pathological collection," which, under his auspices, has grown into a most interesting and valuable Museum. The present treatise, therefore, differs essentially from what is commonly called a compilation. For although he has "not relied solely on his own investigations, but has largely availed himself of facts recorded by others," he has been chary of making use of other men's experience. This, perhaps, may account for some apparent deficiencies. The estimation in which the book is held in Germany is sufficiently attested by the fact that since its publication the author has had the offer of the chair of Clinical Medicine in five Universities, and now holds that vacated by Professor Schönlein at Zurich.

Before proceeding to give our readers an account of the work itself, we have only to add, that the author's reputation does not appear likely to suffer at the hands of his English translator and editor. The translation is, as a whole, very well executed. And although occasionally the version would have been improved by a less literal rendering, and further use of the pruning knife, Dr. Swaine has acquitted himself most creditably and honourably in what appears, from his preface, to have been a labour of love. The getting up of the book, like that of most of the Sydenham Society's publications, is beautiful.

The allied work, of which the title stands at the head of this article, is of a very different character from that of Professor Hasse. It is a ponderous tome of between 8 and 900 pages royal octavo, of transatlantic origin, emanating from one of the American Universities. As it has arrived at a second edition in the course of a few years, it may be presumed to be held in some estimation by our American brethren. From the author's preface it would indeed appear that it is the only system of pathological anatomy of American origin, and that this branch of medical science has hitherto been very much neglected on the other side of the Atlantic. We doubt not, therefore, that the author has conferred an important service on his countrymen by the compilation of this work. As, however, it is little else than a tolerably well executed compilation, having few or no pretensions to originality, either of design or execution, it comes before us under very disadvantageous circumstances, after the perusal of the delightful and original work of Hasse, which is beautifully written and marked throughout by a philosophical spirit and critical acumen that cannot fail to strike and captivate as well as benefit every careful reader. Moreover, there is in the work of the American Professor much that we cannot but consider as irrelevant matter. Lengthened introductions on the normal anatomy of the organs and tissues prefixed to each section treating of their morbid conditions are certainly out of place, and needlessly swell the size of the volume. Nor is it consonant with the title of the work, "Elements of Pathological Anatomy," to introduce disquisitions on such subjects as the general doctrines of Inflammation, to say nothing of many others which

are rather physiological than pathological. We hold it to be a duty in these book-making days to protest against this encyclopædical method of treating all subjects. With these preliminary remarks, we proceed to give our readers an account of the two works. We shall, however, take Professor Hasse as our guide, and our remarks will principally have reference to his work.

The present volume of Hasse's Pathology treats of Diseases of the Organs of Circulation and Respiration only, and is divided into two Parts. The first consists of five Chapters, comprising the diseases—1, of the Lymphatic Vessels and Glands—2, of the Veins—3, of the Arteries —4, Heterologous Formations in the Circulating Organs—5, Diseases of the Heart.

Having pointed out the general participation of the lymphatics in all inflammatory diseases, and that the inflammatory process is originally developed in the contiguous cellular tissue, and is thence communicated to the walls of the canals—that in consequence of the check presented by the glands, the diffusion of morbid products is much less frequent by the lymphatics than by the veins, that it is not till the irritating matters have reached the lymphatic glands, and have undergone organic assimilation that inflammatory re-action is established, Hasse proceeds to demonstrate how the lymphatic vessels are influenced by the vitiated condition of their contents. The first condition requisite for that septic inflammation which attends the absorption of morbid matters, is asserted to be a liability to transudation of the morbid contents through the parietes of the lymphatic vessels. This species of exosmosis, he thinks, is induced "partly by the immediate action of the irritative matter on the membranes of the vessels, partly by the clogging and distension of the latter, in proportion as the lymphatic glands become less permeable to the affluent lymph." Thus we find the surrounding textures saturated with the transuding fluid, at first limpid, afterwards turbid and becoming converted into pus, in the midst of which the lymphatics are found, as thin knotty cords, with thickened coats, and their internal membrane no longer smooth.

As the suppuration spreads, the lymphatic vessels are destroyed, and circumscribed abscesses are formed. In this way inflammation extends from one group of glands to another, and occasionally to the trunk of the lymphatics, even into the veins, when we have all the general consequences, of the admixture of pus with the circulating current.

The lymphatic glands frequently become inflamed, either separately, or in conjunction with the lymphatic vessels. The affection may be either acute or chronic, and of a traumatic, rheumatic, or septic nature. It may occur sympathetically from disease in neighbouring organs, or from pseudo-formations, *e. g.*, tubercle within the glands. Having briefly described the progress and effects of inflammatory action in the glands, he observes that inflammation of the lymphatic vessels and glands is a sequence of very various diseases, but especially of such as result from the introduction of noxious matters, or which represent some constitutional cachexia. The following are mentioned as the most important ways in which the lymphatic system is involved—from "poisoned wounds," "the plague," "typhus," "syphilis," "porrigo," and "elephantiasis." The inflammation of the occipital and other glands in connection with extensive porrigi-

nous inflammation of the scalp, is admitted by Hasse to be of doubtful specific character. But, for our own part, we see no reason whatever for assuming that there is anything specific in the character of such glandular inflammation. It may be seen in connexion with any extensive cutaneous disease of the head. It is true that such glandular affections may, as in the case of buboes, be of two different kinds, the one resulting from the direct reception of the virus, the other of purely inflammatory origin. The pus of the former, according to Ricord, is alone capable of transmitting syphilis.

Of dilatation of the lymphatics, whether partial and assuming the appearance of hydatids, or general, neither Hasse nor the American professor is prepared to offer any explanation, except in those cases where it is manifestly the result of mechanical causes.

The first Section of the 2d Chapter of Hasse's work is devoted to Phlebitis, which he very properly adduces as an example of the important services rendered to practical medicine by the study of pathological anatomy. The true nature of various, so termed, malignant intermittents, typhoid conditions, puerperal diseases, and the like, has been revealed by the discoveries connected with phlebitis. Inflammation of veins is naturally considered under two heads, the one comprising the purely local, primary phenomena, the other the general secondary consequences diffused throughout the whole system. The description which Hasse gives of the appearances presented by an inflamed vein is carefully drawn, and is a good specimen of the author's style. The discolouration of the lining membrane of an inflamed vein is said to differ in no respect from the redness of imbibition, except in the mottled alternations of its various shades. But the surrounding cellular tissue early "exhibits an incipient infiltration of a faint-red serous fluid, together with a dense network of delicate little vessels, which, in the larger veins, are distinctly seen to extend to the cellular coat of the vessel. A double consequence now appears to ensue, —and in most instances very rapidly: namely, deposition of an inflammatory product within the canal of the vein, and a change in the blood itself. For, as the morbid blood vitiates the membranes of the vein, producing inflammation, so, in like manner, these membranes, when once inflamed, exercise a reflex action upon the blood; the first sign of which is, probably, the inflammatory redness of imbibition already described. The further changes consist in the formation of fibrinous deposit, at first loosely connected with the internal membrane of the vein by means of a tenacious mucus-like substance, but which subsequently adheres more and more firmly through the medium, as it were, of cellular tissue." P. 12.

After describing the subsequent processes which follow the formation of the plug by which the vein is, for a time at least, obliterated, our author lays great stress on the importance of the changes taking place in the surrounding cellular tissue. This becomes condensed, assumes a brawn-like character, and is intimately connected with the external coat of the vein, which is thus kept permanently distended, so that when cut through its calibre remains patent, like that of an artery. In dilatation of the veins a similar state of things exists, and the danger of pus passing into the circulation after the excision of hæmorrhoidal tumours, &c., will be pro-

portionate to the number of vessels in this state of permanent patency within the wound.

Although the question, how pus is formed within inflamed veins, belongs, strictly speaking, to general pathology, its importance, in connexion with the topics under discussion, has induced the author to give a brief account of the present state of our knowledge on this subject, and he appears to decide for the view which supposes the pus-globules to be the result of changes in the formation of epithelium-cells, and not modified blood-corpuscles. The latter, however, are altered in form, and cling to one another and to the sides of the vessels. The mingling of pus with the blood causes its coagulation, the contents, moreover, being unceasingly propelled towards the heart, the more or less solid products of the inflammation are conveyed beyond the original site of the morbid action; so that we cannot always decide that the part of a vein at which we may happen to find a pus-coagulum is the true seat of the disease, for this is frequently remote and difficult to discover. The pus, however, more usually becomes isolated by the coagulation of the blood, and the consequent impediment to its circulation, and this constitutes what Cruveilhier has termed sequestration of veins. It is only when this sequestration is prevented, or is imperfect, and the pus and softened fibrin pass into the current of the circulation, that we have the serious constitutional typhoid phenomena. The further organic changes consequent on this morbid impregnation of the blood are divisible into such as occasion a stagnation and interruption of the sanguineous current in the central portions of the vascular system, and such as have their seat in the capillary system. But that something beyond the mere presence of morbid matter in the blood is necessary to account for the coagulation and interruption of the current of the blood is manifest from the observations of Glüge, viz., that pus-globules are found in the blood contained within the heart in phthisis, and yet little or no coagulation occurs. The occurrence of lobular abscesses, or purulent deposits in the lungs, &c., Hasse does not hesitate to ascribe to phlebitis, which, in many instances of external injuries and operations, may readily be overlooked. He admits, however, that sometimes there may be direct absorption through the open mouths of wounded or ulcerated capillaries. But although we must, with our author, admit that no such thing as absorption of unaltered pus through the sides of the capillaries takes place, yet, from the experiments of D'Arcet and Sedillot, there is every reason to believe, that some of the principles of putrid or altered pus may thus be absorbed, and, moreover, that the occurrence of typhoid symptoms may be dependent, not so much on the mere existence of pus in the blood and the consequent purulent deposits, as on the altered or putrid character of the disintegrated pus-globules, or of the serous and albuminous principles of the pus. Neither Hasse nor Gross, however, makes any distinct reference to this, which we consider an important part of the question. It appears also highly probable, from D'Arcet's experiments, that by exposure to the influence of oxygen in the lungs, pus, which may have been conveyed thither in substance, may undergo there those changes on which the occurrence of typhoid symptoms depends.

There is, at all events, an important distinction to be drawn between those cases in which we merely have lobular abscesses from mechanical

impediments to the capillary circulation in the lungs or liver, and those in which, from the first, there is marked depression of the system, and rapid supervention of typhoid symptoms. But we have occupied so much space with this subject, that we must pass over the remainder of this and the next section, including puerperal phlebitis, phlegmasia dolens, and obliterations of veins, merely calling our readers' attention to an important variety of phlebitis, viz., inflammation of the sinuses of the dura mater. The anatomical characters of this resemble those of ordinary phlebitis. It is mostly a secondary affection, but it may occur as a primary disease. Death ensues very speedily, Hasse observes, amid the phenomena of functional disturbance of the brain. A very interesting case of primary inflammation of the right cavernous sinus has however lately been described by M. Breschet of La Charité, and is mentioned in a recent report on Pathology and Practical medicine, by Dr. J. R. Bennett. The primary symptoms were, shivering and intense cephalalgia occurring after exposure to wet and cold. These symptoms were succeeded by pain in the eyes, photophobia, pain in the right shoulder and neck, and over the orbits, with œdema of the right conjunctiva. Delirium, dilated pupils, and typhoid symptoms preceded death, which did not occur till the 31st day from the beginning of the illness.

The dilatation of veins, which Gross dismisses in a single short meagre paragraph, Hasse devotes an entire section comprising 20 pages of his second chapter. Phlebectasis, or Dilatation of Veins, he believes to be one of the most frequent morbid changes of structure occurring within the human body. There are three varieties of disease which, anatomically speaking, are referable to dilatation of veins—varicose veins, properly so called, varicocele, and hæmorrhoids. The chief and common source of all these varieties is referred to a peculiar habit of body, a morbid predominance of the venous system. Exceptions, however, may, we think, be taken to this view, as well as to that which admits all these varieties to possess an hereditary character. No doubt, in many instances, there is a peculiar disposition to dilatation of the veins, without, however, much evidence of this being referable to any predominance of the venous system. It is remarkable, as Hasse states, that in men, the subjects of varices of the leg, the disease usually arises from the trunk or the principal branches of the saphena, whilst, in women, it commences in the minutest cutaneous twigs. An excellent and lucid sketch is given of the attendant anatomical changes and the consequences of varices of the leg, which our limits compel us to pass over.

All hæmorrhoidal tumours are considered by Hasse as referable to dilatation of veins, but Gross admits two varieties, one of which consists in the extravasation of blood into the surrounding cellular tissue from rupture of a hæmorrhoidal vein. Andral, Lobstein, Puchelt, and Hodgson, with other high authorities, coincide with the opinion adopted by Hasse, in support of which both he and Brodie assert that it is in general easy to inject, from the inferior mesenteric artery, piles not in a turgescent state, and that the reason why Chaussier, Recamier, and others found the injection pass merely into the cellular tissue was, that the piles were, at the time, in a state of erethism, or filled with coagula. There is, however, a tumour of a very different character from the true hæmorrhoidal, though

often confounded with it, which Gross has well described. "It consists of an effusion of serum in the sub-mucous cellular tissue, in the form of a rounded, or ovoidal elevation, from the volume of a cherry to that of a small hickory nut; it is of a pale yellowish colour, or nearly perfectly white, almost transparent, glossy, and very much like the vesicle of a common blister. It pits slightly under the finger, and generally attains its full size in ten or fifteen hours. A few delicate straggling vessels, the colour of which beautifully contrasts with that of the other parts, may often be seen, ramifying over its walls, or intersecting its substance. The tumour, although sometimes multiple, is almost always solitary. The disease is strictly analogous to œdema of the vulva or glottis."—*Gross*, p. 625.

The following is Hasse's account of the difference between arteries and veins, in regard to their susceptibility of inflammation and the occurrence of secondary symptoms. After referring to the experiments of Trousseau and Rigot, to show that only the most powerful irritants injected into arteries will excite inflammation, he says—

"This may be accounted for by the different structural relations of arteries and veins; in the latter every irritant quickly penetrates the internal membrane, and the peculiar, but very thin, felt-like fibres of the so-called middle tunic, so as to reach the cellular coat; in the latter, there is interposed between the internal non-vascular membrane and the cellular coat (rich in blood-vessels and nerves,) a pretty strong layer of elastic texture, possessing a very low degree of vitality, so that the cellular coat is endued with all the vegetative and animal activity of the artery. Accordingly, this latter coat is highly susceptible of every irritation, and readily inflames when acted upon by either chemical, dynamical, or mechanical influences." P. 60.

"The secondary symptoms," he observes, "which play so decided a part in phlebitis, are very seldom observed to follow arteritis. This cannot be attributable to death ensuing earlier in the latter than in the former disease; for in phlebitis lobular abscesses frequently form with great rapidity, whilst in some cases of arteritis the fatal issue is deferred until a very late period. The real cause seems to depend upon some peculiarity in the arteries, in virtue of which plastic matter only is deposited within their canals, and inflammation restricted to the adhesive form. Hence, the more solid product of morbid exudation is not under the control of the heart's impulse, and can alone pass into the circulation when the exudation and the coagulated blood, or the fibrin deposited by the blood, are too scanty to close up the arterial calibre completely, or for a sufficient length of time. This peculiarity manifests itself most distinctly in the tying of arteries, after which operation, notwithstanding the long sojourn of a foreign body, there results a firm plug and adhesion, unless from a concurrence of extraordinary circumstances, diffuse suppuration is induced, which then commences in the surrounding parts and in the cellular tunic, and only *indirectly* invades the lining membrane of the artery,—the plug being originally *always* of a purely plastic nature. In simple arteritis the circulation in the involved artery is, in like manner, mechanically arrested, up to the nearest diverging branch, by the product of the inflammation, just as it is from the application of a ligature. Should suppuration ultimately occur even *under these circumstances*, it remains purely local,—shut out from the general current of the circulation, however far it may extend downwards in the direction opposed to that of the heart. Should the purulent softening extend upwards, so as to destroy the closing plug, the blood-stream will find its way back to the inflamed portion. Here, however, the artery is no longer entire, since the suppuration usually begins in the external coat, and in its progress destroys the others. Thus the blood, meeting with no resistance, perforates to a greater or smaller extent the softened membranes. If

this happens with an artery to which a ligature had been attached, a fatal hemorrhage necessarily ensues, unless a fresh ligature be instantaneously applied higher up. In spontaneous arteritis occurring in parts previously sound, there ensues either extravasation, or a so-called *diffuse spurious aneurism*, the hemorrhage being restrained by the thickened and indurated cellular texture in the neighbourhood of the ruptured part. Such occurrences sometimes follow external injury; but in spontaneous arteritis they are so rare, that I can call to mind but a single authentic example, namely, the case before cited from Hodgson's work." P. 63.

Spontaneous gangrene in the aged, the gangrena senilis of Pott, is we think correctly considered by Hasse as the result of essentially the same disease as occurs in the earlier periods of life, viz., arteritis, and that the ossification of the arteries is the accidental and remote, rather than the proximate, cause of the gangrene. Gross also adopts the same view. This is a question of great practical importance, to which the attention of the profession has of late been especially called; and it appears to us that the symptoms during life, the comparative results of the old stimulating plan, and of a modified antiphlogistic treatment, together with the appearances observed after death, alike prove the correctness of the view adopted by Hasse. It cannot, however, be doubted that there is considerable pathological diversity in the various cases which have been described under the name of spontaneous gangrene, and that the true senile gangrene connected with degeneracy of the arterial coats presents distinctive features. A little work published a few years ago by Hecker of Stuttgard contains an excellent account of almost all that is known on this subject.

The remaining sections of this chapter are occupied with the subjects of atheromatous disease, and ossifications of arteries and aneurisms, of which a very instructive and succinct account is given. A short chapter is devoted to heterologous formations in the circulating organs. "No well authenticated example is known," says our author, "of the arterial membranes being the seat of heterologous growths. Whatever have been described as resembling such, have been either various forms of atheromatous degenerations, or else tumours intimately incorporated with the filamentous sheath. This immunity of the arteries has led to the idea of identifying atheroma with tubercle, a groundless assumption, seeing that the two substances are essentially dissimilar, and also that arterial degeneration occurs but seldom, and never to any extent in the tubercular habit."

The veins, on the other hand, it is well known, may become diseased in various ways, and the occurrence of fungoid, or lardaceous growths within the veins has excited much attention.

"Some refer it to the absorption of the heterologous matter by the minutest venous twigs, others to immediate deposition from the blood. The former view is at variance with physical laws. The primitive cells of those growths exceed the blood-globules in size; it is therefore inconceivable that they could traverse, supposing they could enter the closed capillaries, unless indeed the nuclei of the cells be reckoned the germinal principles of disease. Nor will the second view hold good until the proximate elements of fungus can be *proved* to pervade the whole sanguineous mass." P. 108.

That we have not as yet satisfactory proof of the latter theory, of the origin of cancer-cells in the blood, must be admitted, still we think that no satisfactory theory of either the physiology or pathology of malignant

diseases can be given, except by the admission that, the primary blastema may and does occasionally germinate within the vessels. This subject has been discussed by Dr. Walshe in his work on Cancer with great acumen, and he appears to have been compelled to come to the same conclusion.

In the fifth chapter, which treats of Disease of the Heart, our author gives due prominence to the various modifications of pericardial inflammation as influencing most materially the ulterior course of the disease and the consequences to which it gives rise. Both in respect of prognosis and treatment it is of much practical importance to be aware of the great difference that exists in the character and amount of effusion which attends pericarditis. The most intractable forms of pericarditis are referable frequently not so much to the violence of the inflammation as to the unorganizable character of the effused matter.

"The products of pericarditis are not always uniform, even in the same subject, but as before stated, the same exudation frequently contains various elements, which, by virtue of the ulterior changes wrought in them, modify the course of the disease in many different ways. This variable composition of the exudation may proceed from alternations in the intensity of the inflammation,—a rapid succession of inflammatory attacks giving rise in turn to serous and sanguineous —organizable and unorganizable effusion; or else it may proceed from some peculiar ingrained morbid predisposition to engender diseased products. Thus it not unfrequently happens that true tubercle, and even medullary fungus, is developed out of the exuded substances, as seen both by Kolletschka and by myself." P. 116.

The Section treating of Endocarditis is admirably drawn up, and most valuable. We regret that our limits prevent our extracting largely from it. Having indicated the distinctions between the various forms of fibrinous coagula found in the heart's cavities, he thus points out the means of discriminating "between redness of the endocardium from inflammation, and redness from imbibition; as also between polypous formations engendered by exudation, and coagula resulting from the inflammatory irritation of other organs."

"The inflammatory redness is almost always spotted,—pale and dark alternately; in one place more of a violet, in another more of a scarlet colour. The redness of imbibition is, on the contrary, more equable throughout, and darker, perhaps, where the blood makes a prolonged sojourn. The redness of imbibition is therefore almost invariably observed in the following descending order: darkest in the right auricle, paler in the right ventricle, with the exception of the valves of the pulmonary artery, which are as deeply coloured as the auricle; still paler in the left auricle, whilst the left ventricle often retains quite its natural tint, except that the aortal valves are darker; in the great vessels the posterior surface is strikingly dark in comparison with the anterior. Moreover, the endocardium continues smooth and polished, and in all other respects natural, whilst inflamed portions of it assume a dull, velvety appearance, and are sometimes thickened and softened. If besides the polypous coagulum the endocardial surface is invested with a soft, firmly adherent false membrane, the inflammatory state is no longer doubtful. At the valves, particularly the tricuspid and mitral, this membranaceous covering takes the character of soft, hemispherical granulations, capable of increasing in bulk, and in

some instances, as shown by Kreysig and Bouillaud, susceptible of organization. Secondly, these polypoid concretions can be regarded as products of endocarditis only when met with on the inner surface of the heart, in the manner before described, or when they cannot be traced to any other morbific condition. Here absence of inflammatory redness is not always a sufficient reason for denying their inflammatory origin,—seeing that the redness is very fugitive, more particularly in intense degrees of inflammation, when the colour of the membrane gradually from a dull and equable brown red becomes paler,—changing to a dirty grayish yellow. Carswell has figured a remarkable case of inflammatory polypus, in which the lining of the heart appears preternaturally pale. This polypus was partly of a grayish, partly of a lively red colour; it exhibited, both on its surface and in its substance, little drops and stripes of pus, had insinuated itself amongst the columnæ carneæ, and adhered as if glued to the equally pallid mitral valve. Legroux and others contend that the pus is the result of inflammation of the fibrinous layers, within which it is sometimes found to accumulate,—an error probably refuted by Bouillaud. Admitting the above granulations resulting from exudation to be susceptible of organization, it may still be doubted whether the polypous coagula are so. Otto could never detect in them any vascular development. It is true that they exhibit points and streaks of blood, sometimes extending into their interior, and which might certainly *pass for* the rudiments of organization; but it is not conceivable that foreign bodies of such dimensions could occupy the central organ of circulation without speedily inducing death. The few authenticated instances of organized polypi leave it problematical whether they were anything more than mere vegetations upon the valves; and vegetations there, as elsewhere, can hardly be reckoned among the immediate products of inflammation."

"The first and most constant change resulting from endocarditis relates to the endocardial membrane. This, after the aforesaid red spots and patches have disappeared, loses its smooth pellucid aspect, becomes relaxed, turgescent, and rough, puts on a grayish and dull appearance, and is capable of being stripped off with comparative ease. Rokitansky states that under these circumstances the endocardium frequently becomes the seat of irregularly-jagged fissures, which forthwith form a depot for a grayish yellow coagulum."

"When the inflammation assails more particularly the apparatus of the auricular valves, a still more frequent occurrence is the rupture of one or more of the papillary tendons. This is particularly common at the mitral valves. The filaments or shreds of such lacerated tendons curl up and form little elevations, invested with fibrinous coagulum and hardened lymph, whilst the valve, previously angular, recedes and acquires a broad raised margin." P. 128–130.

After observing that, according to Bouillaud's views, all defects of the valves, all ossifications,—in a word, all diseases of the heart's apertures, are directly referable to endocarditis, Hasse very justly says:—

"This view does not, however, fully correspond with facts; for, in the first place, experience shows that the several organic changes above alluded to, affect the left side of the heart far more frequently than the right; whilst, according to Bouillaud, endocarditis is observed with equal frequency on both sides, and with

most intensity on the right. In the second place, we meet with those indurations and ossifications, especially after the prime of life, too often to admit their having been universally preceded by endocarditis; in many subjects, indeed, the contrary could be proved. Thirdly, most of those changes present, in the mode of their development, so perfect a similarity to the progress of atheromatous disease of the arteries as to render it more appropriate to class them under this head.

"It cannot be denied, however, that, under propitious circumstances, endocarditis may be productive of thickening of the internal lining of the heart and of the valves, as well as of granulations and ossifications of the latter, though much less frequently than Bouillaud pretends. In fact, ossification, even when met with in young individuals, cannot invariably be ascribed to endocarditis. After careful consideration of all the circumstances, it would appear that the following organic changes arise from either a partial or a general inflammation of the internal surface of the heart: the milk-white or mother-of-pearl coloured patches and thickenings,—perhaps themselves the relics of earlier relaxation, disorganization, and scar-like puckerings,—of the endocardium; secondly, membranaceous deposits on the inner surface of the heart, more especially in the left auricle, and also in the left ventricle, upon the septum and about the origin of the aorta. These have the appearance of elevated, rough, and puckered connecting membranes. They are of a dull yellow tint, and are easily separated. To the same class belong adhesions of the valves amongst each other, and with the heart's parietes, rupture of the tendinous filaments of the auricular valves; and, finally, a state of partial destruction of the valves, with irregular notchings, and fringed appendices of variable length. This has heretofore been observed only in the semilunar valves of the aorta, and twice by myself in the pulmonary artery. Sometimes it is complicated with partial adhesion of the valves together." P. 132.

There are certain secondary phenomena produced by endocarditis, which are but little attended to, and of which Hasse thus speaks:—

"They are referable to the effusion almost always consequent upon that affection, in the various forms and localities already specified. A considerable portion of the effused substances being carried away in the first instance by the circulation, and another portion during the subsequent period of softening, it is obvious that these inflammatory products must, just as in phlebitis, act a subordinate part within the capillary system. The *spleen* and *kidneys* appear particularly liable to such changes. In the spleen, coagulated fibrin is found of some breadth at the periphery, but gradually tapering towards the centre, having mostly a tolerably sharp outline and a brownish-yellow hue. Smaller deposits, of a similar kind, are met with in the cortical substance of the kidney; sometimes even intersecting the papillæ. According as the inflammatory product of the endocardium partakes more of the character of fibrin or of pus, the secondary deposit will be of a more or less consistent kind, and shrivel in the event of recovery,—or else liquefy, and terminate in abscess. Though frequent in the localities above indicated, these secondary phenomena are proportionately rare in other organs, as in the liver for instance. Sometimes, however, they occur in the serous cavities." P. 134.

In the sections on inadequate valves and hypertrophy and dilatation of the heart, will be found an excellent account of these important morbid states and their pathological relations, with full reference to the labours of Kingston, Bizot, and other recent investigators. A just tribute is paid to the value and general correctness of the statements and views of Bizot. On the relative width of the pulmonary artery and aorta, our author says:

"I began, several years ago, to pay attention to the width of the pulmonary

artery relatively to that of the aorta. In E. H. Weber's edition of Hildebrandt's Anatomy, and in J. Müller's Physiology, it is asserted that the former is narrower by one third than the latter, and from this statement many physiological inferences have been drawn. Great was my astonishment, however, to find that, on the contrary, the pulmonary artery is somewhat more capacious than the aorta; yet, on repeating the trial in a multiplicity of cases, I invariably arrived at the same results. Whilst I was thus engaged, Bizot's comprehensive Researches were published, and proved far in advance of my own. Nevertheless, whenever I found time I continued my admeasurements upon a somewhat more comprehensive principle and improved method. My results, based upon 122 admeasurements, almost entirely coincide with Bizot's, and are richly suggestive in a pathological point of view. It should be stated that the measurements from which the general results are taken were made on subjects who, during life, had betrayed no symptoms of diseased heart." P. 156, *Note*.

There is, we think, frequently a very vague and incorrect notion entertained with regard to the relation between cardiac and hepatic disease. On this subject Hasse observes, "hepatic disorders effect inconsiderable passive dilatation in the instance of fatty heart only; they engender, besides, a thickening, only ascertained by measurement, in the walls of the right auricle and ventricle. Thus, in my tables I find almost invariably in the cases presenting the largest dimensions, short of actual hypertrophy, hepatic disease noted; and it appears that enlargement resulting from chronic inflammation, with or without fatty or wax-like degeneration, bears a much closer relation to diseased heart than scirrhosis and simple fatty degeneration. Liver-disease may, however, be a consequence, as well as a cause of disease of the heart. We rarely meet with disease of the heart, coupled with stagnation of blood in the right chambers and in the veins generally, without hyperæmia, and a flabby condition of the liver. Nay, this stagnation may go so far as to occasion extravasation of blood within the parenchymatous texture (Bouillaud.) The so-called nutmeg-liver is a very common sequence of hypertrophy and dilatation of the heart."

The phenomena connected with the respiratory organs, are among the most prominent and well-known symptoms of cardiac disease, but it is important to be aware that the pulmonary congestion is apt to merge in inflammation, and we have met with some cases that have strongly impressed us with the importance of being awake to this tendency, for the complication, it need hardly be said, is important and dangerous, and very apt to be overlooked amid the more common and habitual symptoms with which these deplorable cases are attended. On this point Hasse says—

"In several instances of considerable hypertrophy, I have seen the resulting pulmonary affection merge in inflammation. The inferior lobe, sometimes of the left, sometimes of both lungs, was displaced, shrunken, not crepitant, softened within a greater or smaller range, of a clear reddish brown colour, and of augmented density. On being cut into, the section was tolerably even, presenting only here and there those soft elevations which, in pneumonia, produce the well-known granular aspect; these, when aggregated in distinct groups (which was however seldom the case) were moister, more decidedly gray, (if not of a dirty yellow,) and much softer than the rest of the texture. While the whole remainder of the lungs was marked by an excess of blood, the portions alluded to contained more or less of a dingy reddish troubled fluid; circumstances all com-

bining to establish a peculiar modification of the second stage of inflammation of the pulmonary substance." P. 167.

There are two forms of fatty heart that are well known, the one in which there is an unusual deposition of fat in the cellular tissue betwixt the serous investment and the muscular substance of the heart, and the other where the fat globules collect not only within the compartments of the sub-serous cellular tissue, but are deposited within the muscular substance, even between its primitive fibres. There is, however, a perfectly distinct species of fatty heart, occurring almost exclusively in hypertrophied hearts, which exhibit at the same time the remains of earlier endocarditis and carditis. In this form, which was first described by Rokitansky, "the fat does not accumulate in masses, there being no fat-vesicles inclosed within fasciculi of cellular tissue, but is beaded, as it were, in minute microscopic granules, closely interlaced, and imbedded among the primitive fibres of the heart's muscles. Those primitive fibres have lost their transverse striæ; the fibrils are friable, and easily reduced to minute molecules. The whole heart may suffer this species of fatty infiltration, although commonly distinct portions alone are diseased. In the latter case the flesh is here and there found, within a certain sphere, pallid and dull, of a dingy yellow —soft and flaccid; at a more advanced stage these spheres become more numerous, and pass very gradually into the healthy mass; the endocardium has become thin and transparent, rendering the dull aspect of the heart's muscle here and there conspicuous; the trabeculæ, and the papillary muscles are either wholly or partially diseased. In its greatest extent this fatty condition may affect an entire ventricle, more commonly the left, although, when partaking of the hypertrophy, the right ventricle, and consequently, as above stated, the whole heart may become involved. Rokitansky is of opinion that spontaneous rupture of the hypertrophied left ventricle is, in the majority of cases, referable to this disease of the heart's muscle. In youth this form of fatty heart has been observed, even where there was no hypertrophy, the organ being dilated, probably in consequence of the degeneration. Rokitansky believes that, when in such instances the papillary muscles are diseased, the consequent imperfect tension might occasion insufficiency of the valves."

Passing over the remaining section on cyanosis, we come to the second part of Hasse's work.

The first Chapter of this division of the work treats of Pleurisy. After alluding to the frequency of this form of inflammation, and the influence exerted by the degree of the morbid action on the character of the products, and thus on the whole progress and issue of the disease, he describes the first appearance of inflammation of the pleura as consisting in a congested state of its blood-vessels, which are seen congregated here and there, in dense though delicate nets beneath the still transparent membrane. The redness, at first punctated, gradually spreads till the whole becomes uniform, and the membrane loses its polish. At the same time, the sub-serous cellular tissue manifests increased vascularity and numerous ecchymosis, the meshes or intervals between the cells being here and there filled with a yellowish, half-fluid, half-gelatinous effusion. This implication of the external cellular tissue is, however, by no means

constant, and becomes less and less manifest in proportion as the disease advances. Now, though Hasse makes no allusion to the subject, we are disposed to think that much of the difference of character presented by the effused fluids in the earlier periods of pleurisy depends on the implication or non-implication of the external sub-serous tissues. The statement of Gross regarding the changes presented by the serous membrane in the early stages is meagre in the extreme, and in many respects incorrect. Hasse denies the existence of a dry stage of pleurisy, characterized, as it is believed, by a total suppression of secretion, and says, "I have never encountered this dry stage as described; having always, even at the very outset, found the serous fluid somewhat, however slightly, augmented in quantity, and marked by its deep yellow tinge and its increased consistency. There were likewise almost invariably present those grayish or yellowish points the initial and quickly expanding rudiments of membranaceous effusions." P. 184. Gross, however, adopts the more usual, and, we think, the more correct view, and states that, at first, there is "if not an entire suppression, at least a considerable diminution, of the natural secretion." The truth of the position maintained by Hasse, and many of the French pathologists, he thinks, is by no means established. One of the highest authorities in our own country, Dr. Hodgkin, coincides with Hasse, and states, in his 2d Lecture on the Serous Membranes, that, when turbidity of the serous effusion and false membranes "are not observed in persons who have died of suspected pleurisy, inflammation of the muscles or cellular membrane must, in most instances, have been mistaken for it," and further he says that, from his own experience, he cannot cite any case in which the inflammation, however remote, had not left some traces of its existence. If the inflammation have been "so rapid that no false membrane has been formed, we find an effusion of serum which, in the majority of cases, is sanguinolent or puriform." Analogy, however, is certainly in favour of a suppression of the ordinary secretions in the early stages of inflammation of a serous membrane.

Having portrayed the subsequent steps in the inflammatory process, Hasse enters on the consideration of the mode in which inflammatory products become organized. He justly observes, that as no one has yet been able to watch the movements of blood currents in inflammatory products during life, cadaveric phenomena are deserving of close investigation.

"With this intent, I have omitted no opportunity of removing false membranes of every description, either alone or together with the subjacent pleura; and, having first washed them carefully in cold water, proceeded at once to spread them on glass plates to dry. As the congested blood-canals in inflamed or newly-organized textures, do not part with their contents to the larger veins, as in healthy parts, I have been able to furnish a series of preparations in which the course of at least the larger of the newly-formed branches is conspicuously shown by a surprisingly delicate distribution of blood-vessels.

"Wherever vessels had formed in the adventitious membranes, they proved to be continuations of the branches ramifying in the serous coat; they penetrated the false membrane at numerous points, and then branched out in a stellate manner, or formed into partly divergent, partly parallel, fascicular groups. Hodgkin describes them similarly. Such was the character of the vascular development, more particularly in the gelatinous form of exudation, which, in

this respect, so nearly approaches the normal formative substance, as, after a short period, to display ramifications of vessels precisely similar to the parent ones, and, in all probability created in the manner witnessed by Döllinger in the embryo of fishes. I have repeatedly and very carefully taken up, upon pieces of glass, detached and floating fragments of the gelatinous product in question, and searched for independent vascular development, but in vain; and I feel convinced, that if such flakes should ever be found to contain blood-vessels, they must originally have clung to the pleura, and been severed subsequently." P. 195.

In less organizable false membranes, these processes do not take place so readily, and in these, Hasse considers the descriptions given by Laennec and Gendrin to be applicable. Where tubercular matter is effused with the more ordinary products, and does not prove rapidly fatal, it is occasionally met with as a residue of the aggregate morbid product in small scattered masses included between the pleuritic adhesions. These isolated masses are susceptible of farther transformations, which Hasse thus describes:—

"Most frequently, a deposition of phosphate of lime takes place little by little, until the whole is converted into a hard earthy concrement, constituting an irregular, rough plate, sometimes separable into two layers, which inclose an intermediate residue of the caseous, atheroma-like mass already alluded to. The plates in question are wont to adhere so firmly both to the parietes of the thorax and to the surface of the lungs as to prevent the two folds of pleura being distinctly recognized. These relations apply to most instances of so-termed ossifications of the pleura, which would seem, even where they form in the pleura itself, or in the cellular tissue external to it, to originate in inflammatory action (Hodgkin.) The thin lamellæ occasionally found without any accompaniment of pleuritic adhesions, and the fibro-osseous tumours of the pleura, are obviously referable to a different source." P. 197.

We do not, however, see wherein lies the proof that these changes are referable to calcareous transformation of tubercular matter. Under the head of Empyema we have looked in vain for any corroboration of the important observations of McDonnell with reference to enlargement of the liver or the discharge of the purulent accumulations by the bronchial membrane, independent of any perforation.

The Chapter on Pneumonia, we are compelled to pass over, with the exception of the following passage, the practical importance of which our own experience has taught us:—

"*Drunkards* are proportionately most prone to the disease, and, what is remarkable, it scarcely manifests itself in them by the usual vital symptoms, although spreading, and advancing to the last stage with astonishing rapidity." P. 216.

The next Chapter, entitled "Diseases of the Lung not necessarily dependent upon or allied to Inflammation," comprises gangrene, œdema, hæmoptysis, apoplexy, and the atelectasis of Jörg. Of hæmoptysis considered as a primary affection, Hasse admits four varieties, simple bronchial hæmorrhage, (or genuine hæmoptysis,) effusion of blood into the pulmonary vesicles, (apoplexy in a restricted sense,) hæmorrhage from rupture of the pulmonary texture, (pneumorrhagia,) and the most fatal form arising from rupture of the large blood-vessels. There can be no doubt that much unnecessary alarm is often occasioned by confounding simple bronchial

hæmorrhage with that which is symptomatic of incipient tubercular disease, and we fully agree with the following remarks :—

"This kind of hæmoptysis is seldom fatal, unless when complicated with other morbid processes. It presents itself in its simplest and least hazardous form, in very plethoric individuals, during the years of puberty, generally ceasing spontaneously when the thoracic organs have become fully developed. There is more ground for alarm, when it occurs as a forerunner of menstruation, or as the reflex of hemorrhoidal ailment, in which cases the attacks often recur to an exhausting amount. Even here, however, it hardly by itself endangers life, however serious its immediate and remote consequences, (dropsy, &c.) Examples are indeed known of these bronchial hemorrhages acting vicariously for the menstrual or the regular hemorrhoidal flux, with greater or less violence, periodically, during a space of thirty or forty years, the patients at last succumbing under some other disease. Under these circumstances there is no real local disease, the lung being merely in a state of congestion, or, more correctly speaking of hyperæmia." P. 242.

The propriety of the term pulmonary apoplexy has been disputed, but Hasse very properly says that, in true apoplexy of the lungs, the effusion of blood into the air-cells, or into the general pulmonary texture, is attended with the loss of consciousness through sudden suspension of the main springs of vital action. In this sense, since the time of Hippocrates, the term apoplexy has been as applicable to the lungs as to the brain. Indeed, the prostration of the vital powers, from the first, forms the most striking characteristic of these cases, which are seldom accompanied by hæmoptysis.

The anatomical division of pulmonary apoplexy into that in which the effusion is thrown out into the air-cells, and that in which it is produced by rupture of the pulmonary texture, Hasse thinks is borne out by the etiological relations and by the vital symptoms. In true apoplectic effusions the blood, he believes, is thrown out from the twigs of the pulmonary artery directly into the air-cells of the smaller bronchial ramifications, and then becomes consolidated without any further change. The defined limits of the apoplectic centre are owing to the absence of rupture, the effusion being limited to a set of pulmonary cells, supplied by particular ramifications of vessels; in short, to a single lobule, whose covering separates the diseased from the healthy parts. The well-known connexion of pulmonary apoplexy with hypertrophy of the right ventricle, is certainly corroborative of this view, and against the notion which Hasse, and we believe Rokitansky also, controverted, viz., that the apoplectic clot is formed by the reflux of blood from the bronchia. Moreover, by the theory of Hasse, the dark black colour of the clot is accounted for by the complete exclusion of the air.

Under the head of Atelectasis of the Lungs will be found a very good account of that peculiar pulmonary affection of children, the inflammatory character of which Hasse very successfully controverts. We think, however, that we are as yet scarcely in a condition to affirm with confidence that it is nothing more than imperfectly expanded lung. We cannot find that Gross has more than a passing allusion to this interesting morbid condition.

Diseases of the Air-passages are next considered. Under the head of Catarrh, acute or chronic; we have not found anything specially deserving of notice. The following remarks, however, are of practical value :—

"It is remarkable that senile catarrh, by determining a continued irritation to the respiratory organs, causes morbid predisposition to unfold itself there,—or disease, elsewhere existing, to throw itself upon the lungs. Thus in the very individuals, who in their youth had been scrofulous, but then escaped,—perhaps even recovered from tubercular disease *by means of* repeated catarrh and its sequelæ, bronchial dilatation and pulmonary emphysema,—we find, in the decline of life, catarrh productive of tubercular development, and even of phthisis. We have already seen what an ascendency gout exercises over catarrh. In like manner hemorrhoidal disease,—which at the age of manhood is prone to manifest its periodical exacerbations by hemorrhage from the rectum,—sometimes gives rise, under the influence of catarrhal irritation, to repeated bronchial hemorrhage. In females, at the period of menstrual decline, catarrh probably leads to similar accidents." P. 269.

After pointing out the various causes inducing those very serious and fatal forms of angina termed œdematosa, Hasse observes that it was probably in consequence of Bayle having noticed only the latent forms depending on mechanical obstruction to the circulation, from long-standing disease in the contiguous parts, and where the œdema is the result of exhaustion, diminished vascular and nervous energy, rather than of inflammation, that this pathologist characterised œdema of the glottis as essentially uninflammatory. The disease is comparatively rare, and our readers may therefore be glad to have Hasse's account of the post-mortem appearances.

"The whole of the mucous membrane between the root of the tongue and the glottis, is uniformly tumefied, so that the outlines of the epiglottis and arytenoid cartilages, together with the numerous folds and recesses in the vicinity of those parts, have become effaced.

"This is the result of inflammatory effusion into the interspaces of the loose cellular texture, subjacent to the mucous membrane. In proportion to the intensity and duration of the inflammatory process, this exudation is sometimes of a purely serous and liquid nature, so as to flow away upon incision; sometimes blended with coagulable materials, and jelly-like; sometimes again mingled in various proportions with pus; sometimes wholly purulent. Hence the tumour, which is always soft, lax, and tremulous, like jelly, varies greatly in colour, being of a pale or of a reddish yellow,—sometimes of a dingy yellow or grayish white, and more or less opaque,—but for the most part superficially dotted with red. The swelling being dependent upon infiltration of the sub-mucous cellular texture, cannot extend to the inferior surface of the epiglottis, because, there, no layer of cellular tissue exists; hence the epiglottis has the aspect of having both its lateral edges bent over towards its nether surface, so as to form a narrow perpendicular groove, which is sometimes almost covered by the overhanging tumour. Neither does the swelling extend to the vocal ligaments (so that the term *œdema glottidis* is not strictly correct;) but the tumour, hanging down on each side and in size often exceeding a pigeon's egg, overlays the glottis in such wise as to leave but a narrow opening towards the posterior part, which allows the column of air to pass out during expiration, but is closed up by any attempt at inspiration. In no example recorded did the œdematous infiltration beneath the mucous membrane show itself in any marked degree beyond the glottis. The inner surface of the larynx was reddened, puffy, and covered with a puriform mucous layer. Galen's (Morgagni's) ventricles were always more or less implicated in the sub-mucous tumefaction. The mucous membrane of the affected parts is always variously changed. Its surface is rough, its epithelium partly thrown off in single scales and tufts, or raised by serous fluid; it exhibits here and there blood-specks, or scattered bright-red vascular streaks; it is moreover

easily separable, and, when separated, friable. The neighbouring muscles, especially the arytenoid, are sometimes unaltered, at other times saturated in like manner with the effused fluid, in which case they have become blanched, or undergone yellowish or grayish softening. Where the disease is of catarrhal origin, the fauces and the palatine region are generally found simultaneously inflamed, and the tonsils destroyed by suppuration. The inflammation commonly ends where the pharynx passes into the œsophagus, and again at the glottis; yet there have been instances (see Bouillaud's first case) of its pervading the whole of the air-passages and attacking portions of the lungs.

"Œdema of the glottis, when at all violent, commonly proves fatal. Inflammation of the epiglottis *alone* is perhaps more susceptible of cure, in which case the organ appears to undergo hardening, and to shrivel and diminish in size." P. 273.

In the chapter on Exudative Inflammation of the Air-passages there is one passage having reference to croup which deserves notice. It is observed that, where the false membrane cannot be entirely expelled, the whole mass sometimes liquefies into a grayish or puriform mucus; or else it becomes attenuated and perforated with little holes, without separating, so as occasionally to remain long fastened like a network to the surface of the mucous membrane. In such cases, suffocation may ensue (as it is known to do) some days after apparent amelioration, owing to remnants of the false membrane becoming detached, and clogging up the aperture of the glottis.

The catarrhal pneumonia of Hasse is the peripneumonia notha of the ancients. The condition of the lung in this form of pneumonia is thus described:—

"In many instances this variety differs from ordinary pneumonia in nothing else than in the peculiar mode in which it is developed and diffused. In other cases, on the contrary, there is a wide difference in the condition of the pulmonary texture, in the quality of the inflammatory product, and in the phases which the latter undergoes. When near the surface of the lung, the diseased portions rather sink in than bulge out, and are, if recent, somewhat incompressible, but of progressively increasing flabbiness where the disease has been protracted. They are, at the outset, of a dingy, blue reddish hue, which, however, becomes paler by degrees, changing first to a brown-red, and eventually to a yellow-brown. The texture thus altered is altogether impermeable, and soft, though less friable than in hepatization. At the beginning, it is saturated with a turbid reddish fluid, which contains numerous blood-globules; by-and-bye it becomes more and more opaque, gradually passing into grayish,—as though it were mixed with purulent mucus, and ultimately assuming altogether the aspect of a pale, yellowish-brown, puriform fluid. This, when examined by the microscope, is seldom found to be amorpho-granular; it usually contains granule-cells in predominant numbers, but often intermingled with exudation-cells, and sometimes with a proportion of pus-globules. When a large portion of a pulmonary lobe becomes thus disorganized, which is, however, rare, it is found shrivelled, lax, moist, of a yellow-brown hue, and wholly devoid of air,—resembling a wet rag. This kind of catarrhal pneumonia appears peculiarly calculated to produce obliteration of the pulmonary texture, with permanent exclusion of air therefrom, and consequent general dilatation of the several branchlets of the bronchia engaged." P. 285.

Catarrhal pneumonia invariably originates in a catarrhal affection of the air-passages, is a frequent result of pertussis, and almost invariably present, to a greater or less extent, in fatal cases of bronchitis, more particu-

larly in children. The diagnosis is usually difficult in consequence of its affecting certain patches of the lung, often of small extent, and frequently occupying the centre of a lobe. In certain epidemics, catarrh is peculiarly apt to take on this form, and the typhoid symptoms occurring sometimes rapidly in the course of bronchitis, are frequently attributable to this complication, and the consequent increased impediment to the aeration of the blood. Hasse states that a further distinction between catarrhal and ordinary pneumonia consists in the presence of plastic exudation in the bronchia leading to the hepatized lobules. That there always is a bronchitis of the tubes in immediate connexion with the affected lobules is certainly true, but we much doubt whether the exudation is always or even generally of a plastic character. The very peculiarities of the pneumonia would be, in our opinion, an argument against this statement of Hasse. But the special characters of this affection, both in a pathological and therapeutical point of view, are deserving of further investigation.

Dilatation of the bronchi Hasse describes as of three forms, the first consisting of a single spherical or pouch-like protrusion of the walls of the tube, the second constituted by a series of these cystic dilatations, and the third of distinct character, formed by the uniform dilatation of a number of tubes throughout their whole length, making the portion of lung to which they go appear to consist exclusively of tubes widened to many times their natural size. The former two, or spherical, forms are produced chiefly by bronchitis and tubercular disease of the lungs. The influence of bronchitis is probably exerted in the following way :—

"First, the air-passages are stripped of their epithelium-lining in the ordinary manner, their canals becoming loaded in part with a mucous secretion, in part plugged with fibrinous exudation. This latter occurrence takes place chiefly within certain of the lesser twigs, occasioning a collapse of the adjunct air-cells. The space thus set free is sought to be filled up by expansion of the neighbouring parts, giving rise in the majority of cases to emphysema; where, however, the collapse does not occur close beneath the surface of the lung, but at a greater depth and near a larger bronchial tube, and where it comprehends a larger tract of pulmonary substance, the result is bronchiectasis. These circumstances do not, however, suffice for the formation of a bronchial cavity; the parietes of the involved bronchial tube must needs have previously suffered the changes pointed out by Stokes, namely, loss, through inflammation, of elasticity in the longitudinal, and of contractile power in the annular fibres, with consequent incapacity on the part of either to resist the mechanical influence of forcible inspiration, or of violent cough. It is difficult to say whether Stokes is right in believing that a saccular protrusion of the mucous membrane is caused by yielding of the fibres;—such, however, may probably be the case, where the dilatation is one-sided, and its principal portion external to the axis of the bronchial tube. The analogy, likewise adverted to by Stokes, with the forms of aneurism, must, on the other hand, fall to the ground, as untenable." P. 297.

We see no reason, however, for dissenting from the opinion of Stokes in reference to the analogy between these bronchial and aneurismal dilatations; on the contrary, it affords, we think, the most plausible explanation. With regard to the third variety, or the cylindrical dilatations, Hasse adopts a similar theory to that of Corrigan, who has termed the disease scirrhosis of the lung.

The cylindrical form of bronchiectasis arises where the pulmonary cells

have become extensively obliterated by previous pneumonia, and the bronchial tubes been constrained by the pressure of the air to fill up the space vacated, before the parietes of the thorax have had sufficient time to collapse. In like manner, pleurisy may give rise to dilatation, where the effusion is of a character to keep the pulmonary cells long compressed, without subsequently affording them an opportunity for due expansion. "The bronchial tubes not being similarly encumbered, are the more liable to yield to the pressure of the air inspired." If, in addition, the parietes of the air-passages have lost somewhat of their elasticity from the previous inflammation, this passive (as Hasse terms it) dilatation will be the more likely to occur. We doubt, however, whether the dilatation occurring in these circumstances is so entirely a passive phenomenon, as Hasse supposes. Why should not the air-cells give way rather than the bronchial tubes, unless indeed it be assumed, that this is prevented by their being filled with inflammatory products, in which case there is no space vacated, and requiring to be supplied by the distended bronchia? We are rather inclined, with Dr. Corrigan, to believe that contraction of the surrounding tissue is an important, if not the main, cause of the distension of the bronchia. We have looked in vain in Gross's work for any explanation of this or any other form of dilated bronchi.

Emphysema of the Lung.—Hasse is disposed to coincide with the majority of writers who have followed Laennec in assigning chronic catarrh as the main cause of the most important and frequent variety, viz., that which consists in dilated air-cells.

"My own observation," he says, "of the entire course, the duration, and the general character of the disease, induces me, to a certain extent, to agree with the majority. Catarrh is, without doubt, the principal occasional cause of emphysema; and hooping-cough, in particular, is evidently capable, within a very short space of time, of affecting the highest degree of dilatation in the pulmonary cells. The character and real import of emphysema must, however, be admitted to depend on other causes, as yet not thoroughly made out." P. 304.

"It is, on the whole, surprising how rapidly dilatation of the pulmonary cells may take place, although it requires time to attain such a height as to manifest itself by physical signs. The means whereby catarrh effects a dilatation of the cells, are purely mechanical. In catarrh, the lesser bronchial twigs are obstructed, partly by tenacious mucus, and partly by the swollen mucous membrane. During inspiration, the action of the muscles is powerful enough to impel the air through these obstacles: expiration, which is carried on rather through the force of elasticity than through the medium of the comparatively feeble expiratory muscles, is less complete. Thus more air is inhaled than exhaled, and the surplus, expanding through the natural warmth, ultimately overcomes the resistance due to the elasticity of the pulmonary texture. On the other hand, the more forcible acts of expiration—coughing for example—will rather compel the surrounding vesicles to yield to the pressure of the dilated ones, than allow the air, contained within the latter, to surmount the obstacle to its natural exit. In like manner, swellings of the bronchial glands, &c., or tubercular deposits within the lungs, may give rise to emphysema, by the compression of bronchial tubes, —as shown by Reynaud, Andral, Carswell, and others." P. 305.

These mechanical causes are doubtless efficient to a certain extent, and in a certain number of cases. But the undoubted hereditary origin of the disease in a large proportion of cases, as first clearly shown by Jackson,

and since amply confirmed, together with the history of the disease, in many instances where no such hereditary tendency can be discovered, and an accurate estimate of the structural changes attending the disease, have long since satisfied us that, in the large majority of cases, some original weakness or defect in the walls of the air-cells, and the structure of the lungs generally, is the true and main source of the disease. In other cases, probably, there is an induced weakness of the walls of the pulmonary cells, the result of catarrhal or other previous disease. The account which Gross gives of the disease is meagre and defective in the extreme. After strangely asserting that the specific gravity of an emphysematous lung is "increased!" he goes on to observe, "*vesicular emphysema is rarely observed before the age of fifty!*" The whole of his observations, on its pathological relations and modes of origin, are contained in the following brief passage:—"It is a very common attendant on tubercles, aneurismal tumours of the heart and aorta, enlargements of the bronchial glands, and asthmatic disorders. Amongst the occasional causes may be enumerated whatever has a tendency to over-distend the air-cells of the lungs, as playing upon wind instruments, singing, and *loud screaming!*" (P. 440.) Considering that loud screaming is to be reckoned among the occasional causes, we should scarcely have expected the statement that it is "rarely observed before the age of fifty." No allusion whatever is made to the labours of his distinguished and lamented countryman Jackson, although he alludes to Louis, whose deductions were mainly drawn from the data collected by his pupil Jackson.

In remarking on the peculiar shape of the thorax in aggravated cases, Hasse observes, "Louis mentions another kind of projection, or rather filling up, of the supra-clavicular region, which gives a plumpness, he says, to the otherwise emaciated neck. I believe this appearance to have been present in the cases noted by that accurate observer, but have never myself witnessed it. Indeed, I have mostly found, above the vaulting of the chest, and above the clavicles, a decided pit or depression, wherein are displayed, almost like thick, tense cords, the cervical muscles of inspiration, which in general play a subordinate part, but here, where the operation of the principal inspiratory muscles is defective, become prominently active." P. 310.

We can ourselves bear testimony to the accuracy of Hasse's statement, having directed particular attention to this physical sign, and never, in a single instance, met with the filling up of the supra-clavicular region of which Louis speaks, but, on the contrary, with Dr. Stokes, have found the triangular spaces answering to the insertion of the sterno-mastoid and scaleni muscles singularly deep.

The chapter on Tuberculosis of the Lung is carefully compiled and full of instruction. We have, however, so recently brought this subject before the attention of our readers, that it is the less necessary to enter into any detailed examination of Hasse's views. Moreover, as the researches of Mr. Addison and Mr. Rainey have appeared since the publication of Hasse's work, he has been precluded from availing himself of the observations of these gentlemen, which are of great interest and importance in connection with the minute anatomy of tubercle. We cannot, however, here avoid alluding briefly to the very careful observations of Mr. Rainey.

That gentleman has, we think, incontrovertibly shown the truth of the opinion of Carswell and others, (and adopted by Hasse,) that the common opaque yellow tubercle is thrown out into the cavities of the air-cells. In proportion as these become distended by the accumulating tubercular matter, the inter-cellular vascular plexuses become compressed, obliterated, and eventually destroyed, together with the walls of the air-cells. In this way several cells are thrown into one, several tubercles become amalgamated into an irregular mass, penetrated here and there by the crescentic edges of the partially destroyed cell-walls, and on careful examination, presenting occasionally detached fragments of the remains of the inter-cellular plexuses and their corresponding cell-walls. We have thus two important causes of derangement of the respiratory function. Not only is the supply of blood to one part of the lung cut off by the obliteration of its vessels, but also an almost necessary congestion of adjoining portions induced, and thus a tendency to hæmoptysis established.

There cannot, we think, be any doubt that Mr. Rainey's views afford the true explanation of those appearances observed in tubercles which have induced many to believe them to be vascular. It is strong confirmation of these views, that the vessels which are not unfrequently seen, appear, for the most part, as arcs of circles, of the same radius as the plexuses between the air-cells, and of which they are the unabsorbed fragments. The following passage, from the Postscript to Mr. Rainey's paper in the last volume of the Medico-Chirurgical Transactions, is so important in connection with the general pathology of tubercle, that we shall be excused for extracting it. A rabbit, whose lungs, liver, kidney, mesentery, and other parts, presented numerous tubercles, all of a scrofulous character, was injected with fine injection.

"Some parts of the lungs were studded with white masses of different sizes; others, even as much as a third of a lobe, appeared very much like a lung which had never respired. On examining the latter, I perceived, in the arterial trunks leading to those parts, distinct masses of white granular matter mixed with the injection; and, continuing the examination, I found that this appearance was due to all the capillaries being literally choked up with this same matter. The air-cells were free from it and contained air. The white masses in the other parts appeared to be produced by the vessels being filled with this matter, as in the preceding, and also by its escape into the air-cells and surrounding structures. On examining the kidney, I found that the vessels were filled in the same manner as in the lungs. I mentioned this to Mr. Quekett, who told me that he had, in scrofulous cases, seen strumous matter mixed with blood which had been pressed out from an artery going to a diseased part."

Hasse justifies himself for classing glanders with affections of the respiratory organs, in consequence of the disease originally and chiefly implicating the nostrils and air-passages. In our present state of comparative ignorance on this subject, the classification may be admitted. Hasse, however, has nothing new to offer on the subject, and it is not even mentioned by Gross.

From his own experience, as well as that of others, Hasse has attempted to draw up such an historical account of cancerous tumours in the respiratory organs as shall assist in the detection of the disease during life. The account, though very succinct, is valuable and instructive.

"Cancer of the lung is incomparably more frequent in males than in females. Out of 22 cases, collected by myself, 5 concern women, and 17 men. In childhood the disease is unknown. Of the above 22 cases, 9 occurred between the 20th and 29th years; 8 between the 30th and 39th; 2 between the 40th and 49th; 2 between the 50th and 59th; and 1 between the 70th and 79th years. The morbid disposition is, accordingly, greatest in the prime of life." P. 370.

In almost every instance the tumours in the lungs were medullary, he having only once met with the colloid variety. The medullary cancer was chiefly found in isolated masses; and in the form of infiltration, only where the lungs were the part originally affected, and primary infiltration appears to be characterized by one lung being exclusively involved, the other remaining exempt. From the statistics collected by Dr. Walshe, it would appear that the right lung is the one affected in two-thirds of the cases. These points are of practical importance in reference to the diagnosis. It is an important fact, that cancer in organs whose veins are tributary to the portal system, does not appear to spread to the lungs, although it is known very often to lead to corresponding disease in the liver. This limitation of the disease to the capillary range of the portal vein, Hasse observes, is the more remarkable when we consider the promptness with which medullary cancer, in particular, proceeds from one cluster of lymphatic glands to others situated in the remotest parts of the body. It is also remarkable that the lung shews no disposition to disseminate the morbific elements of which it is the seat; and in the case of primary cancer of the lungs there does not appear to be a single well-marked instance of other internal organs becoming secondarily affected. Cancerous disease of the lungs never co-exists with scrofulous tubercles.

The remaining Sections of Hasse's work, on cysts in the respiratory organs, pseudo-melanosis, and diseases of the thymus and thyroid glands, our limits compel us to pass over. We have already given a sufficiently full abstract of the work to enable our readers to judge of its character, and we trust to justify the opinion we have expressed of its merits. For the purposes of reference the book would have been much improved by a more methodical division of the Chapters and Sections.

With Professor Gross's work we need not longer occupy the time of our readers, as it is not likely ever to obtain a place in the literature of this country, where we have so many much better works. The coloured drawings and engravings with which it is illustrated are so coarse as to be for the most part quite useless and altogether beneath criticism.

THE STRUCTURE AND FUNCTIONS OF THE FEMALE BREAST AS THEY RELATE TO ITS HEALTH, DERANGEMENT AND DISEASE. By *E. W. Tuson*, F.R.S., F.L.S., Surgeon to the Middlesex Hospital. London: John Churchill, 1846.

WE have always thought it very desirable that surgeons or medical officers to the larger hospitals in this country should publish the result of their experience, when they have filled their stations for fifteen or twenty years.

We conceive that few things would be of greater practical benefit to the profession than a more general acquiescence in this plan. Mr. Tuson, since the year 1823, has been attached to the Middlesex Hospital, and it cannot be denied that he must have had great opportunities for the pursuit of practical surgery. His treatise is on a very interesting and extensive subject, which has already been well written on by the best surgeons of the present and a former day. But while we are willing to accord to Mr. Tuson praiseworthy motives in undertaking the task in question, we regret to add our conviction that his monograph is a very ordinary production. He has managed to work it out into a good sized volume numbering 485 pages, and we are quite convinced that it might well have been condensed into a fourth part of the size. It has rarely fallen to our lot to read a more confused and clumsily written Preface than that which is prefixed to this volume. If the author really had a well-defined meaning in one half of it, the language he has used has been a most effectual cloak to conceal it. We take at random the following quotation :—

"The opportunities since the period I entered the Hospital, in the year 1823, have been very numerous, both of watching this disease (cancer) and observing the effect of many remedies that have been recommended; still I must confess that I feel little satisfaction with the progress made in such a number of years; but, urged on of late by the hope of alleviating nature's sufferings, I have been more than particularly active in my researches, and feel a greater gratification in having accomplished much more in a shorter space of time. Promises are easily made, when unaccompanied by diligence and perseverance; but, with a full determination to enter into the subject in question, let us hope that we may realize more advantages in less time by following up the course already in progress, and by making use of what has already been acquired." P. ix.

The first Chapter of the Book is occupied with an Anatomical Examination into the Structure and Functions of the Mammary Glands, and the Diseases of the Breast are arranged under three classes. Class I. includes the functional and painful affections of the Breast. Class II. comprises those organic lesions of the Breast generally occurring independently of Inflammation. Class III. includes another variety of organic lesion—viz., Tumours or Formations of a Malignant and Contaminating Nature. A number of Cases illustrating the use of certain remedies in the Cure of Cancer in different parts of the body concludes the book.

After a brief description of the anatomy of the mammary glands, our author "takes a short survey of the changes these glands undergo during certain periods of life, such as puberty, menstruation, parturition, (gestation ?) lactation, and the decline of life." When speaking of the properties of human milk, Mr. Tuson digresses into a consideration of the value of Proteine as a medicine.

"Five pounds of the fibre of beef will produce about three ounces of proteine. Its quality is highly nourishing, and it may be given in sugar; to weakly infants it bids fair to be of the greatest service, and in delicate constitutions, when the blood is insufficient for the secretion of a healthy milk, there is little doubt that it will be highly beneficial, as it is identical in composition with the chief constituents of blood—animal fibrine and albumen—and therefore it at once gives the essential properties of nourishing the frame and increasing its growth and strength. Where nourishment is necessary for the restoration of certain parts of the body, it is most highly useful. Ten grains given to an adult, combined with sugar in the form of a powder, or with strong barley-water, will be more

beneficial than four grains of the disulphate of quinine, and will even do more than that most useful preparation, under certain circumstances, in the restoration of various parts of the animal economy. After reading the observations of Liebig respecting proteine, I was induced to administer it to several patients, and have carefully watched its effects; and from my personal observation can most strongly recommend it as a medicine likely to prove highly beneficial. In scrofula its exhibition has been extremely serviceable, and no doubt it will prove very useful in other diseases. In a constitution where the milk is not of a sufficient quality to satisfy the child or to give it nourishment, or where the mother feels weak and debilitated from the act of nursing, proteine will be of service, not in the shape of diet, but in a medicinal point of view, to strengthen the secretion and the system generally, not only of the mother but also of the child." P. 28.

Amongst the disorders occurring during lactation, Mr. Tuson speaks of Suppression of the Milk, and he mentions a case where both the mammary glands were so injured by an accident as to be incapable after subsequent pregnancies of secreting milk.

"A woman was admitted into the Middlesex Hospital in the year 1826, who had been knocked down by a horse that had run away with a cart, the wheel passed over her chest, which contused and lacerated both the mammary glands; sloughing took place, the wound healed, and the patient was discharged cured. I have occasionally seen her since the accident. She has borne several children, but was obliged to bring them up by hand, as no secretion took place in either of the breasts, although every means were employed to induce the glands to perform their functions." P. 68.

Mr. Tuson disapproves of the use of warm fomentations or poultices in the treatment of milk abscesses, as their tendency is to derive blood to the affected part, and so promote, instead of repress suppuration. He speaks very favourably of a lotion composed of two drachms and a half of hydrochlorate of ammonia in six ounces of rosemary or common spirit, which is to be frequently applied over the affected gland by means of lint saturated with it and covered over with oil silk. The suggestion was derived from an old surgical work which our author chanced to be reading some years ago. But we suppose that there are few practical men who have not been in the habit of using lotions of the hydrochlorate of ammonia without knowing anything of Justerman's observations.

Mr. Tuson quotes at full length Cruveilhier's description of fibrous tumours of the mammæ, without adding to it, and he transcribes Müller's, Dr. William Budd's, and Dr. Copland's descriptions of the varieties of cancer, without attempting to condense or analyse them—and thus we find a large portion of his book made up of lengthy extracts from various authorities. We do not see Dr. Walshe's name amongst the authors on cancer.

Mr. Tuson has described with some care the different remedies which have been employed for the relief or cure of cancer, and we think this part by far the most useful of the book. Mr. Tuson discredits the notion of cancer being curable by any local means whatever, but he speaks highly of some ingredients which, being applied over an open cancer, establish a clean granulating surface, in place of a foul and unhealthy one. He thinks compression of a cancerous breast, either by bandages, with pads, or by the air-cushion recommended by Dr. Arnott, as ingenious methods to promote absorption, but that the effect of this absorption is to transfer the

malignant disease to other structures. Chloride of Zinc is regarded by our author as one of the most useful remedies in the treatment of cancer. It may be given internally, and used as a local application. Mr. Tuson has "frequently healed a large ulcerated surface by its use, not only on one occasion, but several times at different periods upon the same patient." A solution of the chloride of zinc may be applied to large irregular cancerous ulcers by means of a syringe, and with great but temporary benefit.

A plaster made with the following ingredients has been found by Mr. Tuson serviceable in many tumours of the breast :—

"Mercurial ointment, one ounce; gum ammoniacum, six drachms; extract of deadly nightshade, four drachms; prussic acid, one drachm: reduce the gum to a fine powder, and with the extract and a little water form a thick mass, and then add the ointment previously mixed with the acid, and unite them by means of a pestle and mortar. This composition is to be thickly spread upon leather, and a piece cut to the size of the tumour and applied carefully over it." P. 410.

We transcribe Mr. Tuson's account of the chloride of carbon, and its use in cancer and other affections. We believe that Mr. Tuson has used this agent extensively, and his experience, with reference to it, is valuable.

"Chloride of Carbon is a thick ætherial oily fluid, dissolving slowly in water. When dropped into a glass of that liquid, it may be observed to descend gradually floating about like some spirits. It has a strong odour of chlorine, not an unpleasant taste diluted with water, and removes the fetor when applied to ulcerated surfaces.

"The first trial of this preparation, which I introduced as a medicine was as a local application to a cancer of the breast, and its effect was of a sedative character. The patient was relieved from a great deal of the pain which she had previously suffered from. It was then given internally, one drop in water at night. It produced sleep, and gave perfect ease. The dose was increased to two and then to three drops; and the patient, after taking a dose of three drops, slept for twenty-eight hours: when she awoke, she appeared and said she felt as if she had been intoxicated. In this case the remedy produced sloughing of the diseased structure. In very many cases that have come under my treatment, this remedy has produced perfect freedom from pain, quieted the mind and nervous system generally, prevented the rapid growth and progress of the disease; and has rendered the patient's life comparatively happy to their previous feeling and condition. In some cases it has little or no effect; in a few instances affecting the head, and making the symptoms more aggravated. When sleep is not produced by one or two drops of this preparation, a very small dose of the solution of acetate of morphine will produce immediate effect in such cases where it is likely to be serviceable. Acids will also produce the same result. In cases of uterine irritation or neuralgic affections of that organ, it has proved most highly beneficial, not only in allaying the pain, but by producing a perfect cure. In such cases its exhibition has been internally; but in others attended with obstinate discharge, it should be used as an injection as well as being prescribed internally. In violent sickness, when all the usual and most approved remedies have failed to allay the vomiting, three drops of this preparation has at once taken effect; I have in such cases found benefit from applying it locally to the pit of the stomach. In cancer of the pylorus it has proved most efficacious in preventing the return of the food, and in relieving the pain and sufferings of the patient. In sloughing ulcers I have used it extensively, and I am not acquainted with a remedy more beneficial. In phagedæna there cannot be a more useful local application; but care should be taken to apply it only to the sloughing

parts. The usual strength as a local application is a drachm of the chloride of carbon to a pint of water; either as a lotion or an injection, it has been frequently prescribed with poppy decoction, and extract of conium, and as an injection in this form its effect has been very successful. The dose internally is from one to four or five drops; but in cases of malignant diseases, one to two or three drops will be quite sufficient to produce sleep, if it is likely to produce this effect. Like all remedies, it does not act upon every constitution alike; but if any person doubts its effects, the better way would be to take a dose of three drops and watch the result: it is a safe remedy. In cases where it has been continued for a length of time, (several months,) it has been observed to produce a state of debility in the system which can be remediated by the exhibition of the ammoniated tincture of bark, or sesquicarbonate of ammonia. The preparations of steel will also be useful under such circumstances. In fungoid disease, the application of the chloride of carbon has been very remarkable: it was applied to a diseased structure extending from the mastoid process to near the centre of the clavicle: the whole of the tumour sloughed and the part healed. It was afterwards applied to a diseased structure below the outer part of the knee-joint: this tumour sloughed, the swelling was about the size of an orange, and the slough came away from an orifice in the skin the size of a shilling, and this also healed. It was then applied to a tumour in the groin, which also sloughed and healed. Its application was then employed to a swelling over the shoulder; but at this period, the disease made rapid progress over the abdomen, and the patient died. Had she lived, no doubt it might have had the same effect upon the last swelling; when she was admitted into the hospital these tumours were all formed. Since this period I have used this application to a fungoid tumour of the breast. It produced a slough, and the wound afterwards healed; at present the patient has had no return of the disease. I am induced to believe it may prove beneficial in this class of disease.

"Chloride of Carbon mixed with water forms a very useful gargle in foul ulcerated sore throats, removing the fetor and giving the ulcers a healthy appearance. It is also of the greatest use in affections of the gums and teeth, removing the unpleasant stinging pains produced by the exposure of some nervous filament; and its use not only gives ease, but removes any unpleasant fetor from the breath. It may be used with a common tooth-brush, instead of any other application to the teeth. In neuralgic affections it has given the greatest ease, by being employed in the form of a liniment, composed of soap liniment, two ounces, to one drachm of chloride of carbon, or with camphorated oil. This liniment should be carefully rubbed over the part affected." P. 416.

Mr. Tuson speaks favourably of the chloride of lead in producing a healthy surface on a cancerous sore, and in relieving the pain in "painful neuralgic tumours."

Traité de Nosographie Medicale. Par *J. Bouillaud.* Tomes I. II. III. IV. V. Paris et Londres, 1846. Baillière.

This work possesses no ordinary pretensions to the notice of the professional critic. The mere circumstance of its consisting of five bulky volumes, which profess to treat of the entire subject of medical Nosography, is in itself sufficient to attract his attention. It is moreover the production of a physician, who occupies a prominent position in the

French metropolis; for M. Bouillaud is not only a Professor to the Faculty, and attached to one of the largest hospitals in that city, not to mention his being a member of the Chamber of Deputies, but he is also well known as a most active and busy author. His previous works have gained for him the reputation of being a quick observer, a bold unhesitating practitioner, and a rapid and not inelegant writer. Perhaps no man in the medical world of Paris has had, during the last 20 years, his name more frequently and more prominently before the public eye. Besides numerous contributions to various journals—he was the editor, if we are not mistaken, of one of them—he has published distinct treatises on Fever, on Rheumatism, and on Diseases of the Heart; an ample work on Clinical Medicine, and an elaborate disquisition, in two large volumes, on Medical Philosophy. The present work embodies the sum and substance of all his previous writings, and contains besides, as a matter of course, much new matter on other subjects which he had not previously discussed in writing. It professes, as we have said, to be a complete systematic exposition of the present condition of Medical science, and evidently aims at taking the place of the *Nosographie Philosophique* of the celebrated Pinel—a book, it is well known, of very high merit and extensive circulation; for it passed through no fewer than six editions, and, for nearly a quarter of a century, was the leading authority on the theory and practice of physic in all the medical schools of France.

One of the first impressions on the mind of the reader, from the perusal of the present work, is, that it is much too controversial to be generally useful, or to warrant the expectation of its ever becoming a standard treatise. It is written too much for the present day, and for a particular class or school; it is the exponent rather of one section, than of the entire body, of the medical profession of its country. M. Bouillaud's acquaintance with the literature of medicine is moreover far too scanty to have justified any reasonable hopes of his success in reviewing the entire subject of nosography. He seems to know little or nothing of what has been going on in other countries save his own, during the course of the present century. Whether this ignorance arises from his time having been completely absorbed in the duties of active practice, or from a patriotic disregard of all foreign literature, we cannot pretend to say. The defect is certainly an unfortunate one for the author himself, if he has ever hoped to be known beyond the limits of "la belle France;" and that his publisher, at least, fondly entertains this laudable hope is pretty obvious from the somewhat curious advertisement on the fly-leaf of the first volume—that this Treatise "se trouve" not only in all the leading cities of the continent of Europe, an enumeration of which is given along with the names of the resident booksellers, but also in Boston and New York, in Mexico and Valparaiso, in the New World. We trust that the spirited bibliopole's expectations may be realised; but we must not forget to add, at the same time, that, just in proportion to the expected wide circulation of any didactic work, so much the more necessary it is for the medical critic to scrutinise its contents, and to estimate, with candour yet impartiality, the claims which it puts forth to public approbation. We have said that the present work is of a much more controversial character than well suits a systematic treatise. Scores and scores again of pages are

taken up with a review of the writings of Bichat, Pinel, Broussais, and one or two other writers; and then there is such an unmerciful repetition, on many occasions, upon the same subject, that the reader often feels indignant at his patience being so unnecessarily taxed.

Another prominent feature of this Nosography is the ever-renewed and almost ceaseless attempt to reduce all diseases to a very few simple types; whose existence, it is alleged, may very generally be discovered by the physician versed in all the exact knowledge of modern times, and the discovery of which is supposed to lead with an almost inevitable precision to a course of successful treatment. The author's grand aim and object are, as he often tells us, to establish *unity* and a sort of mathematical precision and exactitude in medical practice. Diseases are, in his view, so many entities, that have very generally certain steady characters, and that may be made subject to certain curative and therapeutic rules, which it is the object of the philosophic physician to discover. We shall find, as we proceed, that he talks of an almost unerring certainty in the cure of some maladies, by following a particular line of treatment. He goes further than Bichat, who anticipated that the time might come when the diagnosis of diseases would be so rigorously accurate as to entitle medicine, in one respect at least, to take its place among the exact sciences. M. Bouillaud would extend this remark to the department of therapeutics. He seems, indeed, to regard the human body as an engineer regards any piece of finely-adjusted machinery, which is every now and then subject to casualties and injuries; but these may generally be rectified by the skill of the wise and cunning workman. No allowance is ever made for the influence of the mind and feelings on the corporeal tenement, or of those subtle and mysterious agencies, atmospheric and terrestrial, of which we know so little except from their morbific effects. He is essentially a Materialist in all that pertains to pathology. Hence his inordinate confidence in what he considers the proper line of treatment, and his contemptuous disparagement of all who trust much to the *vix medicatrix naturæ*. The reader might often believe, from the unwavering nature of his author's assertions, that medicine was almost a perfect science; and yet, a few pages further on, he discovers a lamentation over the utter uncertainty that still prevails on the nature of some of the most wide-spread maladies. At one time, the fancy strikes him that, if there could only be a M. Bouillaud in every region of the world, the whole troop of acute diseases might be *chassé* from the surface of the earth, or at least brought under the dominion of science, and the clear light of unerring truth would be spread over every spot in the wide domain of medical inquiry. At another, he finds himself with his author plunged in the most perplexing darkness and confusion.

But it is time to proceed to an examination of the specific contents of the work before us; and we shall begin with the beginning. The first volume opens in these words:—

"Medicine is unquestionably in the first order of those sciences which, in modern times, have undergone that fundamental transformation invoked by the illustrious Chancellor Lord Bacon. It is indeed a magnificent spectacle for the philosophic observer to contemplate how, under the supreme empire of the law of progression, the sciences in general, and medicine in particular, accomplish,

in spite of obstacles that are constantly springing up, the great work of their evolution. It would be too long to review all the successive stages through which the science of diseases has passed before arriving at the *new era*, that is to say, at the era of precision and exactitude. We must content ourselves with some rapid observations upon this very important subject.

"It is proper to render justice to the labours and discoveries of medical antiquity, and we should, without doubt, ever respect the almost divine name of Hippocrates, and bow before the lofty and vast genius of Galen. But our respect and admiration for the ancients must not, in any way, approach to idolatry, and hinder us from rendering justice to the moderns. Thus, then, without forgetting to honour past ages, let us also dare to praise the grand epoch in which we live, the true golden age of medicine and of the other natural sciences. To propose for our imitation, in the present day, the Hippocratic school of medicine, is about as reasonable as if we proposed for our model in physics and chemistry, the physics and the chemistry of Epicurus, Aristotle, or of Thales. The human mind does not thus move backwards, and our modern Hippocratists are at least twenty centuries behind their age! Who does not see, at the first glance, that Medicine—as long as she was deprived of the light supplied by *post-mortem* examinations, and unacquainted with the exact methods of investigation derived from the study of physics and chemistry—could be little else, if I may use the expression, than a scientific abortion bereft of every sense? Now this was very nearly the sad condition of ancient medicine!"

Without even so much as alluding to the writings of any medical men between the age of the Greeks and Romans and the commencement of last century, M. Bouillaud then with rapid wing comes down to the period when Morgagni "erected the first great monument to the new science of medicine," by the publication of his admirable work *De Sedibus, &c.* But it is to his own immortal country that belongs, we are told, the glory of having established the new school; and it is, our author thinks, a circumstance worthy of notice that this medical regeneration was effected "at nearly the same period at which, working for the benefit of the whole world, she (France) gave birth to a political revolution, which all future ages will remember with gratitude, and without which every sort of scientific progress must have been deprived of the liberty necessary for its fulfilment." Forthwith, we are favoured with a long analytic review of the labours of Pinel, and more particularly of those of Bichat—"he who with an eagle's flight traversed, in the course of a few years, the entire field of medical research, and, like an Alexander of a new stamp, achieved the conquest of the whole scientific world,"—and are then brought down to the time of Broussais, of whom our author has ever been a most ardent disciple. The subsequent career of medical science is thus portrayed:—

"From the time when the doctrines of Broussais were predominant, medicine has steadily continued the course of its discoveries, and has been acquiring a continually increasing character of precision and exactitude. Favoured by happy circumstances, this science, thanks to the labours of men whose names will be cited in the course of this Nosography, has, during the last twenty years, made more *real* progress than she had ever accomplished in the same space of time, and the conquests, with which she has enriched herself, have been achieved in all the divisions which compose her vast empire. Medical philosophy has been systematized (formulée.) The old methods of observation and research have been perfected, at the same time that new ones have been invented. By the united help of all these means, and above all, by the clear light of the (properly called) accurate methods, physical and chemical, Medicine in our days has suc-

ceeded in discovering a good many diseases, hitherto overlooked, and it has, at the same time, more accurately determined the nature and true character of the greater part of those which were already known."

M. Bouillaud proceeds to detail some decisive conquests which he has achieved himself, within the last fifteen years, by following in the steps of his great teacher; he alludes more particularly to the discovery of the frequent existence of Endocarditis in Rheumatism; of the blowing and other peculiar sounds in the arteries when the blood is poor and watery, as in Anæmia, Chlorosis, &c.; and of the new formula for extinguishing inflammation by bleeding *coup-sur-coup*, &c. His grand object, he goes on to inform us, is to establish *unity* in medical doctrines, as it exists in those physical sciences which have attained the highest degree of exactitude. "This great work once accomplished, there will henceforth be no more merely routine medicine, and our science shall then have its code, like that of jurisprudence." He seems full of ardent hope upon this subject, being strongly impressed with the idea that the department of the diagnosis of diseases has already been brought to a truly marvellous degree of exactness, and that, when we have once discovered their natural seat, we can scarcely have any difficulty in employing the appropriate means for their cure. Something has gone wrong with the machine. Find out the seat of the mischief at once; be prompt and vigorous in applying the proper remedy; and, in the course of a day or two, all will be right as before. He over and over again appeals to the astonishing success which has attended his practice for the last twelve or fifteen years; and, upon this ground alone, he utterly rejects the idea adopted by some recent writers (Louis among the number) that the duration and mortality of diseases are often but little affected by the line of treatment that is pursued. Need we say that he is any thing but favourable to the *expectant* mode of treatment under almost any circumstances? If an enemy be in the camp, why not expel him at once, if you have the means? all delay but serves to make his expulsion the more difficult.

In the treatment of Inflammatory Diseases, he does not hesitate to affirm that his favourite plan of bleeding *coup-sur-coup* is omnipotent (*toute-puissante*) for their cure, reducing their mortality almost to zero! He illustrates his peculiar doctrines by the following comparison, that is, it must be confessed, more lively than convincing:—

"Therapeutics is, at the bottom, an art which approaches to that of war; it is a sort of warfare against diseases. Now, just as is the case with the art of war, so that, which consists in combating diseases, ought surely to be subject to precise and accurate rules; and therefore the tactics of therapeutics, if I may venture to use the expression, must be reduced to certain exact formulas, in the same manner as military tactics are. Moreover the general principles of therapeutical tactics must be made to accommodate themselves, and bend to the different circumstances, from the operation of which various maladies are developed. In acute, violent diseases, which compromise in a greater or less degree the life of the patient, it should be the first principle to act with the greatest possible promptitude, and to oppose to the disease, that is to say, to the enemy, means which surpass it in strength and rapidity. The means generally used until of late years were much less active than the intensity and rapid progress of the disease required. These means also failed in a great number of cases. It is to avoid similar checks and defeats that, in our practice, we have acted with

more force and rapidity than our predecessors have usually done; and, thanks to heaven, success has crowned our efforts beyond all our hopes."

M. Bouillaud is naturally very much annoyed that scarcely any of his *compatriotes*, who have written on the subject of fevers and inflammation since the first announcement of the *nouvelle formule*, have even so much as taken notice of it. M. Martin Solon, in the Dictionary of Practical Medicine and Surgery, does nothing more than *dire le plus petit mot* upon the subject. M. Magendie is still more discourteous; for he does not hesitate to affirm that experience has condemned the practice of our author, and he goes so far as to insinuate that M. Bouillaud is often incorrect in his diagnosis: *il est fort douteux que ce soit toujours la maladie qu'on jugule*—the last word alludes to the strangling or extinguishing effects of bleeding *coup-sur-coup*. M. Andral has expressly said in the second edition of his Clinique Medicale, that there are very few cases indeed in which an inflammatory disease can be suddenly removed by blood-letting, however copious and frequently repeated. The same sentiment is expressed in his edition of Laennec; and in his late work on Hematology, he does not even deign to make any allusion to a practice which, we are over and over again assured, has already saved the lives of thousands. According to M. Andral, "it seems that, whenever the blood has begun to be charged with the excess of fibrine, it requires a *certain time*, whatever is done in the way of treatment, before this disposition can be removed." No one, replies our author, will deny the truth of this assertion, that "a certain time" is required to effect a cure; but this is not the point at issue. M. Andral cannot be ignorant that the new formula has not only diminished the amount of mortality from inflammatory diseases, but has also very materially abridged the duration of each case. Lastly, M. Louis has been equally unjust and illiberal; for, without giving the results of M. Bouillaud's practice, he does not hesitate to condemn the treatment employed by him: "we cannot strangle (*juguler*) inflammations, as some persons are too often pleased to say. Blood-lettings are not to be multiplied in the vain hope of obtaining this imaginary result." All this must be very mortifying to our author's vanity and pride: it is only to be regretted that he should so pertinaciously adhere to dogmas in practice, the fallacy of which it would not be difficult to prove from the pages of his present work. To us it is satisfactory to find that the opinions which we have uniformly held respecting M. Bouillaud, and which, during the last ten or twelve years, we have so often expressed without reserve in the pages of this Review, are fully confirmed in every particular by the express declarations of some of his most distinguished *confrères* in the existing school of French medicine.

There are many other subjects introduced and discussed in the Prolegomena; but our limits utterly preclude our noticing them now; and this is the less to be regretted, as they are all subsequently brought, at greater length, under the consideration of the reader in the special divisions of the work. We shall therefore now, without any further delay, proceed to a rapid analytic review of its chief contents; and, first of all, the subject of the nosological arrangement, or Classification of Diseases, adopted by our author, deserves a brief notice. He makes scarcely any reference to the labours of those who have preceded him, in this department. Pinel is the

only one whose system is mentioned; and his "nosological pentateuch," as our author calls it,—1, essential or primitive fevers; 2, phlegmasiæ; 3, hæmorrhages; 4, neuroses; and 5, organic lesions,—is thus summarily disposed of:—

"Content with having criticised the doctrines of his predecessors, and substituted his famous code for more ancient systems, he troubled himself very little with formularising (*formuler*) any new doctrines on the nature or mechanism of diseases. Proceeding after the manner of naturalists, as he often complacently remarked himself, it was enough for him to describe what he called the external characters of diseases; and, conformably to this method, he described his *essential* fevers without formally attaching them to any special organs, in other words, without *localizing* them; and thus he incurred the reproach of being an *ontologist*, and too often shewed himself unfaithful, even in principle, to the fundamental doctrines of the medical school, whose founder was Morgagni." So much for the system of the celebrated author of the *Nosographie Philosophique*. Let us now see what is proposed in its stead, in the pentateuchal or five-volumed Nosographie Medicale of M. Bouillaud.

He starts with the assumption that "a truly philosophical and rational classification of diseases must rest upon the *nature* of these diseases; every nosological edifice, founded on any other basis, is necessarily unstable and fragile." He admits that a good deal remains to be done, before it can be said that we really know or understand the real nature of many maladies; still he thinks that our knowledge of medical etiology is sufficiently accurate to warrant the classification of all diseases under the following three heads or families:—

"The *first* family is composed of organico-vital or chemico-vital diseases, which essentially affect the chemical constitution or (as it is called) the internal structure of parts. The *second* comprehends such diseases as are purely dynamic, the Neuroses or lesions of the vital or nervous powers. To the *third* belong all the changes which occur in the purely physical, anatomical and static conditions, or, in other words, in the external structure of parts." But we must enter a little more minutely into particulars; because the nosological classification of our author, professing to be built upon the physiological nature of diseases, forms a key, as it were, to the discovery of his therapeutic principles. His primary three-fold arrangement is divided into, and made to comprehend, 12 classes; of which the first five belong to the first and second great families, and the seven last belong to the third family. Here is the Synoptical Table of these twelve classes, which, be it remembered, embrace surgical as well as strictly medical diseases.

I. Fevers and Inflammations (Pyrexiæ.)

II. Affections consisting in a defect of excitation or vital action.

Appendix to the first two Classes. Excess and Defect of Hematosis.

III. Ataxiæ of the Nervous Centres.

IV. Miasmatic and Virulent Diseases.

V. Heterotrophies, Heterocrinies, and Heterogenies, of a non-inflammatory nature.

VI. Extravasations in general, and Hæmorrhages in particular.

VII. Solutions of Continuity, and Abnormal communications.

VIII. Displacements and Deviations.

IX. Abnormal Adhesions, Connexions, &c.

X. Changes of Extent, Volume, and Capacity.

XI. Foreign and retained substances.

XII. Changes in the configuration.

The first Class, including the Pyrexiæ or Fevers and Inflammations, is like Aaron's rod; for it nearly swallows up all the rest, according to M. Bouillaud's views of the matter: no less a portion of his work than three volumes and a half out of the five being occupied with a consideration of the diseases in question. Our readers may therefore expect to find an immense assemblage of maladies classified under the head of Inflammations and Fevers. The class is subdivided into three orders. These are—I. The genuine phlegmasiæ and continued fevers; including not only such diseases as are usually so denominated, but likewise the whole tribe of Cutaneous diseases, from small-pox and scarlatina to prurigo and scabies, as well as glanders, tuberculization of the lungs, and cholera-morbus! II. Irritations properly so called, and Intermittent fevers; or active Neuroses (hyperneuries.) Besides agues, this order contains various convulsive and spasmodic diseases, such as asthma, neuralgia, epilepsy, chorea, mania, &c. III. The third order (which is complementary to the two preceding) comprehends the various products of inflammatory and irritative disease; such as active serous effusions, hypertrophies of different organs, and various morbid degenerations of structure.

The 2d Class is divided into two orders; the distinctive character of the one being the "complete abolition or simple diminution of the nutritive life common to all parts;" and that of the other, the "complete abolition or simple diminution of the forces presiding over the special action of different organs." The former includes gangrene, local asphyxia, and atrophy; while in the latter we find all passive neuroses, paralysis, amenorrhœa, dysmenorrhœa, &c.

The chief members of the 3d Class, or the *Ataxic Affections* of the nervous centres, are 1, the various irregularities in the sounds and pulsations of the heart; 2, irregularities, incoherence in the co-ordinated movements of locomotion, articulation, &c.; and 3, irregularities, incoherence in the moral and intellectual functions.

The 4th Class is devoted exclusively to the reception of the three species of Typhus—European, American (the yellow-fever,) and the Oriental (the plague.)

The 5th Class is somewhat akin to the Cachexiæ of other nosologists, and comprehends scrofula, cancer, scurvy, diabetes and several other urinary affections, entozoa, &c.

The 6th Class contains the numerous varieties of hæmorrhage, from apoplexy of the brain to the formation of petechiæ, vibices, and scorbutic ecchymoses.

We need not particularize the contents of the remaining six classes of this extraordinary nosological table, as they relate chiefly to surgical maladies, congenital malformations, and so forth; nor have we any intention to examine critically its merits and demerits. We should think that there can be but one opinion on the subject, viz., that it is perhaps the

very worst classification that ever entered into the head of learned doctor to propose. The identification of continued Fevers with genuine Phlegmasiæ; the divorcement of the different forms of Typhus from those of Typhoid fever, and the establishment of the former in a class by themselves, as if they were diseases altogether distinct and dissimilar from all others; the dissociation of Intermittent and Remittent fevers from those of a continued type, and their arrangement among the Neuroses; the strange amalgamation of bodily and mental disorders in the 3rd Class;—these are a few out of the many glaring blemishes that deface the nosological scheme under review.

As the present article shall be confined exclusively to the consideration of our author's views on the very important subject of Fevers, it will be obvious that the 1st and 4th Classes of his Nosology are those which will chiefly occupy our attention. The great aim and object of all his remarks is to show that the so-called *essential* continued fevers are really and truly inflammations, either simple or complicated with a septic (typhoid) element; and this position he is now more than ever anxious to establish, seeing that "quite recently several authors of authority, (Andral, for example, *Rev.*,) retracing in some measure their steps, have endeavoured again to distinguish into two separate classes diseases which they formerly regarded as identical in nature." If a confident and unwavering hardihood of expression—founded, we verily believe, on a conscientious conviction of the truth of his observations—will ever make converts, few, who read the present work with an orthodox (query, popish?) faith, would escape becoming Bouillaudists; unless, indeed, the inordinate zeal of the writer to establish his own views suggested the idea of some weakness in the cause that he was advocating.

The subject of inflammation in general is discussed at great length. All that we can attempt to do is to select a few passages, which seem to explain the peculiar views of our author on the important question, how far inflammatory and febrile actions are to be considered as identical in their nature and phenomena.

"Considered in itself, in its essential nature, Inflammation is *one*, and constitutes a *single* species; but, considered in relation to its complications, its causes, its degrees, and extent, &c., it exhibits numerous differences, and may be divided into a great number of species. As to its complications, that which is most worthy of attention, is the *septic*, *putrid*, or *typhoid* condition. What we have said of Inflammation in general is strictly applicable to *Continued Fever* in particular; a disease, which is nothing more than the result of a primary or secondary phlegmasia of the sanguineous apparatus." The supervention of the *typhoid* condition, in the course of an inflammatory disease, is attributed to the absorption into, or to the generation in, the system of certain morbid matters, which poison and infect the whole mass of blood. When this is the case, "there exists (to use the lucid description of our author) a sort of two-fold general state, composed of inflammatory re-action on the one hand, and a septic infection of the circulating fluids on the other—elements so different from each other, that the latter is characterised by a softened condition of the coagulum of the blood and the absence, or at least extreme softness, of the buffy coat, which is invariably present in all pure and simple inflamma-

tions." He describes this typhoid complication as being to the entire system what gangrenous decomposition, in consequence of local inflammation, is to a part: "in the latter case, there is, so to speak, a *local* typhoid fever; while, in the former one, there is a *general* typhoid fever." In the same spirit, he tells us, that "the general state known by the name of *fever* is for the entire sanguineous system, what the inflammatory state is for that portion of the vascular system in a special diseased organ. In one word, acute fever is a general phlogosis of the vascular system, and inflammation of an organ is a *local fever*."

We come now to consider another question; and, as it is one that has important practical bearings, the reader, we trust, will be pleased to excuse the inconvenience of the occasional repetition of opinions, and, it may be, of nearly the same language.

"Is the division of Fevers into idiopathic or essential and symptomatic natural or legitimate? This grave question, which for the last 25 years has been discussed under every possible aspect, is not yet finally settled in the opinion of some physicians. Assuredly it would have been so, had the facts been presented to them by public lecturers and teachers with that perspicuity, force, and good faith, which constitute the very soul of science in general, and of medicine in particular. Unhappily this has not been the case. For the last 20 years that the author of this work has studied the great, the immense, question of fevers, he has neglected nothing that might contribute to the triumph of truth, and he still continues his devoted exertions; too happy if he might at length succeed in dissipating, past all chance of return, the uncertainties which still prevail on a subject of so much importance, and in introducing order and light, where as yet we meet with only darkness, confusion, and chaos."

After this flourish of words, the reader is informed that all (the diseases usually considered as) essential fevers have an element or character in common; to wit, that state of inordinate excitement of the system, which is indicated by increase of *heat*, and of the force and frequency of the cardiac and arterial pulse:—otherwise, says our critical author, the term *fever* would be utterly inappropriate! This is a mere etymological quibble, that is utterly unworthy of any practrical writer. But M. Bouillaud does not appear to regard it so. Starting from it as a point, on he goes in the most amusing style of self-complacency and self-gratulation, pitying his poor *confrères*, who are still groping their way in darkness, while all is sunshine along his path. After alluding to the discordance of opinion that still prevails on the subject of the nature and cause of fever among the physicians of that metropolis, which is the centre of civilization, the eye of the whole world, (!) he thus modestly tells them how everything may be rectified:—

"What must be done to put an end to the truly afflicting spectacle of such contradictory doctrines, this veritable anarchy that prevails in the midst of the foremost medical school of the universe?—an exact knowledge of facts, and an equally exact nomenclature, *i. e.*, a nomenclature that might be a faithful representation of the seat and sort of lesion, simple or complicated, single or multiple, which characterises each of those morbid conditions that have been studied under the name of essential continued fevers. But then we must remember that, to understand pathological facts aright, we must observe them under all their aspects, and with the aid of all those exact methods with which the majority of medical men are as yet but very imperfectly acquainted. It is by following this

method that the author of the present work has at length succeeded in solving, as far as the existing state of science will admit, the various questions appertaining to pyretological science. He has had the satisfaction of seeing his doctrines adopted by all the students that have duly attended his clinical instructions. He believes that they will be generally adopted by the whole of the rising medical generation, provided they be taught in a worthy and suitable manner in lectures and books, as well as at the bed-side of the patient. But he is quite aware of the difficulties which oppose *unity* in the mode of instruction, and, if he regrets at having still so much to contend with for the triumph of what he *knows* to be the truth, this is not from any feeling of vanity, but solely and altogether from the sound and deep-felt interest he has in the welfare of humanity. Not a year passes but thousands of persons, affected with the so-called essential fevers, owe their lives to that treatment which he has been so fortunate as to propose; for these fevers, of a non-miasmatic origin, are invariably veritable inflammations at first."

It will naturally be asked, inflammations of what part, texture, or system?—Of the sanguiferous system, the blood and the blood-vessels, replies M. Bouillaud. In accordance with this view, simple fever is described and treated of under the head of the Phlegmasiæ, and in the chapter, entitled, "Of inflammations of the sanguineous apparatus in general, and of each of its divisions in particular." One of the opening passages in this chapter runs thus:—

"This phlegmasia—inflammation of the internal membrane of the sanguineous apparatus, or angeio-carditis*—so long unknown, is nevertheless one of the most frequent of all, whether it arises primarily and idiopathically, or consecutively to local phlegmasiæ. When simple, exempt from all foreign complication, it constitutes the pure inflammatory or *angeiotenic fever* of Pinel, the *synochus* of other authors. When complicated with a septic, putrid, or typhoid element, it is the fever that has been called *putrid*, *typhoid*, or *adynamic*."

Without noticing the inflammatory lesions of the heart and blood-vessels *alleged* to be found in all fatal cases of Synochus, let us ascertain what our author says respecting the condition of the blood itself, and see how far his dicta correspond with the hæmatological observations of Andral and some other of the leading physicians of the French metropolis. M. Bouillaud affirms that the blood is uniformly strongly buffy and cupped, exhibiting all the characters that we see in cases of pneumonia and acute rheumatism. It is truly surprising, he says, that distinguished authors, Pinel for example, should assert that in inflammatory fevers the appearances of the blood drawn are very variable, and do not present any well-marked diagnostic features; and then he adds, with most amusing simplicity: "Whenever we meet with the buffy coat of the blood, such as I have described, we may confidently assert that there exists a general inflammatory diathesis, a phlogosis of the sanguineous system, a *sanguine fever*."

Before concluding his remarks on the state of the blood in Synochus, we are favoured with the following very satisfactory (!) mention of the recent researches of MM. Andral and Gavarret on this subject.

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* The appellation of *angeio-hemitis*, proposed by Professor Piorry, is, in our author's opinion, preferable; as it points to the state of the blood, as well as of the blood-vessels.

"The blood, in cases where the characters described above are present, contains a larger proportion of fibrine than exists in a state of health. The buffy coat is in a great measure composed of this fibrine. Whence this excess of fibrine? MM. Andral and Gavarret unhappily leave us in a state of complete ignorance on this point. May we not attribute it to a sort of pseudo-membranous secretion from the inflamed membrane (the inner coat) of the blood-vessels and heart?"

Whether any of our readers will be better able to diagnosticate a case of fever after reading the following observations, we cannot determine; we must leave it to themselves to say:—

"How shall we establish the differential diagnosis between a primitive, idiopathic, *essential* fever, and one that is secondary, sympathetic, and symptomatic. The thing is by no means so easy, even in the present day, as many physicians imagine; and it is not surprising that our predecessors, with their imperfect methods of observation, should have quite mistaken certain kinds of phlegmasiæ, and have designated, by the epithet of *essential*, fevers that are symptomatic of these very phlegmasiæ. I have often witnessed the very opposite mistake committed, and seen, for example, attributed to a phlegmasia of the glands of Peyer, a fever that was quite independent of it; I have seen a case of small-pox, before the eruption had made its appearance, mistaken for one of typhoid or entero-mesenteric fever, &c., &c. It is only by a profound acquaintance with the positive signs, by which the various local phlegmasiæ are indicated, that we can hope to distinguish the two species of fever (essential and symptomatic) from each other. When these signs are *really* awanting, then we must acknowledge that the fever exists by itself, *i. e.*, independently of any local inflammation, and is strictly and truly idiopathic; but it should always be borne in mind that some deep-seated local phlegmasiæ may exist with scarcely any appreciable manifestation of symptoms. It is told of a celebrated observer, that on one occasion, being exceedingly annoyed at not finding any visceral lesion in a fever patient, whom he believed to have had a *gastro-enterite*, he stuck the point of the scalpel into various parts of the corpse, when, lo! a quantity of purulent matter gushed out from a wound made in one of the thighs. This case was certainly not favourable to the doctrine of the essentiality of fevers; but it showed by the double error committed, on the one hand, that a continued fever may be attributed to a phlegmasia which does not exist, and on the other, that the phlegmasia, which really caused the fever, may be entirely overlooked. Many years ago, I witnessed a very similar case. A woman died of continued fever. No trace of any visceral lesion was discoverable upon dissection; and the physician was much puzzled to find the *point de depart* of the fever, when at length he detected a vast abscess situated immediately in front of the vertebral column."

It is scarcely necessary to point out the absurd mistake here of confounding together Synochus and Hectic fever, as if they were merely different degrees of the same morbid action. Our author, indeed, afterwards tells us that the latter fever, whether it be primitive or only symptomatic, is never the simple consequence of an angeio-carditis, but that there is always a greater or less degree of vitiation of the blood from the admixture of purulent matter. Surely such a humoral lesion is not present in cases of synochus; and, if not present, why not dissociate, it may be asked, the one from the other?

It is amusing to observe how very nearly, every now and then, M. Bouillaud is becoming an essentialist, in spite of himself. When speaking of the causes of synochus, he enumerates, first of all, over-feeding, excessive exercise, exposure to great heat or to alternations of temperature, and then he proceeds as follows:—

"The continued fever, which precedes the first fever of acute cutaneous eruptions, specific and contagious, and of *some other analogous affections*, (the Italics are ours, *Rev.*,) is probably attributable also to the introduction of miasms, virus, or subtle foreign principles, into the sanguineous system. Under this aspect, the fever in question may be associated with the different species of inflammatory or angeiotenic fever. But then we must remember that these principles, of whatever kind they may be, impress a peculiar stamp and special physiognomy upon the fever, in consequence probably of some specific action upon the blood and, it may be, the nervous system also. We must wait for further researches before we know what to make of this point of etiology."

As a matter of course, the treatment of simple inflammatory or angeiotenic fever is one of the easiest things in the world. We have only to withdraw from the mass of blood a certain amount (une proportion donnée) of its plastic elements, then prevent its rapid reformation by keeping the patient upon a spare diet, and dilute the remainder by administering cooling diluents: *voilà tout!*

So much for Synochus, or Angeio-carditis when it is "simple, franche et legitime;" let us now hear what M. Bouillaud has to say about "angiocardite typhoide, c'est-à-dire, avec une complication d'un état putride ou septique (adynamique)." This species of continued fever is said to be *often* (we had fancied that it was *always*; otherwise why call it angeiocarditis? *Rev.*) associated with a local phlegmasia; but then this phlegmasia has become the focus of a septic infection: it is from the existence of this double morbid element that the peculiarities of the disease arise. "When Synochus or inflammatory fever is idiopathic, the typhoid phenomena may be the effect of septic matters or miasms received from without or generated within the vascular system, the inflammation of which must have terminated in a genuine suppuration, and have given rise to what, of late years, has been denominated *purulent infection*."

We have no intention of following M. Bouillaud through the "tableau fidèle" which he professes to have given of the symptoms of typhoid angeio-carditis, or *Synochus putris*; they are exactly those usually specified as attending cases of *typhus gravior*. His description of the blood in this disease is worthy of notice, as evidencing a desperate clinging to a favourite doctrine in spite of the most damning proofs of its utter fallacy.

"I have previously said that the presence of the typhoid element exercises a marked influence upon the accompanying inflammatory element: this influence is especially obvious in the state of the blood. In truth, we must no longer expect to meet with the characters of inflamed blood, such as we have described above. If the typhoid condition be decided, or if it has continued for some time, the coagulum is soft, *dissolved*, of a blackish colour, and generally adhering to the walls of the vessel in which it is contained; it exhibits either no buffy coat at all, or this is remarkable for its softness and serous infiltration." These characters, we are then gravely informed, are so very different from those of inflammatory blood, that nothing is more easy to any one who has clinical experience than to pronounce at once, after a simple inspection of the blood drawn, whether the case be one "d'une fièvre franchement inflammatoire ou d'une fièvre avec complication septique ou typhoide." We should think so, indeed. Here again, the notice of MM. Andral and Gavarret's researches upon this

point is most expressively (!) short. All that we learn respecting them is that "in fevers or phlegmasiæ (is not the introduction of *phlegmasiæ* here a very wilful and most unwarrantable interpolation of M. Bouillaud himself!) with typhoid phenomena, the proportion of the fibrine in the blood is diminished,—the very reverse of what exists in active inflammations." Why our author should so rarely and imperfectly allude to the hæmatological investigations of M. Andral, and so designedly slur over their cardinal and most essential conclusions as to the marked difference of the blood in genuine Phlegmasiæ and in Fevers, is very strange. Is it retaliation for neglect of the *nouvelle formule?*

So decided a humoralist has M. Bouillaud become of late, that he even finds fault with other writers for ultra-solidist opinions.

"If," says he, "anything can astonish us now, it is the fact that Pinel should have considered as purely imaginary, and as utterly incompatible with life, a certain degree of putridity of the blood. Nothing is more positive or more real than this important alteration of the circulating fluid;—an alteration that results from a sort of poisoning of the living fluid by putrid principles, in the same manner as intoxication results from the introduction of alcoholic principles into the torrent of the blood. The typhoid intoxication, if we may so speak, being in truth the effect of a lesion of the blood, the term *adynamic*, applied to the fever in which it exists by Pinel,—because there is, as he said, *une atteinte profonde portée directement aux forces musculaires*—suggests no idea of the true nature of the disease."

Lastly, it deserves to be noticed that our author now expressly cautions his readers against mistaking certain post-mortem appearances as evidences of inflammatory action:—

"In consequence of the rapidity with which the corpse of a patient, who has died with typhoid symptoms, runs into decomposition, we must be on our guard, in necroscopic examinations, not to refer to any vital lesions certain alterations which may be the simple consequences of cadaverie decomposition. Such are, among others, certain *ramollissemens*, certain rednesses of the lining membrane of the heart and arteries, certain productions within this apparatus, as well as underneath the mucous membranes, &c."

Such is the sum and substance of the section of the present work that is devoted to the consideration of inflammatory and of typhoid Angeio-carditis; in other words, of the Synochus and Typhus of Cullen, if we can judge from the narrative of their symptoms, apart from all speculation as to their nature or seat. But it would be giving a very imperfect notion of our author's views, if here we closed our remarks upon this subject; as much yet remains in other sections of this Nosography that unquestionably appertains to it. Passing over a great variety of phlegmasiæ, including all the contagious exanthemata as well as other cutaneous eruptions, we at length (Vol. III., p. 92) come to the well-known theme of *Entero-Mesenterite*, under which appellation M. Bouillaud has hitherto been in the habit of treating of the disease, which has been at different times called putrid, malignant, typhoid, ataxic, adynamic, &c. fever. After a variety of details, which it is not necessary for us to notice, the following passage arrests our attention:—

"We now add," says M. Bouillaud, "the epithet *typhoïde* to the term *entero-mesenterite*; as, in truth, this phlegmasia—in consequence of its seat in

an organ which always contains certain fætid and fermentable matters—is more than any other apt to give birth to this class of phenomena. Nevertheless, in its early stage, this phlegmasia is exempt from all typhoid or septic elements, and indeed the supervention of the typhoid symptoms may be almost always prevented by the employment of active antiphlogistic measures at first."

Its essential and pathognomonic character, it is to be remembered, is an inflammation of the Peyerian and Brunnerian glands of the mucous coat of the intestines.

Having given a very minute account of the anatomical lesions of the intestines and mesenteric glands, our author proceeds to describe some other morbid appearances that are often met with. "We should be much deceived if we were to believe that the alterations hitherto described are the only ones discoverable in the alimentary canal in patients who have died of the *so-called essential fevers* of Pinel, which by certain writers have been all classified together under the general term of *typhoid fever* or *typhoid affection*. In an immense majority of cases, the stomach and the large intestine will be found to exhibit marks of inflammation more or less severe and extensive." But then, with a most diverting inconclusiveness, he seems to imply that it is only one sort of Typhoid Fever to which the preceding remarks apply: "I have formally concluded, from my observations, that a veritable *gastro-adynamic* or *bilioso-putrid* (here the regular terms of Pinel are employed) has, for its essential and characteristic anatomical lesion, an inflammation of the mucous glands of the small intestine, and especially of those situated in the lower end of the ileum. The secondary or consecutive lesions of this entero-mesenteric fever are said to be various; we shall only notice those that appertain to the sanguineous system. They are thus enumerated—redness of the internal membrane of the large vessels, as of the aorta, pulmonary artery, &c.; notable puffiness of the valves, and a softening of the muscular substance of the heart; a black dissolved state of the blood; softness of the fibrinous concretions sometimes met with in the cardiac cavities. In a subsequent page, we read the following description of the characters which the blood exhibits in this disease: "In the early stage, when the fever exhibits itself under an inflammatory aspect, the coagulum presents a certain degree of retraction (retraite;) its consistence is nearly normal; its surface is sometimes covered with a general or partial buffy coat; but the blood is never, unless some complication is present, *franchement inflammatoire*, as in a pure simple phlegmasia. * * * In the second and third stages of the disease, when it assumes the typhoid or putrid (adynamic of Pinel) type, —*i. e.*, when the septic are superadded to the inflammatory phenomena—the state of the blood is what has been already described in our first volume," under the head of typhoid Angeio-carditis. Then, M. Bouillaud had a hit at M. Pinel for his neglect of humoral pathology; now, M. Chomel comes in for a gentle rap: "For the last 13 years, I have treated more than 500 cases of *typhoid fever*, and the blood has uniformly exhibited the characters we have described to be present in the different stages. How then comes it to pass that a clinical professor, who has published his lectures on this disease, should assert that the blood drawn from a typhoid patient does not exhibit any appreciable alteration, except in a small number of cases?" We may fairly ask, what are the morbid changes that are specified by our author himself to be present in the first stage of

typhoid fever. From his own description, it is utterly impossible to make out; there is such a confusion and jumbling together of different things on the one hand, and such a forcible divorcement and separation of the same, or nearly the same, things on the other. M. Bouillaud here, as on the former occasion, when treating of angeio-carditis, slurs over and even mis-states some of the leading facts and conclusions connected with the recent hæmatological inquiries of M. Andral.

With respect to the general *symptomatology* of entero-mesenteric fever, it seems to be identically the same with what had been described in a previous volume as indicative of typhoid angeio-carditis; with the simple exception that, in the former, certain abnormal symptoms—diarrhœa, tenderness over the right iliac region, &c.,—*may be* (for it is acknowledged that they are not always) present. That the *Diagnosis* of the disease by the physicians of Paris is any thing but easy or to be depended upon, is abundantly obvious from the following account by one of themselves of the blunders often committed in that seat of learning and centre of medical knowledge: "I have seen," says M. Bouillaud, "I regret to say it, physicians of established reputation, but who have not been educated in the principles of exact medicine, mistake for a case of typhoid fever one of peritonitis and even of pneumonia. On how many other occasions, have I not witnessed a clear case of typhoid fever entirely overlooked; and, on the other hand, a case of simple gastric or intestinal derangement set down as a specimen of fever!" After this acknowledgment, who will gainsay the fairness of those strictures which, for the last ten years, we have felt it necessary to make upon the untrustworthiness of much of French medical literature upon practical subjects?

It is certainly passing strange that, when M. Bouillaud treats of the diagnosis of *entero-mesenteric fever*, not even a hint is made of the very disease, which of all others we should have thought would be most apt to be mistaken for it—we mean, the *angeio-carditis* of which we have already heard so much. Does he really suppose that his readers are so very forgetful, or that they peruse his work with so little attention, as not to be struck with this remarkable oversight (to call it nothing else) on his part? He evidently feels sore at the incredulity of many of his brethren as to the correctness of his own diagnosis in what may be called his *pet* diseases, in spite of his confident and ever-repeated assurances of his almost unerring certainty. Witness the following passage:—

"I know that for the purpose of explaining why, in my clinical practice, the mortality of *real typhoid fever* (is this entero-mesenteritis, or angeio-carditis, or either of them?—we cannot tell.—*Rev.*) is found reduced almost to zero when the disease is treated in time according to my method, many persons have found nothing better to say than that the cases cured were not examples of typhoid fever. Happily for me, and more especially for my patients, this assertion is a most gratuitous insinuation. I can appeal to the impartial judgment of competent men who have followed my clinical visits, and under whose eyes I have dictated the diagnosis and prognosis in each case after an examination whose precision and exactitude are well known. Is it to me, in conscience, that such a reproach, such an insult, can be addressed? What! some one will say, do you cry out so sharply against others who accuse you of errors in diagnosis, and yet make this very charge against them? It is quite true; but be it remembered that I have not done so until after I had acquired the certainty of the error, and it is not assuredly in this manner that my opponents have acted towards me."

How liberal! how modest! how convincing! We have already said that the mechanism (to use our author's language) of the typhoid phenomena in entero-mesenteritis is supposed to consist in the absorption of putrid matters in the intestines by the ulcerated abraded patches on their inner surface. The supervention of the general or systemic typhoid symptoms is always coincident with that of local septic phenomena, evidenced by tympanitic distension of the abdomen, discharge of fætid stools and gas from the bowels, &c. These latter phenomena attest, in the most striking manner, the transformation of the *pure* inflammation into one that is putrid or even gangrenous.

"This circumstance," continues our author, "superadded to the conditions of *natural* septicity which occur in the inflamed organ, serves to increase the activity of the focus of infection; and whoever seriously reflects upon this union of causes of putrid infection, which do not exist to the same degree in any other phlegmasia, must cease to wonder at the circumstance that of all inflammations it is that of the mucous glands of the last convolutions of the small intestines, which is peculiarly apt to give rise to septic or typhoid phenomena. It would indeed be surprising, if it were otherwise; and it is a thing truly most satisfactory, that the researches of anatomy and physiology have led the moderns to the discovery of a phlegmasia so situated in a very great number of patients affected with putrid or adynamic continued fever, the *point de depart* of which had been so long unknown, and to which the term *essential* had been so unfortunately applied."

As to the exciting causes of true typhoid or antero-mesenteric fever, M. Bouillaud has absolutely nothing to tell us. He has rigorously examined 600 cases within the last 13 years, in the hope of arriving at some certain conclusions on this head; but hitherto without success. Vitiated air from the crowding together of masses of people may have occasionally some effect; but we must be on our guard, we are told, not to attach undue importance to this particular; for "how many are the patients that have assured us that they were well lodged and breathed a pure air!" Truly, M. Bouillaud is a most believing man, on some occasions. With respect to *contagion* having anything to do with the diffusion of the disease, he unhesitatingly determines the point in the negative.

After what we have said of our author's pathological views, we need scarcely dwell upon the treatment which he recommends. As a matter of course, bleeding is the sheet-anchor in the first or (alleged) inflammatory stage of the fever. In the second and third stages, the chief reliance should be placed on the use of demulcent, detergent, and anti-septic drinks and enemata, for the purpose of purifying and cleansing the abraded intestinal surface from the putrid matters adhering to it. The closing paragraph in the chapter on the treatment of the disease conveys the following most instructive advice:—

"In the second and third (the putrid) stages of the disease, ought we to have recourse to the purgative method, as it has been proposed (formulée) of late years? I declare that I do not possess the necessary data for the perfect solution of this question. But I must from this moment confess that, after the results obtained from the use of this method, such as I myself know them, it seems to me preferable to keep to the use of mucilaginous diluent drinks, emollient poultices and injections, anti-septic medicines such as the chlorurets, external revulsives, and of some other means suited to the various complications of the disease."

We pity the medical man that is turned adrift to practise his profession, perhaps in military hospitals, on board ship, or in some unhealthy locality, with such a miserable cargo of therapeutic directions; but ten-fold more are their poor patients to be compassionated.

Having thus ascertained the opinions of M. Bouillaud on two forms of *typhoid fever*, we shall now briefly examine what he has to say on the subject of *typhus*; which, as has been already stated, is considered by him to be a disease so totally distinct from the former as to be arranged in a class by itself. This class is the IVth, and is devoted to the description of "Miasmatic and Virulent Diseases in general, and Septic Diseases or Typhus in particular." Were we disposed to be very particular, we might, in all simplicity, inquire whether Ague and Remittent fever are not miasmatic diseases, not to mention the purely infectious exanthemata. But, without spending time on controversy, let us proceed to find out what our author has to say about Typhus.

First of all, he finds fault with all such vague terms as *typhus*, *plague*, *yellow fever*, *jail-fever*, and such like appellations. They have no precise meaning attached to them; it is, therefore, all but impossible to discuss their history. "Hence it comes that we know little or nothing about them; and the only plan to do, if we hope to understand their nature, is to throw overboard all the works that have been hitherto published, and have recourse anew to the book of nature. Ten observations, made with *exactitude*, on each of the above maladies, would do more to enlighten us than all that has yet been written upon them."

The first position that our author lays down respecting typhus and all other pestilential fevers, whether in Europe, Asia, or America, is, that in all cases, without exception, there is a double morbid element at work—inflammation, and a septic or putrid infection. In some cases, indeed, that latter element predominates over the former from the very outset; but this is not always the case; for the typhoid condition may not be present at first, and may be only grafted upon the inflammatory, in the manner we have described in a former page, when treating of the history of entero-mesenteritis. His second position is, that there are three sorts or species of Typhus—the European, the American or yellow fever, and the Oriental or the plague. These three are not so much different diseases, as varieties of one and the same disease. None of their alleged characteristic features are uniform or constant. Buboes and carbuncles have been witnessed in some cases of typhus; and the black vomit is not invariably peculiar to the yellow fever of the new world.

With respect to the seat or nature of the three species of typhus, our author acknowledges that it must be sought for in the blood. But, in seeking to explain the changes that take place, he is continually falling into the most bewildering errors, in consequence of his mind being ever intent upon mixing up some of his own conjectures with the facts which he describes. For example, he asserts that all miasmatic poisons induce an inflammatory action in the parts with which they come in contact; and then, having settled this point in his own mind, he proceeds to talk very complacently of the invariable co-existence "des états phlegmasiques" with "des états de décomposition septique," in the different forms of typhus or putrid fever. He says that, as yet, we possess no satisfactory

information as to the change that takes place in the mass of the blood in this disease. He calls it "a sort of gangrene of this fluid; for it is essentially the same change as that which gives rise to gangrenous decomposition in a case of local septic infection." In confirmation of this statement, he quotes the observations of M. Bonnet of Lyons, to the effect that the blood of typhus patients contains *hydro-sulphuret of ammonia*—a salt that is well known to exist in putrescent blood. He refers to what he had previously said concerning the state of the blood in typhoid angeiocarditis and entero-mesenteritis, as being applicable to the hæmatology of genuine typhus; and he expresses his astonishment that Pinel, Broussais, and several other writers (whom he afterwards calls *exaggerated solidists*,) should have been so blind as, "in the midst of symptoms of the most flagrant septicity," to call in question the existence of an altered state of the blood—*ce grand élément pathologique*—in patients labouring under typhus or adynamic fever. As he proceeds, he becomes quite a zealous out-and-out humoralist:—

"It is not the blood only that exhibits alterations in typhus. As this fluid supplies every part and tissue of the body, and furnishes the materials of all the secretions, it is obvious that every part must be more or less morbidly affected. Among the most frequent alterations, I may mention the vascular injection of different organs, *ramollissemens* partly inflammatory, partly gangrenous, suppurations, sanguineous effusions, and extravasations, &c."

With respect to the Etiology of true Typhus, M. Bouillaud agrees with the general opinion; for we find him admitting, that "the essential cause of the disease is a putrid miasm diffused in the atmosphere, and acting on masses of persons crowded together in ill-ventilated situations." He recognizes the contagiousness of the three species of Typhus; but adds that, in his opinion, the influence of this agency has been much exaggerated. His views on the subject of Contagion are evidently the result of a very imperfect examination of details. Indeed, the whole of this section of his work, that professes to treat of miasmatic diseases, is rather an unconnected commentary on the statements of certain authors than any regular exposition of the matter in hand. For example, he denies that any form of true typhus ever exhibits a remittent or intermittent character, and gives the following most convincing reason for his opinion:—

"The element, that is common to intermittent and to continued fevers, is the excitation of the sanguiferous system: this element may exist either alone, or combined with a state of miasmatic infection. But in *continued* fevers, there is present, either in the sanguineous apparatus itself, or in different special organs, a veritable inflammation that keeps the febrile excitement under its dominion, and makes it assume the continued type which necessarily belongs to itself; whereas, in *intermittent* fevers, on the other hand, the febrile excitement is exempt from all such inflammatory complication: if this were not the case, the intermittent type would be absolutely impossible."

Our author seems to have forgotten all the while that he was treating of typhus, which, he had himself admitted, might exist without any inflammation at all!

The description given of the symptoms and treatment of typhus as seen in Europe, is about as absurd a thing as we have ever encountered. M.

Bouillaud evidently knows nothing of the matter from his own experience; and at length, after a good deal of most unnecessary verbiage, he is obliged to admit that there is not an atom of difference between typhus and many cases of his entero-mesenteric, or of his angeio-carditic, fever. "The symptoms of the former," says he, "are essentially those which I have described when treating of the septic forms of continued fever (is typhus not a continued fever?—*Rev.*) and of entero-mesenteritis. I will only remark, that there is in the last-named disease phenomena of local septicity and symptoms of general septicity, associated with those of inflammation. Now, *if* (!) typhus exists without being accompanied with the special inflammation of the mucous follicles of the ileum, it will not exhibit the local phenomena peculiar to this affection. In the contrary case, these phenomena will present themselves in a more or less decided manner."

The account given of the American Typhus or Yellow Fever is, as might be expected, very scanty and imperfect. Pinel, we are told, considered it not as a disease *sui generis*, but merely as one form of *gastro-adynamic* or *putrid bilious* fever; its peculiarities depending upon climate and other accidental causes. Tommassini was of the same opinion. M. Bouillaud flatly denies that the yellow fever (although he never saw a case) ever assumes a remittent, or intermittent type! The leading and most frequent anatomical characters of the disease are inflammation and ulceration of the stomach and small intestines, the presence of a dark sanguineous fluid in the stomach, engorgement of the lungs, traces of inflammation of the archnoid, &c. M. Louis, in his report of the Gibraltar Epidemic, stated that a yellow discolouration of the liver was the only uniform and characteristic lesion, not observed in other diseases! On the other hand, M. Rufz of Martinique assures us that this is only an occasional appearance. Not even a passing allusion is made by our learned (?) author to the writings of any English physicians, although, we need scarcely say, no men have seen so much or written so ably, on Yellow Fever as our countrymen. But M. Bouillaud knows nothing about them; perhaps he has not even heard their names. He regrets that he has never been able to institute any personal researches on the true nature of the disease; then doubtless all difficulties would have been cleared up. He tells us that the symptoms of Yellow Fever are essentially the same as those of Typhus, and thereupon he sends his reader back to what he had said on this head!

With respect to the question whether the disease be contagious, it appears that the majority of French writers are inclined to return a negative answer; and even those, who adopt the contrary opinion, are all in favour of greatly relaxing the existing quarantine regulations.

As to treatment, M. Bouillaud very summarily dispatches that subject. "Hitherto," says he, "nothing precise or rigorously exact has been established. We must look to the labours of writers yet to come, for accuracy; their researches must be conducted in such a spirit of exactness, that no serious objection shall be made against the results which they have obtained!"

We shall not follow our author through his equally meagre, and often incorrect, description of Oriental Typhus or the Plague; we shall wait till he has seen it.

Even on the more domestic, and therefore better known, subject of Intermittent Fevers, many of his views appear to us crude and most crotchety. We have already mentioned that this important family of febrile diseases, including remittent as well as intermittent fevers, is entirely separated from the genuine Pyrexiæ on the one hand, and from the different species of Miasmatic and Virulent diseases on the other, and is described as one of the members of the Neuroses. M. B. commences his description of them in these words:—

"Like those of a continued type, intermittent and remittent fevers may be either of a miasmatic or of a non-miasmatic origin, *simple* or *compound*, *i. e.*, either consisting of the febrile element alone, or of this element, combined with a septic one proceeding from paludal emanations. The intermittent fevers of the first category are pretty generally *benignant*, whereas those of the second may, if not properly treated from their outset, quickly cause the death of the patient; hence the appellation of *pernicious* has been applied to them."

As we proceed in the narrative, we soon discover that our author's opinions are anything but settled on the ætiological history of intermittent fevers. In some passages, he leaves the impression on the reader's mind that paludal exhalations are the only exciting cause; while, in others, he intimates that a case of genuine ague may be produced by mere alternations of atmospheric temperature. But he will not take upon himself to decide the question; although he does not see in what other manner the not unfrequent occurrence of cases of intermittent fever in Paris is to be accounted for. We should have thought that the exhalations from the filthy Seine, when its waters are low in summer, would have been deemed to have had sufficiently potent influence in this respect.

The proximate cause of intermittent fever is declared to be "an intermittent irritation, a sort of *neuralgia* of the great sympathetic or ganglionary system of nerves, either with or without paludal infection." Its essential character is a simple nervous excitation of the sanguineous system, whereas that of a continued fever is, as we have already seen, a phlegmasia of this system. M. Bouillaud very adroitly lets go the subject of Remittent Fevers; as it is pretty obvious that their phenomena and course do not tally well with the Procrustean limitations assigned to the other two families of febrile diseases. They are therefore quietly allowed to drop out of consideration, the reader hearing little or nothing about them.

The nervous nature of simple ague is very well shewn by the intimate association and dependence of many truly neuralgic affections on malarious influence, and also by the character of the medicines that are known to be most efficacious in relieving both; for example, quinine, arsenic, &c.

It is amusing to observe how anxious M. Bouillaud is, now-a-days, not to be considered a strict disciple of the *physiological school of medicine*, which has hitherto been lauded by him as the depositary of all that was safe and sound in our profession. Treating of the administration of quinine in the treatment of Agues, he points out the necessity of often exhibiting it in full doses, "sans craindre le developpement de cette gastrite, tant redoutée de certains médecins sous le règne de la doctrine dite *physiologique*; mais c'est en pure perte." And to give greater emphasis to this renunciation of out-and-out adherence to the doctrine in question, he tells us in a note that, so far back as 1826, when he published his treatise on

so-called Essential Fevers, he did not partake of the *terreur panique* which most of the *pure* disciples of the physiological school entertained on the subject of administering quinine in the early stage of intermittent fever.

He subsequently informs us that he was the first to propose, and employ, (?) with a truly surprising success, the use of Digitalis as a succedaneum for quinine in the treatment of simple mild intermittent fever. He was led to this happy therapeutic thought by the theory he had formed as to the seat and nature of the malady. "Considering that it essentially consisted in a sort of periodic irritation of that portion of the nervous system which presides over the phenomena of circulation, and proceeded from the calorification in the substance of all the internal organs, I then drew the conclusion that Digitalis, whose fundamental property is to diminish the action of that portion of the nervous system, and of the heart and arteries, would be well suited to the treatment of ague." The result of his first experiments exceeded his expectations; but latterly, it would seem, he has recourse upon all occasions to the Quinine. Such is the history of too many of the therapeutic proposals of recent times.

M. Bouillaud seems to be afraid lest some of his brethren might suppose that the cases, in which the Digitalis had effected a cure, were not cases of ague at all; and he therefore takes the trouble to assure us that, before exhibiting the medicine, either he or his head clerk had always witnessed one or more of the attacks of the fever after the patient's admission into the hospital!

Before parting with our author for the present on the subject of Fevers, it may be as well to let our readers know what opinion he has formed upon one of the most important topics connected with the history of this ubiquitous class of diseases—we allude to the influence of what has been called the Medical Constitution of different seasons in modifying their characters, nature, and results. M. Bouillaud seems to consider much that has been written upon this theme as the result rather of meditation in the closet, than of actual experience at the bed-side of the sick. He talks, with an air of almost contemptuous complacency, of anything valuable being discovered at a time when Physics and Chemistry were yet unknown, when Astronomy was not entirely freed from the chimæras of Astrology, and the science of medicine consisted of little more save the most vague data and most shapeless materials. Such is M. Bouillaud's estimate of the age when Sydenham lived and wrote!

"To form," says he, "an idea of the precarious state of the science upon the point which now occupies us, such as it has been treated of by the ancients, the reader may consult the faithful analysis which I have given of the doctrines of Sydenham, of Stoll, and of Pinel, in my *Clinique Medicale*. I shall content myself with quoting here some passages which refer more especially to our subject.

"According to Sydenham, *continued*, *epidemic* or *stationary* fevers differ so much in themselves, (toto cœlo,) according to the different constitutions of the seasons, that the very same course of treatment, which may have been most successful during the greater part of a year, shall prove useless or positively hurtful towards its decline. Nothing can be plainer or stronger than the assertion of Sydenham; but we shall search in vain for any *positive proofs* of its truth in the work of this writer.

"There is nothing more celebrated than the doctrine of Stoll upon the im-

portance of changing the therapeutic management of diseases, which he nevertheless designates by the same name—that of pleurisy, for instance—according as this or that state of medical constitution prevails.

"We cannot help groaning over the miseries of our nature, when we reflect that those who, under a new kind of superstition, thus believe, without reserve and serious examination, in this sort of *medical astrology*, which they have adorned with the name of the 'doctrine of medical constitutions,' are the very same persons who deny truths of which the most evident and convincing demonstrations are daily given them.

"As for ourselves, who for twenty-five years have passed our life by the bedside of invalids, and have been entrusted with clinical instruction for more than twelve years, we have neglected no means of ascertaining to what point we may believe all that certain practitioners affirm, respecting the influence of the medical constitution upon the effects of remedies in general, and in particular on the treatment by bleeding in the phlegmasiæ. Well, we declare, with the greatest sincerity, and with a mind free from all systematic prejudice, that these assertions rest upon no solid grounds, that they are for the most part in direct opposition to sound experience (an experience which must not be confounded with that which, till lately, we have been contented with,) and that the different phlegmasiæ, when treated according to our formula, all the other conditions being at the same time alike, have been uniformly cured with the same success, however different may have been, according to some practitioners, the medical constitutions which have prevailed during this tolerably long space of time."

Very different is the experience of all our best physicians from that of M. Bouillaud. At present we shall only point to what Dr. Wilson has said upon the subject, as quoted at page 89 of the present number of this Review.

We must reserve to a second article our examination of the other contents of this Medical Nosography. The work is a large and bulky one, demanding no little patient fatigue in its critical perusal, with the view of communicating to others its leading principles and features. It would have been much more agreeable to us if, in the exercise of this labour, we could have conscientiously bestowed praise instead of dealing so much in censure. But the duty we owe to the medical public forbade the former, and, in our judgment, imperatively called for the latter; and we have the less hesitated to express our disapprobation of M. Bouillaud's pyretological opinions, seeing that the subject involved is one not only of prominent theoretical interest, but of the most immediate and paramount practical importance. More lives are annually sacrificed to the pestilence that is generated in our dense and populous towns, the offspring of filth and want, of sorrow and fatigue, and to that which is exhaled from the marsh, the swamp or the desert, than to all other maladies put together. How necessary then that medical men should make themselves thoroughly acquainted with everything appertaining to the history of Fever; so that, under the guidance of Divine Providence, they may be blessed to be the means of at least mitigating the severity and limiting the extension of the disease, if they cannot hope either to prevent its outbreak or suddenly to arrest its attacks.

In the following article, we shall examine one of the most interesting questions connected with this truly important subject.

I. NOTES AND RECOLLECTIONS OF A PROFESSIONAL LIFE. By the late *William Fergusson*, Esq., M.D., Inspector General of Military Hospitals. Edited by his Son, *James Fergusson*. 8vo. pp. 248. London, 1846. Longman & Co.

II. INAUGURAL DISSERTATION ON YELLOW FEVER, AND ON THE TREATMENT OF THAT DISEASE BY SALINE MEDICINES. By *G. F. Bone*, M.D., Assistant Surgeon to the Forces. 8vo. pp. 85. Edinburgh, 1846. Black.

III. CORRESPONDENCE ON THE SUBJECT OF THE "ECLAIR," AND OF THE EPIDEMY WHICH BROKE OUT IN THE SAID VESSEL. Presented to the House of Commons by Command of Her Majesty, in pursuance of their Address of the 23d January, 1846. Folio, pp. 94.

DR. FERGUSSON was one of the most active and intelligent medical officers which our army has produced during the present century. He had seen a great deal of service in different parts of the world from the year 1794, when he joined the 90th regiment, as assistant-surgeon, at Ghent, in the unfortunate expedition under the Duke of York. Two years afterwards, we find him at St. Domingo, where he remained several years in active employment. On the final evacuation of that colony, he received thanks in public orders for his services. In 1801, he accompanied the expedition to the Baltic as Staff-surgeon of the troops embarked, and was on board the Admiral's ship on the occasion of the famous attack, under Nelson, upon Copenhagen. In the Peninsula, he was present at the taking of Oporto, and at the battles of Talavera and Busaco. In 1815 he accompanied the expedition against Guadaloupe, and then remained in the West Indies for two years. In 1817, he settled as physician at Edinburgh; but in 1821, finding the ground entirely pre-occupied in the northern metropolis, he moved his quarters to Windsor, where he eventually acquired an extensive and very lucrative practice, and where he continued to reside till the period of his death in January of the present year. Throughout the whole of his busy career, he had been an ardent and zealous writer upon medical matters; more especially upon those appertaining to the subject of Fevers, Contagion, Quarantine, &c. Many of his communications appeared in the pages of our respected cotemporary, the Edinburgh Medical and Surgical Journal. For some years before his death, he had formed the plan of collecting together his scattered writings, extending and enlarging them, and adding others, with the view of publishing them all in one substantive volume. This he was not spared to complete. The first half only of the present work was revised and corrected by the author; the other half consists entirely of re-published papers. The former is chiefly occupied with articles on subjects strictly military; as Military Tactics; the Exercises, Dress, and Arms of the Soldier; Barracks and Barrack-Life; Military Hospitals and Staff; and Diet and Rations; there are also short

articles on Fever as an Army Disease, on Disinfectants, and on Dysentery, Ophthalmia, and Syphilis. The latter part of the volume contains the author's papers on Plague and Quarantine, on Yellow Fever, on Typhus Fever and Contagion generally, on Marsh Miasmata, on the employment of Black Troops in the West Indies, and, lastly, his letters on Cholera. As a matter of course, we must pass over the military papers without notice; especially as we have already devoted a good many pages, in the present number, to one of the subjects discussed—that of Military Punishments. We are glad to find that Dr. Fergusson had taken the same enlightened and benevolent views upon the question of Flogging, as Dr. Marshall has done. What we propose to do at present is to select the most prominent facts and reasonings adduced by our author upon the very important subject of Fevers, more especially in reference to their direct transmissibility from one person to another, and the sanatory measures that should be adopted to check their diffusion.

The paper on "Contagion of Typhus Fever and Contagion generally," opens thus :—

"The true essential contagions, which, under a gaseous or aerial form, act of themselves independent of, and unaided by, the circumstances of climate, atmosphere, locality, quantity, and accumulation,—do not amount to more than five or six, and may all be comprehended under that class, of which it is the distinguishing characteristic, to occur only once, generally speaking, during the lifetime of an individual; with the exception always of those infections that can only be communicated by inoculation, or the actual contact of matter. I am far, however, from pretending to say that contagion is limited to so confined a range; for the whole class of *pyrexiæ*, under every shape and form in which they can be presented to us, including even those of erysipelas and ophthalmia, can be made infectious diseases, through an undue accumulation of human exhalations, and defective medical police, constituting at these times, and under these circumstances, an undoubted well-marked atmospherical contagion of locality,—but of locality alone.

"From the first of this enumeration I have no hesitation to strike out typhus fever, and class it amongst the latter." P. 162.

Typhus fever may be truly called an endemic of the British Isles. It seems to spring up spontaneously with each revolving season, under the operation of "moist cold, when applied to the destitute under circumstances of moral and physical depression." Extremes of temperature appear to resist its development; for it is but little known in tropical regions, except under circumstances of most defective medical police—even the Plague stops at the tropic of Cancer;—and it is equally rare in very high latitudes. Dr. F. is of opinion that the practice among the Russian peasants of heating their close, and anything but cleanly, habitations so much by the constant use of a stove night and day, as well as of so frequently making use of vapour baths, has unquestionably a salutary effect in counteracting the development of typhus. Besides the endemical cause alluded to, this fever is apt to arise from the mere accumulation of the effluvia arising from the human body, whenever masses of people are crowded together in a narrow space, while cleanliness and due ventilation are at the same time neglected. Hence the frequent prevalence of the disease in prisons, ill-constructed hospitals, on board certain ships, &c.: and hence, too, the not uncommon circumstance of its becoming superadded to, and, as it were,

engrafted upon other maladies—such as the various forms of remittent fever, dysentery or diarrhœa, influenza, &c.—if the patients be unfavourably situated or improperly treated. Dr. Fergusson expresses the sentiments of a very large proportion of his professional brethren, when he says: "Diseases, which in their origin are beyond all doubt non-contagious, may temporarily acquire that property from generating a contagious atmosphere amid multitudes of the sick, and high concentration of what was the original cause during the progress of an epidemic."

That the contamination of the air with human effluvia will give rise to a truly infectious miasm, is abundantly proved by the history of Hospital gangrene or Contagious ulcer.

"This ulcer is precisely a local form of typhus fever,—a visible incarnation, if I may use the term, of the typhoid poison. It never occurs but under the most distressful crowding of sick and wounded; and it is then so highly contagious, that all other ulcers, or even abrasions of the skin, however healthy before, are speedily involved in its destructive course; and so highly does it impregnate the surrounding atmosphere with its contagion, that it is not even safe to bleed a patient in the same ward where it lies. You may look in vain for its origin under any circumstances in our hospitals, but those just enumerated, as being capable of inducing typhus fever upon the sound healthy inmates; but in the wounded, where the poison finds a nidus and a vent, instead of affecting the constitution generally, it commits its hideous ravages upon the wounded limb." * P. 164.

That Typhus fever is apt to become contagious, although it may not be so primarily or intrinsically, cannot be reasonably disputed by any candid and experienced observer. Nevertheless, while we admit the truth of this, it should never be forgotten that the disease will seldom or ever spread when the patients are removed from the original *habitat* of the evil and placed in airy and clean dwellings. In this respect, therefore, typhus is essentially distinct from small-pox, scarlatina, pertussis, and other intrinsically contagious diseases; which, it is well known, often prevail over the entire extent of a district and among all classes of its inhabitants, unopposed, even in the slightest degree, by any sanatory regulations that can be adopted. Not so with typhus. The risk of infection from it is, *cæteris paribus*, nearly exactly proportionate to the impurity of the atmosphere, and the absence of personal cleanliness; although we must also admit that the disease appears to be infinitely more malignant and communicable in some seasons than in others.

The mere circumstance of a fever spreading from one person to other residents in the same infected locality is certainly no proof of its contagion; for all are equally exposed to the same morbific influence. Unless the patient can carry the disease to a distance and there communicate it to

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* "The above relates to hospital gangrene alone, and has no reference to endemic or constitutional ulcers, which, however formidable their ravages may be, are never in themselves contagious. It may, however, reconcile my readers to the above doctrine, to state the fact that ulcers, devastating ulcers, have been seen in many parts of the world to be the substitute for endemic fever. Our army in St. Domingo, during the years 1796–7–8, abounded with such proofs; and in some parts of the East Indies, where I have never been, I understand that examples of it are even more rife."

others dwelling in a purer air, we cannot fairly recognise the existence of decided contagion. "In this transportability resides the very touch-stone and answer to the question of contagion; to talk of contagion limited to one spot is merely only saying that the spot of ground, and not the person of the patient must be the source of the disease."

It seems probable that, when typhus arises from the effluvia generated in, and proceeding from, the human body in circumstances unfavourable to cleanliness and free ventilation, rather than from any merely endemic or local cause, the disease is much more liable to acquire a contagious character. The following remarks on this point will be read with profit:—

"That other form of typhus fever, which I may call factitious, (as being created by ourselves out of causes over which we ought to have exerted due control,) to distinguish it from the endemic, which we cannot prevent, must also be contagious under the same circumstances. Here, however, I believe, in like manner, that the person of the patient, independent of *fomites*, never gives out at any one time a sufficiency of the typhoid poison to affect another healthy person; and the poison can only be made effective through contamination of atmosphere, under long-continued accumulation of morbific effluvia; and in fine that the atmosphere of the patient is infectious, and not his person, which, if once cleansed and purified, and ventilation restored, may be approached, however ill he may be, with perfect impunity. In this belief I feel warranted, from the knowledge of several important facts, of a character so general as to warrant the greatest confidence in their application. 1st, The Bristol Hospital, for a great many years, has received typhus fevers into its well-disciplined wards, without having ever spread the disease even to the most contiguous beds. 2nd, Several of the great hospitals in London have followed the same example with the same results. 3d, The most pestilentially dangerous to approach when single in the confined dwellings of the poor, have almost everywhere been found devoid of every infectious principle when collected together in numbers, and confined by the hundred within the walls of a well-regulated fever hospital. Upon this point the question of contagion must turn; for, if the evidence here given be not impugned, it will be impossible in human testimony to adduce anything more decisive and conclusive. Reasoning from single individual instances will generally deceive; but well-digested impartial observation upon masses of men can never lead to an erroneous conclusion."* P. 172.

In reference to the admission of fever patients into general hospitals, we may remark that it has been found safer and better to have them scattered as single cases through different wards, instead of congregating them together into one. This plan has been adopted in more than one of the London Hospitals; and we believe that there has been, on the whole, no cause to regret having followed it.

Every one, who has written on the subject of contagion, has recognized the powerfully predisposing effects of whatever tends to depress the energies of the mind and body.

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* "The author alludes here to what he witnessed more especially at Liverpool, when on the staff of the north-west district, in the year 1804. Typhus fever had, up to that time, proved a most virulent and dangerous contagion to the medical faculty visiting the poor in the lower part of the town, who lived for the most part in cellars or other wretched dwellings: but on the building of an excellent Fever Hospital its character there as a contagious disease altogether ceased."

"There is still another principle bearing resemblance to contagion, and affecting the health of masses or bodies of men, of which, as it acts upon the fluctuating bases of moral and mental agencies, it is difficult to give any intelligible account. The health of a ship's company, for instance, or a corps of troops, when the *morale* is good, the discipline strict but kind, and the confidence of the men assured, will often be found firm in the midst of endemic and epidemic diseases; and plague and pestilence seem to pass them harmless until the introduction of unseasoned recruits, even though healthy at the time,—change of system, or some other cause, makes a break into the general health; and then will the whole rush into disease with a proclivity of current fully as remarkable as the preceding immunity. It would be ridiculous to say that this can be similar to what we often witness in *Chorea Santi Viti*, or the imitative hysteria of girls, or the familiar acts of yawning, stuttering, &c. Yet it may be akin in a certain sense; since all our sympathies are contagious, as the word itself implies; and when the protecting shield has been removed, these seize the reins, and guide the man without restraint. So fully, indeed, do they possess him, that when his mind is impressed with the dread of impending pestilence, his ordinary diseases, it is well known, will be suspended, and, when it actually arrives, will be merged into the vortex of the new epidemic." P. 178.

There is much good sense and practical value in the following observations on the proper method of arresting the diffusion of typhus, and other contingently contagious diseases:—

"Could we establish the point, that this febrile contagion, unlike the poisonous leaven of the variolous fluid, or the venomous fluid of the viper's tooth, which on the reception of a particle contaminates the whole mass, is an infection of accumulation and quantity alone, we shall have attained a degree of security of nearly equal importance to mankind as the discovery of inoculation, or the vaccine preventive itself; for then, instead of man constantly generating a poison which would eventually be fatal to his race, the typhoid virus, without any great stretch of fancy, may almost be taken for the monitor of his life, and preserver of his existence in its due integrity; because that accumulation and quantity, to the degree that can constitute a contagion, being incompatible with the wholesome decencies of life, ought to find no toleration in civilized communities, and may always be obviated by the simplest precautions of domestic police. When these are neglected, the powers of mind and body bestowed by the Creator languish, and are deteriorated, like unhealthy plants deprived of their nourishment in a deficient soil, or of the air and light necessary to their well being; for it has been wisely ordained, that man shall use his faculties to uphold and improve the station that has been assigned to him; and the behests of Providence can never be despised without incurring the appropriate penalty in the diseases that afflict him, and the various plagues that beset his life. It will be vain for him, and his rulers more especially, to plead contagion as a predicament from which they could not escape; for that very contagion is part of their fault, being nine times in ten the work of their own creation, through default of humanity, and in consequence of their crimes. Wherever good government prevails the worst contagion that ever appalled an hospital may be dissipated by the simple separation of the inmates; and the most saturated lazar that ever came out of a pest-house may be disinfected in the burning of a basket of charcoal, putting to flight at once by this simple process the ill-omened hosts of quarantine throughout every nation, with all their vexatious machinery of imprisonment, fumigation, and delay." P. 181.

We pass on to consider what Dr. Fergusson says respecting

THE YELLOW FEVER.

It rarely affects the Creolised white inhabitants of the West Indies, and the coloured classes never. How is this?—asks our author. Import

among them small-pox, or any truly and essentially contagious disease, and they will suffer even more than Europeans. Take them to England, and they are as liable as ourselves to be affected with typhus. The truth is, that yellow fever "is a seasoning fever of malignant type, the product of high temperature, and unwholesome locality alone."* As a matter of course, the residents are not so much affected by those influences as new-comers from a temperate climate. To show how little contagion has to do with the diffusion of yellow fever, Dr. F. asserts, as the result of his own experience, that the medical men are not usually more liable to its attacks than the other officers of a regiment, and that the orderlies and immediate attendants upon the sick in hospital—provided always this be situated in a healthy locality and be kept cool, clean, and well ventilated—invariably suffer less than the soldiers in barracks. The reason is pretty obvious: the former are not so much exposed to the sun during the day and the dews that fall at night, nor yet to the chances of drunkenness and other mischievous excesses. Again, a vessel, while at anchor in one of the West India ports, may be daily losing some of her crew from yellow fever. No sooner does she set sail, and get a little to the northward, than all trace of the disease usually ceases. As for the alleged transportation of the pestilence from the West Indies to Gibraltar, Cadiz, or any other European port (where it has been known to prevail,) the idea is, in our author's opinion, utterly untenable. These places may produce, or be visited by, the very same malarious influences which gave rise to the fever in the New World. As the typhus of our own country cannot be transported to the West Indies, so the endemic of the latter cannot be transported to, or at least established among, us. A tropical climate is necessary to its existence. These remarks of our author, we need scarcely say, must be received with a good deal of qualification. We shall see immediately how far the history of the "Eclair" fever tallies with some of them.

Is Yellow Fever merely an aggravated form of the Remittent Fever of most tropical countries? or is it primarily and essentially a different disease? Dr. F. frankly admits that he is unable to decide the question. There are several circumstances connected with the history of the former—more especially the fact of its unexpected outbreaks in particular seasons which exhibit no appreciable peculiarities, and its equally inexplicable subsequent lulls, while the invasions and departure of the latter are annual and can be calculated with something like certainty; not to mention also the singular circumstance of the black-vomit fever being confined to the African coast and the West Indies, and scarcely if at all known in the East—which must make the cautious enquirer pause before he takes upon himself to pronounce a positive opinion. In considering this question, it should not be forgotten that we are often very much puzzled to account for the exceeding malignancy of certain epidemics of scarlatina, measles, puerperal fever, erysipelas and so forth; while, in other seasons, these diseases, though still

* This definition may fairly be objected to; but we are to remember that the present work is a posthumous publication, and we must not therefore look for the same accuracy and precision of expression that would have been expected, had it been otherwise. Dr. F. himself subsequently disclaims the opinion that yellow fever is merely a *seasoning* fever.

existing, are comparatively mild and innocuous. To add to the difficulty of solving the problem, Dr. Fergusson states that the Yellow and the Remittent fever often prevail at the same time, running side-by-side of each other. But, whether we regard them to be identical or not, "one point has been clearly ascertained, that there is no contagion whatever appertaining either to the one or the other." The following passage is valuable, from the interesting illustration that is adduced :—

"No experienced men, unblinded by the prejudices of the schools and authorities, or biassed by the expectation of quarantine office, can seriously believe it (yellow fever) to be a contagion. It is a terrestrial poison which high atmospheric heat generates amongst the newly arrived, and without that heat it cannot exist; but it affects no one from proximity to the diseased, and cannot be conveyed to any low temperature. This was finely exemplified at Port au Prince, St. Domingo, where I spent the earlier months of the year 1796. Our headquarters were the town and its adjunct, Brizzoton, as pestiferous as any in the world, and there we had constant yellow fever in all its fury. At the distance of a mile or two, on the ascent up the country, stood our first post of Torgean, where the yellow fever appeared to break off into a milder type of remittent. Higher up was the post of Grenier, where concentrated remittent was rare, and milder intermittent, with dysentery, the prevalent form of disease; and higher still was Fourmier, where remittent was unknown, intermittent uncommon, but phagedenic ulcers so frequent as to constitute a most formidable type of disease; and higher still were the mountains above L'Arkahaye, of greater elevation than any of them, far off, but within sight, low down in what was called the bight of Leogane, where a British detachment had always enjoyed absolute European health, only it might be called better, because the climate was more equable than in the higher latitudes. Here were the separate regions or zones of intertropical health mapped out to our view as distinctly as if it had been done by the draughtsman. Taking Port au Prince for the point of departure, the three first could be traversed in the course of a morning's ride. We could pass from the one to the other, and, with a thermometer, might have accurately noted the locale of disease, according to the descending scale, without asking a question amongst the troops who held the posts: and what kind of contagion must that be, which, amongst men in necessary intercommunication, cannot be conveyed from the one to the other, which refuses to mingle with another of lower temperature, although within sight, and so near, topographically speaking, as almost to touch? The men could, and did, constantly exchange duties, but not diseases; and it was just as impossible, and more so, to carry a yellow fever up the hill to the post in sight, as it would have been to escape had they been brought down and located amidst the swamps of Port au Prince. These things were known to every person in the army, whether medical, civilian, or military, and amongst them all there was not to be found a single person who had the smallest belief in contagion, provided always he had been a year in the country, and possessed opportunity of seeing with his own eyes,—all, I may say, came out contagionists, myself amongst the number, none remained so." P. 153.

Dr. Fergusson adduces a variety of considerations to show that Yellow Fever is never essentially and intrinsically contagious. That it may acquire contagious properties, when the air, in which it was first developed, becomes vitiated with emanations from the sick, is highly probable: but this is an adventitious, not a necessary, property of the disease. The very exemption of the negroes from its morbific influence is a strong argument in favour of its being not contagious at first. Then, again, it is limited, in a great measure, to particular localities, temperatures, and elevations—

a character not very consistent with essential contagiousness. "Places, not persons," constitute the rule of its existence. It prevails during the highest tropical heat—the very condition that usually arrests the progress of infectious fevers, such as the plague (which usually ceases on the advent of Midsummer) and typhus.

In reference to the proper means to be taken to mitigate and arrest the pestilence, Dr. F. most justly remarks:—

"To pen up the inhabitants upon the infected ground is to aggravate the disease a thousand-fold, and is, in fact, as cruel and absurd as it would be to barricade the doors against the escape of the inmates of a house that had taken fire, on the insane pretence that they would otherwise spread the conflagration. The quarantine authorities will no doubt interpose to save the world from the dire contagion: but let them be referred to the annual epidemic at New Orleans for information how often the yellow fever has been conveyed from thence to the upper settlements on the same river, to which the fugitives, sick and well, uniformly fly for refuge, or even to the steamers that carried them; how often at Vera Cruz it has been carried out of the town even to the first stage on the road into the interior; or how often in Spain it has ever been transported, except to another station under the same circumstances, of heat and drought and defective perflation, therefore falling, if not having already fallen, into the same predicament from the same causes. Let them inquire whether seclusion and shutting up has ever saved the terror-struck. In the army of St. Domingo it was notorious that they were ever the first to be taken ill, and the surest to die; and during the yellow fever epidemic of 1816, at Barbadoes, I have recorded remarkable instances of the same both from my own observation and that of others. In short, whenever the endemic agency could not be avoided, the best place of safety was often to be found in the sick apartment, for there quietude reigned, and fatigue, exposure, and intemperance, were most likely to be avoided." P. 158.

Drs. Bone, father and son, are as decided anti-contagionists as Dr. Fergusson; and, as the former has had a long and most extensive acquaintance with the diseases of the West Indies, his testimony must carry very considerable weight. We may quote the following passage from the inaugural dissertation of his son upon this point:—

"In hospitals that are well constructed, and where a correct hospital discipline is enforced, the attendants of yellow fever patients are not liable to the disease.

"The Naval Hospital, Barbados, was built by Admiral Cochrane, and was well constructed; each orderly employed in the wards had a sleeping-room partitioned off from the ward. The sick of the Pyramus frigate and of detachments were treated in that hospital in 1821 and 1822. The total number of persons treated in the hospital from 21st of June, 1821, to 22d February, 1822, was 243. Of these, 101 were fever patients, and 38 were convalescents from fever. None of the patients under treatment for other diseases caught yellow fever; none of the servants, 38 in number, or of the other persons employed in the hospital, 53 in number, were affected with yellow fever. Twenty-two persons died of yellow fever, and were all, except one or two, carefully examined after death by Dr. Bone or by his assistants, Dr. Bain and Mr. Campbell, and none of them caught yellow fever.

"But is there any danger in entering an hospital where there are patients with yellow fever? Yes, if that hospital be filthy, crowded, ill-ventilated, and the patients lying on the bedding they have soiled: for the air must be vitiated, deprived of its oxygen, charged with azote, and other pernicious gases, and therefore unfit for respiration; and the sight is shocking and the groans of the dying frightful; but were the patients affected with any other disease, or, on admission

with no disease, and similarly circumstanced, the danger from breathing the vitiated air among them would be equal." P. 21.

Both Dr. Bone and Dr. Fergusson expressly assert that the disease is apt to be generated on board ship, in tropical climates, from an impure state of the hold. The latter, alluding to the cases of the Regalia transport from the coast of Africa, and of the Childers sloop of war—of which he has given an account in the 8th volume of the Medico-Chirurgical Transactions—assures us that, whenever the holds of these vessels were cleaned and purified, the fever ceased. "Before that was done, whatever stranger slept in either of them for a single night, was assuredly taken ill; whoever was brought sick ashore into the fully occupied wards of our general hospital, and received with open arms, brought with him as little infection as the new-born babe."*

We may point to the testimony of Mr. Birtwhistle also, late surgeon of H.M.S. "Volage," as given in a recent number of the Lancet (3d Jan. 1846.) In the Spring of 1842, yellow fever having broken out in this vessel at Port Royal, she was ordered by the commodore on the station to proceed at once to Halifax. For some time after her departure from Jamaica, the disease prevailed, and indeed with increased violence, on board; for fresh cases continued to be added to the list even for some time after the ship's arrival at Halifax, and did not completely cease until all hands were landed on Navy Island. Let us now see what opinion Mr. B. formed as to the producing causes of the fever, and consequently as to the best means of arresting its diffusion.

"After maturely reflecting on the origin, progress and termination of this dreadful malady in the 'Volage,' and weighing well everything which could in any way tend to produce or aggravate it, I must confess that it is difficult to come to a perfectly satisfactory conclusion; but it is, I think, evident that the primary cause was our lengthened stay at Chagres, Carthagena, and Santa Martha, where the fever first seriously presented itself, and where all the sources of malaria are most abundant; and that the manner in which it subsequently raged amongst the crew, first attacking the officers and servants residing in the steerage and after-part of the vessel, and thence proceeding to the adjoining mess of the marines, only one of whom escaped, and afterwards, forwards to the seamen, clearly indicate that in the vicinity of the pump-well there was some fatal miasma arising, which could not be, and was not removed until the thorough cleaning, drying, whitewashing, and fumigating, which she received at Halifax, all the stores and tanks being taken out for the purpose, and the holds being kept empty for nearly a fortnight. I had the gratification to find that the removal to the shore, and cutting off, for a time, all communication with the vessel, proved a most effectual remedy, inasmuch as from that period I had not a single new case, and the sick regained their strength in a rapid and surprising manner."

He adds:—

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* We need scarcely say that Dr. Fergusson rejects the idea of Cholera being a contagious disease: "always however with the proviso and exception of the possibility of its being made a temporary contingent contagion amidst filth and poverty, and impurity of atmosphere from overcrowding and accumulation of sick, but neither transmissible nor transportable out of its own locality through human intercourse."

"It will be perceived that the fever had gone on unchecked for nearly three months, the latter cases, at Halifax, being as urgent and dangerous as those which took place at the beginning, and from its great severity, and almost unexampled duration, conjoined with the facts previously mentioned, it is, I think, demonstrated that the cause existed in the vessel herself; and this opinion is further confirmed by the circumstance, that two officers came on board to assist us, both of whom were taken ill within four days, and the same occurred to two others who were only on board an hour or two on a visit to some friends, having been exposed, of course, to the same cause which operated on our own men. No one, therefore, could join the ship with impunity, although I am satisfied that the whole of the sick might have been landed in any part without the slightest risk to the inhabitants.

"As to the question which has been so much discussed, and upon which such discrepancy of opinion exists—I allude to whether this affection can be imparted from one person to another,—it will be observed, that I am a non-contagionist, but I am, at the same time, not prepared entirely to deny its communicability, especially situated as we were in the 'Volage,' with so many cases of a malignant character crowded into such a small space, and where the air, in spite of every precaution, was impure and offensive." P. 8.

After the details which have now been given, touching the very important question of the contagiousness of the yellow or black-vomit fever, our readers will be the better prepared to follow and appreciate the melancholy history of the "Eclair epidemy," which we proceed to lay before them.

The "Eclair," a new steam-sloop, left England under the command of Captain Estcourt for the coast of Africa, in Nov., 1844, with a crew of 146 officers and men. She reached Ascension on the 1st of December, and Fernando Po by the 25th. During the next two months, she kept cruising along the coast, anchoring every now and then for a short time at different places. On the 23d of February she was at Sierra Leone, and took on board the extra complement (40) of Kroomen and liberated Africans allowed on the coast. She remained there a few days, and then returned to Seabar, off the island of Sherboro. At this time, the men were much employed in the boats, blockading the place, and watching the movements of the slavers. They were often away for several days at a time, and had to sleep either in the boats or on shore, thus exposed to a tainted nocturnal atmosphere after a fatiguing day's work at the oars. It would seem that the men were sent upon this service six different times, and were absent altogether 29 nights. On some of these occasions, they were exposed to the heavy rains that fall at that season of the year. It deserves notice, at the same time, that in the place where the Eclair was anchored, the water was excessively filthy, from the washings of the mangrove marshes. The fresh water, used on board, was also very bad in quality. Several men suffered from diarrhœa and mild attacks of cholera. Besides all this, we learn that a degree of mental despondency prevailed among the crew, increased by seeing prizes captured by other vessels of the squadron frequently pass within sight, on their way to Sierra Leone.

Notwithstanding these many injurious influences, no case of decided fever occurred for five or six weeks at least. The first case occurred on the 3d of April; it was severe, but the man recovered. On the 18th, three men, who had been much away in the boats, were seized; one died on the 5th, and two on the 6th day of the attack. The type of the fever

was well-marked Remittent, often exhibiting very distinct intermissions. The head was generally much affected. No mention indeed is made, in the medical report of the ship, that the matters vomited were of a black colour; but it deserves especial notice that, in the account of three *post-mortem* examinations at this period, the following entries were made by Dr. Maconchy, the surgeon—"the stomach contained some reddish glairy matter"—"the stomach was distended with dark serous fluid mixed with black flakes"—"not a clot was seen in the body, but fluid blood exuded from the nostrils and blistered back." The average duration of the fatal cases seems to have been five or six days.

From the last date up to the 22d May, nothing particular seems to have occurred. The steamer continued to move about the coast; only one case of fever occurred, and the patient did well; and although many of the men suffered occasionally from slight diarrhœa, bilious vomiting, and such-like ailments, there was no serious illness on board. On the 22d of May, three new cases of fever were entered on the sick-list: they all proved fatal: one on the 5th, another on the 7th, and the third on the 13th day. Between this date and the 8th of June, other five cases occurred: four of the patients died in periods varying from three to nine days. Most, if not all, of the men seized had been engaged in the boat-service. The fever after this seems to have ceased for several weeks, until the Eclair again reached Sierra Leone. While there, part of her crew were employed in cleaning out the hold of the "Albert" steamer, which was in a most offensive state; it was supposed that nothing had been done to it since the vessel had returned, two years before, from the ill-fated Niger expedition. Moreover, the men sent on board the Albert seem to have had the opportunity of indulging in the most shameful intoxication; and, to add to their danger, permission was most unfortunately granted to them to go on shore, where some of them stayed all night, exposed to the most pernicious influences. On the 19th (of July,) the fever re-appeared on board the Eclair: the patient died on the eighth day of the attack. During the next three days, three cases more occurred; and all proved fatal. On the 23d the ship proceeded to sea, having the Albert in tow, and with twenty of her crew on board that vessel. They proceeded together to the northward, and remained at anchor off the coast from the 28th to the 8th of August; during which time a working party from the Eclair, in addition to the twenty already noticed, were employed on board the Albert. After leaving Sierra Leone, three fatal cases of fever occurred in July. A merchant, who had embarked on board the Albert at Sierra Leone, died on board that vessel on the 27th.* On the 1st of

* In the official report of Sir William Pym and Mr. (not Dr. as it is printed) Arnott, to which we shall afterwards have occasion to refer, it is expressly stated that "four days after sailing from Sierra Leone, one man died with fever and black vomit, the first case of the kind which had taken place: this man had been brought on board on the morning of the 23d, having been the three previous days on shore." No specific allusion is made to this case in the subsequent report of Dr. Stewart, which professes to give an account of the health of the Eclair from the 26th of August, 1844, to the 15th Nov., 1845; nor indeed is any mention made in it when the first case of decided black vomit occurred. All that

August, other two cases occurred; but they did well. On the 2d, there were two fresh cases: they proved fatal. On the 8th and 9th, four men fell sick; two died, and two recovered. (We have not been able to make out whether all these cases occurred in the Eclair, or whether some were on board the Albert.)

On the 10th of August—a most unhealthy time of the year on the African coast—the vessels arrived in the Gambia; the Albert was given up to the government there; and the Eclair remained until the 14th, when she sailed for Goree, having four sick of the fever on board. On arriving at Goree, she was refused pratique: one man died there. On the 17th, she proceeded to Bona Vista, one of the Cape de Verde islands, which she reached on the 20th: three men had died on the passage. Pratique was at once offered by the Portuguese authorities; but Captain Estcourt very frankly acknowledged that he had been put into quarantine at Goree in consequence of the fever on board, and he accordingly declined the generous offer of free admission until a sufficient inquiry had been instituted. Dr. Kenny, a resident English medical man, was commissioned by the governor to report upon the state of the Eclair. Upon consulting with her medical officers, this gentleman said that the disease on board was the common coast fever of Africa, and that there was no risk in granting pratique. This was accordingly done. As the fever continued to prevail—for five fatal cases occurred during the ten days after the arrival at Bona Vista—Captain Estcourt was anxious to have his own men landed, and the vessel completely overhauled. The Portuguese official physician as well as Dr. Kenny, being consulted by the governor as to the safety of granting the permission desired, gave their complete sanction to the proposal; the former gentleman emphatically saying: "no danger at all; I have often brought sick men ashore coming in vessels from the African coast, and I never knew any ill effects to arise." Accordingly, not only was Captain Estcourt's wish granted, but a small fort, situated on an island at the entrance of the harbour, was also most generously given up for the use of the sick; while the officers, &c., were permitted to reside in private dwellings in the town. Scarcely a single man was in perfect health at this time; all were ailing more or less, so that there were not hands enough to do the work of the vessel. The surgeon, Dr. Maconchy, was worn out with fatigue and anxiety; and the same was the case with Captain Estcourt, whose conduct towards his sick men appears to have been truly noble. The Eclair remained at Bona Vista from the 21st August to the 12th of September. While there, the hold was thoroughly cleared out, the tanks removed, &c., by the Kroomen on board, the crew having already been landed. The hold, we are told, was found to be clean, although very

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is said by Dr. Stewart on the subject is—just before describing the arrival of the Eclair at Bona Vista—to the following effect: "The fever appears to have been as distinctly remittent as that at Sherboro, and some of the men had unequivocal black vomit." It seems, therefore, that we cannot with accuracy determine when the first case of decided black vomit occurred. Sir William Pym and Mr. Arnott must have derived their information from the acting Commander Harston and Mr. S. Barnard, the surgeon; but then we are to remember that the latter gentleman only joined the Eclair at Madeira.

badly ventilated.* A gang of Portuguese labourers, about 30 in number, was employed on board the ship, in moving, hoisting, unloading, &c.; but none of these men, we are informed, went into the hold. The soldiers of the fort, too, communicated freely with the British sailors domiciled there. Notwithstanding the removal of the sick on shore, the fever continued to prevail and indeed to increase among the men. No fewer than 28 deaths took place from the 31st of August (the day of landing) to the 12th of September; making in all 39 deaths from the time of leaving Sierra Leone. We shall afterwards see that it is stated by Sir William Burnett that the men committed the greatest excesses in drinking at Bona Vista.

While the Eclair was at Bona Vista, the "Growler" steam-sloop, Captain Buckle, arrived from Sieora Leone on the 6th, just as the assistant surgeon of the former, Mr. Hartman, was taken ill: he died subsequently. Dr. Maclure, a naval surgeon, passenger on board the Growler, generously offered his services to replace his professional brother; the offer was at once accepted. For the next five days, Dr. Maclure was in constant attendance upon the sick in the fort. As the purser of the Eclair had died, an inspection of her stores was made by order of Captain Buckle; the senior officer, a lieutenant, the paymaster, the purser, and the clerk of the Growler officiating.

On the 12th of September, all the sick and the rest of the crew having returned on board the Eclair,† both steamers sailed from Bona Vista for Madeira, where they arrived on the 20th, but were refused pratique. During the passage, several deaths took place. Captain Estcourt, who had sickened on the 11th, died on the 16th; Dr. Maclure on the 17th; and Dr. Maconchy on the 21st, while the vessel was in Funchal roads.

Mr. Sidney Barnard, who was returning to England in the "Rolla," having volunteered his services, was appointed surgeon of the Eclair *pro tempore*, and Mr. Coffey, assistant-surgeon of the Growler, was sent on board as assistant-surgeon.

On the 21st the steamer sailed for England. The Eclair anchored at the Motherbank on the 28th, with twenty-three of the crew on the sick list: five of these were in bed with the fever, and the others (convalescent) in a very weak state. No fewer than 41 fresh cases of the fever had oc-

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* The Eclair was very badly constructed for ventilation. The main deck forward was found to be so close that Captain Estcourt had ordered two circular openings, each about 16 inches in diameter, to be made on each side of the forecastle for the purpose of admitting air more freely. The lower deck was dark and badly ventilated; and from the very awkward position of the hatchways, these being placed not immediately the one over the other, wind-sails could not be used with much effect to send air down. Moreover, there were no gratings in the floor of the main deck, nor any copper tubes, (as in some ships,) leading from the lower to the upper deck. The fore and after holds were utterly out of the reach of free ventilation. "Indeed," Dr. Stewart remarks, "it seemed as if there had been a studied avoidance of having the hatchways of the different decks in the same line."

† A council had been previously held by the surgeons of both vessels, at the desire of Captain Buckle, to determine upon what steps should be taken. It was unanimously agreed that the Eclair should proceed without delay to England.

cured from the time of leaving Bona Vista; and, of these 41, 12 had proved fatal.*

While at the Motherbank, two men were taken ill and died. On the 1st of October, a pilot having been taken on board, the Eclair was ordered round to the quarantine ground at Stangate Creek. Upon arriving there, all those of the crew, who had escaped the fever (41 in number,) were removed on board one of the line-of-battle ships in ordinary, the "Revenge;" while such as had recovered from it, and those of the convalescent as were in a state to be removed, were transferred to another two-decker, the "Benbow;"—leaving none on board the Eclair but the sick, the Kroomen (not one of whom had suffered,) and the acting Commander.

Between the 1st and the 9th of October seven fresh cases occurred; of these, three proved fatal. Mr. Barnard, the surgeon, was among the number; he sickened on the 4th and died on the 9th. The pilot,† who was one of those on board the Revenge, died on the 10th; and Lieutenant Isaacson, who was taken ill on board the Benbow on the 7th, died on the 12th. As the assistant-surgeon, Mr. Coffey, had had a slight attack on the 5th, it was necessary to supply his place by Dr. Rogers, the assistant-surgeon of the Admiral's ship at Sheerness: and, on the following day, Dr. Stewart was appointed surgeon of the Eclair. The former experienced a slight attack of the fever on the 10th; he gradually recovered. The same was the case with Mr. Coffey. No fresh case occurred afterwards; and by the 15th, all the sick were declared to be convalescent. On the 20th and the 21st, indeed, two men, who had acted as attendants upon the sick, and had afterwards superintended the clearing out of the Eclair's hold by the Kroomen, were seized with feverishness and irritability of the stomach; but these symptoms quickly passed away. Some of the Kroomen, too, had slight febricular attacks; but these might be owing rather to atmospheric vicissitudes than to any malarious agency.

On the 31st of October the Eclair was released from quarantine, Lieut. Harston (who had succeeded to the command upon the death of Com. Estcourt) having remained on board all the time.

Before alluding to the opinions of the official medical authorities at home touching the nature of the Fever of the Eclair, and the proper steps to be pursued under such distressing circumstances, we have a few words to say respecting the Growler, which, we have seen, was in company with the Eclair since the 6th of Sept. at Bona Vista. We have said that, while there, four of the officers of the former went on board the latter to make an inspection of her stores. Every one of them was attacked with the fever; one, the purser, was actually taken ill while on board the Eclair. All of these cases, as far as we learn, terminated favourably. One report states that not another case occurred on board the Growler: but

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* During the stay at Bona Vista, no fewer than 60 fresh cases occurred: of these, 31 eventually died.

† This poor fellow seems to have died of alarm rather than the fever. He had none of the symptoms of the disease; but he had scarcely eaten anything since he went on board the Eclair at Portsmouth. He sank with symptoms of cerebral oppression, accompanying a sort of low typhoid fever. His constitution was very unsound.

Sir William Pym asserts that there were five or six other cases.* However this may be, it would seem that none proved fatal. The disease must therefore have been of a much milder character, than that in the Eclair. The Growler arrived at Portsmouth on the 29th of September. Subsequently she moved round to Woolwich to be paid off. While she lay there, two men, engaged in clearing out the hold (which, it is stated, gave out a very offensive smell,) and who had been sleeping immediately over the scuttles of the forehold, were seized with fever on the 10th Oct.; the cases were severe, and the patients were sent to the Marine Hospital on shore. Both died "with all the characteristic marks of yellow fever:" so says Dr. Stewart. Sir W. Burnett states that "the matter vomited approached to the appearance of black vomit, but not entirely so." The latter gentleman goes on to say that "the fever was *most decidedly* not of an infectious nature; no farther case having taken place in the Growler herself, or in any of the attendants at the hospital."

There is a communication, dated the 20th October, from the store-keeper of Woolwich Dockyard, that a quantity of the slop-clothing, &c., returned from the Growler had a most offensive smell. The stores were immediately ordered to be sent down to the quarantine station at Stangate Creek, to undergo the necessary purification.

It may be well here to notice that "since the arrival of the Eclair and Growler," we quote from a letter of Sir Wm. Burnett of the 29th Oct., "H.M.S. Ardent has also arrived with her crew in a state of perfect health, and without a single vacancy in her complement. I have also further to state, that by a report from the surgeon of Her Majesty's ship Penelope, the squadron employed upon the coast had not been visited by any unusual sickness during the previous six months (the period included by the return,) and that the number of deaths from all causes did not exceed that of the most healthy years; nor is there the slightest reason to presume that any disease of a contagious nature prevailed in any part of the station."

Let us now see what measures, quarantine and other, were advised and adopted respecting the Eclair when she arrived in England.

Dr. Salter, of the Motherbank Quarantine office, was the first to report upon the state of the vessel. In his certificate, dated Sept. 28th, he states "that as many as 60 have died of fever, and that as many as 7 are still suffering from that disease. Three of the cases have occurred within the last week, and the type of the fever is of the most severe form—that accompanied with black-vomiting. Under these circumstances I have recommended the superintendent not to release the vessel. I cannot but consider the disease as contagious." Next day, Dr. Richardson, Inspector of Haslar Hospital, visited, or rather went to, the Eclair, by the command of the Port Admiral. In his report, dated Sept. 29th, it is stated that

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* In one account we read that "there had been occasional attacks of fever for many months; viz., in May, 12 cases; in June, 7; in July, 5; in August, when they had been at Ascension, and only returned to Sierra Leone on the 26th of the month, 3; in September, 12; and in October, 2." No particulars, however, are given of the type of the fever, the mortality, &c.

"65 of the crew have died of the bilious remittent fever, endemic on the coast of Africa, that 5 of them died since the Eclair left Madeira, and one last night."

"Notwithstanding," continues Dr Richardson, "the extraordinary mortality that has swept off so large a proportion of the crew of the vessel, I entertain no fears of her being the means of introducing epidemic disease into this country; and were the sick placed in well-ventilated wards, with fresh bedding and the other means of cleanliness afforded by an hospital, I anticipate no further risk to the attendants than would occur in wards set apart for cases of typhus fever; the decision on this point must rest, I conclude, with the Board of Quarantine; and should they decide on giving the 'Eclair' pratique, I should recommend the sick being removed to a wing of Haslar Hospital to be appropriated exclusively for them; and, lest alarm should be excited by any of the remainder of the crew being taken ill, it might be advisable to keep them on board their own ship for eight or ten days."

On the morning of the 30th, Mr. Arnott the distinguished surgeon of the Middlesex Hospital, was sent down by Government, along with Sir Wm. Pym, the Superintendent-General of Quarantine, to determine what measures should be taken. Mr. A., who had visited the Eclair before Sir W. arrived, had most judiciously arranged with the Admiral at Portsmouth to separate at once the sick from the healthy, by removing all the latter on board a frigate in ordinary. This most sound advice was not, however, carried into effect; as Sir Wm. Pym, taking into consideration the very sickly state of the crew, and the difficulty of communicating with the vessel at the Motherbank in boisterous weather, thought it better to order her round to Stangate Creek. What was done there with the crew, we have already seen. The report of Sir W. Pym and Mr. Arnott is dated Oct. 3rd.

On Oct. 7th, Sir Wm. Burnett, the Director-General of the Medical Department of the Navy, addressed to the Admiralty a letter, wherein he expresses a confident opinion that "the fatal fever on board the Eclair had been originally occasioned by the effects of malaria and exposure in the rivers, by the long continuance at Sierra Leone, and the many irregularities committed by the ship's company in that port." Sir Wm. continues: "It is necessary that I should add, that a fever, not originally of a contagious property, may become so when the sick are crowded together in a small, ill-ventilated place."

On the 22d of October, Sir Wm. Pym wrote to Government on the subject of the late fever. The opening paragraph of his letter runs thus: "In consequence of the arrival lately of men-of-war steamers from the coast of Africa, whose crews had suffered from the disease known as the Yellow, Bulam, or Black-vomit fever, a disease of a highly infectious nature, much more to be dreaded than the plague, and, if imported into this country during the summer months, would occasion a most frightful mortality, and would for a long period of time prove disastrous to commerce, in consequence of the rigid quarantine which would be established in all parts of Europe, but more particularly in the Mediterranean, upon all vessels arriving from the United Kingdom. It is a disease of a warm climate hitherto unknown in England, and imported by means of an artificial warm climate having been kept up during the voyage by the fires of

the steamers." To guard against the risk of importation, Sir W. Pym went so far as to propose that Government should give instructions to detain in future all men-of-war, and especially steamers, on the African and West Indian stations until the 1st of Nov.; so that they might not arrive in England before the winter months, at a season, "when the degree of cold would be such as is considered sufficient to destroy the infectious nature of the disease."

When this proposal was submitted to Sir W. Burnett for his opinion, it met with his strongest and most decided opposition; in which, it is but right to state, the Lords of the Admiralty expressed their entire concurrence. We shall make one or two extracts from Sir W. B.'s letter on the subject. After deprecating the proposal to detain men-of-war until November on the coasts of Africa and the West Indies, as fraught with great danger to the crews in consequence of the sudden transition from a tropical to a cold climate, he says:—

"It is perfectly evident from the history of the Eclair, and her proceedings on the coast, that the fever in question arose from causes totally distinct from infection, and that it was in fact the usual remittent fever of the coast, produced originally by the influence of marsh miasmata, heightened by the exposure of the men in boats, when absent from the ship for many days together, to miasmatic influence, and to the subsequent irregularities which the men committed in Sierra Leone, when they unfortunately obtained leave to go on shore, and where great excesses were committed—which combination of causes has never yet failed to produce a fearful increase of febrile disease, particularly on the coast of Africa. I may here add, that the increased mortality, which took place whilst at Bons Vista, is no longer a mystery; it was caused, I regret to say, by the most intemperate use of spirits I ever heard of. My informant told me that a bucketful of spirits had been offered to him. I do not mean to deny the possibility of this or any other fever becoming infectious under such circumstances as attended that in the Eclair; but there is not the least proof that it was so, while there are circumstances and proofs that inevitably lead to a contrary conclusion. But be this as it may, I have no hesitation in declaring my firm belief that the sick men of the Eclair, when that ship arrived at the Motherbank, might have been landed at Haslar Hospital, and placed in the well-ventilated wards of the establishment, without the public health suffering in the smallest degree." *

This opinion has been previously expressed, as we have seen, by Dr. Richardson.

Sir W. Burnett goes on to say: "It is a fact well known that, during the autumn of every year, merchant ships arrive in our harbours loaded with the produce of the coast of Africa, having perhaps lost great part, nay in some instances the whole, of their crew by fever of the country: or some are still labouring under fever when the ship arrives in the Thames, and are sent to the hospital in that state: yet no instance is known of any infection having been produced by such procedure; in fact, it is perfectly certain that it never did take place."

It was but fair that Sir Wm. Pym should have an opportunity of replying

* At the same time Sir Wm. suggested that his patent solution of Chloride of Zinc, which so instantaneously destroys foul smells, should be introduced into the hold, and applied freely to other parts of the vessel.

to the assertions and arguments of his opponent. In his reply, he contends that there are two distinct forms or types of fever prevalent on the coast of Africa: viz., the common Remittent, and the true and proper Yellow or Bulam fever.

The *former* is the same as the Walcheren fever, the malaria fever of the Levant, the jungle fever in India, &c. One attack does not protect from a second; and those who recover from it, suffer almost invariably afterwards from ague. It is *not infectious*.

The latter is a disease *sui generis*, is in no way connected with malaria or unhealthy situations, is unknown in the East Indies, is peculiar to the West Coast of Africa, is *highly infectious*, and therefore has often been imported into the West Indies, Spain, the island of Ascension, &c. Its peculiar symptoms in fatal cases is the vomiting of a black, coffee-ground looking matter: "this symptom does not exist in the remitting fever." Those who have recovered from it (the former) are protected from a second attack;* nor do they ever suffer from ague during their convalescence.

We need scarcely say, that Sir W. Pym regarded the "Eclair" fever as belonging to this second class; and by a variety of arguments he contends that it had been kept up and diffused on board by direct contagion. The circumstance of the disease raging with increased violence after the men had been landed at Bona Vista, is fairly urged as a strong proof of this opinion.

Sir W. Burnett, in a subsequent letter, stoutly, but in a somewhat too dogmatic, tone, utterly denies the accuracy of these distinctions attempted to be established between the (alleged) two kinds of fever; contending that "the more ardent form of yellow fever is a mere modification of the bilious remittent, so extensively known over all tropical regions, and hence can have no infectious properties, unless under such circumstances as an accumulation of sick in a crowded and ill-ventilated space." He proceeds to remark: "With respect to the subject of the importation of the disease into various places, except in one instance, and that even is surrounded with doubts—I mean that of H. Majesty's sloop Bann†—I entirely disbelieve it . . . . . I have caused the medical reports of the Jamaica Hospital for more than 20 years to be examined; and, though hundreds of patients with yellow fever in all its most appalling forms, including black-

* The truth of this is expressly denied by Dr. Bone: "The contagionist doctrine that yellow fever, of which black vomit and yellowness are diagnostic symptoms, affects a person once in life only, as a general rule, is true; for very few persons recover who have vomited black vomit, and of these few it may not be possible to discover any who have subsequently vomited black vomit; but, if it is argued that a person, who has been affected with fever in one epidemic of yellow fever, is not susceptible of fever in another epidemic of yellow fever, or in the same epidemic, the argument is altogether wrong."

† There were many circumstances connected with the fatal epidemy which broke out in that vessel that bear a very marked resemblance to those of the fever on board the Eclair. That the Bann, which had suffered dreadfully during the voyage from Sierra Leone to Ascension, introduced the fever into that island, cannot well be disputed by any one who peruses with candour the official Report that was published at the time (1824) by Sir Wm. (then Dr.) Burnett. An ex-

vomit, &c., have been treated in that establishment, not one of the medical officers in charge has even hinted at the disease being contagious."

It is certainly the opinion of a large majority of naval and military medical men, that the Yellow Fever of the West Indies is not primarily or essentially contagious; and it is a circumstance sufficiently remarkable in reference to the "Eclair" fever, that not one of the, alas! too many medical officers on board of her seem to have held the opinion that the disease had spread by infection from one person to another. In the official report, that was drawn up and signed by the surgeon of the Growler, and by Drs. Maconchy and Maclure, at Bona Vista on the 12th Sept., wherein they recommended the immediate departure of the Eclair for England, not a hint is thrown out about the contagiousness of the fever. The extracts, which we have given from Sir W. Burnett's letters, abundantly show the drift of his opinions upon the subject. While peremptorily denying the primary contagiousness of Yellow fever, he nevertheless admits that it may acquire this property under certain circumstances. When he wrote, nothing was known about the healthy state of Bona Vista after the departure of the Eclair; and he very properly attached great importance to what might take place in that island, as bearing upon the question of the contagion or non-contagion of the fever on board the vessel. The following passage occurs in one of his letters:—

"If it can be fully and satisfactorily shown that any person, who had so visited the ship or tents where the sick were placed, contracted the fever in question and communicated it to others, and they to other persons in succession who had never visited the ships or sick, then there can be no reason to doubt the infectious nature of the disease; but if nothing of this kind has taken place, then the conclusion must be that the disease is not infectious, and is therefore incapable of being communicated; in either case settling this long-contested question."

Dr. Stewart also candidly admits (the news of the outbreak of the fever at Bona Vista had just reached England when he was finishing his report) that, "if it shall appear that the fever, with which the inhabitants of Bona Vista are afflicted, first made its appearance after a reasonable interval in any of those who had been in communication with the crew of the Eclair, either in the town or at the fort; or if it first appeared among any of the Portuguese labourers who were employed in the Eclair, and subsequently extended to the families of those labourers, or any of their acquaintance who visited them when ill," he would readily recognize the contagious character of the disease. He had previously leaned, we may remark, to the opposite opinion.

On the 22d of December, Mr. Rendall, the British Consul at Bona Vista, wrote to Lord Aberdeen a letter, wherein, after detailing the circumstances connected with the arrival, stay and departure of the Eclair,

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cellent analysis—written by the late Dr. Johnson—of this report will be found in the Medico-Chirurgical Review, for January, 1825. Dr. Johnson wisely belonged to the moderate party, avoiding the excesses alike of the ultra-contagionists and the anti-contagionists, and advocating the doctrine of contingent or adventitious contagion. Many of his observations in the article alluded to will amply repay an attentive perusal.

he communicates the following particulars. The Eclair and the Growler left on the 12th Sept. Nothing occurred till the 20th; when it is stated that one of the Portuguese soldiers, who had mixed with the Eclair's crew, had died in the fort which had been occupied by them. As some of the other soldiers fell sick, the fort was abandoned, and the sick men were brought to the town and lodged in a house near the beach. The weather at this time was extraordinarily hot, and much rain had fallen. It deserves also to be mentioned that a large quantity of stagnant water had collected at the back of the town, and that the streets were in a most filthy and offensive state. The fever began to spread daily more and more; all the cases occurring within the immediate neighbourhood of the house where the first death had taken place. The native medical men still persisted in regarding the disease as not infectious. This opinion they continued to hold up to the 20th of November, when they openly declared that the fever was of a most malignant type and very contagious. "Up to the first week in December," writes Mr. Rendall, "the fever continued to rage; and, at that period, it had found its way into almost all the country villages—the deaths averaging seven or eight daily. The last report that I have received is to the 21st instant, (Dec.,) to which date 250 had died." The chief mortality was among the lower orders, whose state was most deplorable from want of food, and little or no medical advice or medicine. Several of the English residents died: among these was Dr. Kenny. The fever was believed to be the genuine black-vomit fever; it proved contagious, almost without exception, to the nurses of the sick."

Such is the substance of Mr. Rendall's letter.

Two days subsequently, Mr. Macaulay, Queen's Commissary Judge at the Cape Verde Islands, wrote to Lord Aberdeen upon the same subject, going over the same ground as Mr. Rendall, and confirming, in most particulars, his statements. Both gentlemen imply, and strongly express their regret, that the medical officers of the Eclair were not so frank and communicative, in their report of the character of the fever on board, as they ought to have been. For example, no mention was made by them of black vomit having been observed in any case, and they uniformly declared the disease to be only the ordinary remittent fever of the coast of Africa. Mr. M. concludes his letter in these words:—

"It is a satisfaction to know that the 'Eclair' had left Bona Vista nearly a month before any case of 'Eclair' fever exhibited itself in the town.* This circumstance, coupled with the fact that no injury whatever had resulted from the unrestricted intercourse which had subsisted during the whole of the 'Eclair's' stay in the harbour, between the officers and men (not in hospital at the fort) and their friends on shore, proves, I think clearly, that had the building which was so long used as a fever hospital been properly fumigated and purified prior to its re-occupation; and had the two soldiers, who were there seized with the fever, been kept (like the fever patients of the Eclair) from all intercourse with the densely-peopled and closely-built town, no bad consequences would have been experienced at Bona Vista from the visit of the 'Eclair,' and the kindness and hospitality which were there extended to her."

There has been another letter received from Mr. Rendall, dated 9th

* This statement is, it will be observed, at variance with what Mr. Rendall says.

March. By it we learn that "the sickness still continues in the villages of the interior of the island, (Bona Vista,) and that the number of deaths has amounted to upwards of 400."

Here we must not omit to mention that Dr. Stewart states in his Report (on the authority, we have understood, of the Consul's son, then recently arrived at Bona Vista) that the inhabitants of that town were healthy during the stay of the Eclair, but that yellow fever existed at the time in the adjoining island of Porto Praya. If this was really and truly the case, we might fairly conclude that an unhealthy condition of the atmosphere, such as must always powerfully favour and promote the diffusion of febrific miasms, prevailed over the Cape de Verde Islands at the very time when the fever seems to have been imported by the infected steamer into Bona Vista. We say, "*seems* to have been imported"—not that we have almost any doubt in our own mind that such was actually the case; but merely because we are unwilling to commit ourselves to any decided opinion upon the point in question, until we have the opportunity of perusing a medical report upon the state of things at that island subsequent to the departure of the Eclair. This, we are glad to learn, will ere long be the case; as Dr. Mac William, well known as the author of an excellent work on the recent Niger expedition, and who has had an extensive acquaintance with fever on the coast of Africa, has been sent out by the Admiralty to Bona Vista to investigate fully everything connected with the destructive pestilence with which it has been visited. Meanwhile, we would suggest to the surgeon of the "Growler" how much it might serve to elucidate the entire history of the "Eclair" fever, if he were to communicate to the public a report of the health of the former vessel for some time before her departure from Sierra Leone until the time when she was paid off at Woolwich. At the same time, we may state that it is our intention to bring the subject of Quarantine under the notice of our readers in our next number, having had the recent very able Report of the French Academy upon that question for some months past under consideration.

It would not be fair to the memory of a most meritorious officer and excellent man, if we closed this article without expressing the gratification we have derived from the perusal of the "Notes and Recollections" of the late Dr. Fergusson. With the prevailing spirit of its contents we cordially and completely agree.

ON THE TREATMENT OF STRICTURES OF THE URETHRA BY MECHANICAL DILATATION, AND OTHER DISEASES ATTENDANT ON THEM. By *James Briggs*, Senior Surgeon of the Lock Hospital, &c. London, 8vo. pp. 62.

HAVING observed the advertisement of this little work on one or two occasions in the foul ward (*i. e.*, column) of the newspapers, we were prejudiced against it as belonging to a class of publications which too frequently of late years have disgraced our profession; but an examination

of its contents proves to us its localization there was due to the mistake of the classifier of advertisements (for we suppose such a post must exist now these have so increased as almost to defeat each others' object of attracting attention) and not to the wishes of the author. It is a purely professional treatise, and a very good one too.

Dimensions of the Urethra.—Mr. Briggs first alludes to the discrepant statements which have been made by different authorities as regards the length of the *urethra*. The mode in which he has endeavoured to determine this is by the passage of a catheter without a stilette, graduated in inches and fractions from a single aperture at its end. As soon as the urine commences flowing, we may observe the point of the scale which corresponds to the external meatus. In 60 persons the length was found to vary from $6\frac{3}{4}$ to $8\frac{1}{2}$ in. In three-fourths of the whole, the persons being of middle stature, it was between 7 and 8 in. The correspondence of the length of the urethra with the stature of the person is found to be sufficiently exact for all practical purposes; and the medium length of the passage may be stated at $7\frac{1}{2}$ or $7\frac{3}{4}$ in. Under all ordinary circumstances, the length of the canal does not undergo any material alteration in the distended or contracted state of the bladder. The length of its different portions will of course vary, as does that of the entire canal, in different individuals, but in the cast of an urethra $8\frac{1}{4}$ in. long, Mr. Briggs found the following to exist: from the orifice to the membranous part $6\frac{1}{2}$ in., and thence to the bladder $1\frac{3}{4}$. The casts which Mr. Briggs has made of the calibre of the urethra do not confirm the representations of this, contained in the plates of Home, there not being the sudden constriction at the junction of the membranous portion and bulb depicted in these:—

"The portion of the urethra which extends from the apex of the prostate forward to a short distance beyond the arch of the symphysis of the ossa pubis, and in the natural state is the narrowest part of it, when distended, greatly exceeds the rest of the canal in its dimensions, and forms a large oblong sinus measuring from $1\frac{1}{4}$ to $1\frac{3}{4}$ in. in length, and in its transverse diameter at the broadest part from $\frac{1}{2}\frac{0}{0}$ to $\frac{1}{2}\frac{7}{0}$ in., the part of the urethra anterior to it not exceeding $\frac{7}{10}$ in. The broadest part of the sinus lies directly under the cartilaginous arch of the symphysis and above the bulb of the urethra, the latter of which with Cowper's glands lies over and conceals the membranous portion. The bulb is continuous or nearly so, with the prostate gland, and is covered by a dense fascia, which extends to the *erectores penis* muscles, there being little or no space between them, such as is usually artificially represented in dissections of these parts when the bulb is detached from the membranous portion and made to appear pendulous. The narrow part of the canal, as seen in these injections of the urethra, is at the point of union between the membranous and prostatic portions, *i. e.*, immediately before its entrance into the prostate gland, where the two are united by a very small neck."

The *curve*, too, of the urethra comprises not only the portion of the canal usually assigned to it, viz., the bulb and membranous portion, but also the prostate and a portion of the canal anterior to the bulb, "commencing, at about an inch anterior to the arch of the pubis, or the point where the corpus spongiosum becomes fixed, corresponding with the boundary marked by the anterior fibres of the *acceleratores urinæ*, the prostatic portion being rather more curved than the rest." The curvature is much greater

prior to puberty; but that of the adult urethra does not undergo much variation, and the degree of this which does exist seems to bear some relation to the stature of the individual; for in persons of short stature, the sweep which an instrument makes in entering the bladder "is more sudden, and the handle carried lower than in those of large make, in whom it is oftener requisite to press the catheter back in a horizontal direction." Mr. Briggs denies the existence of *spasmodic stricture*, at least as dependent upon muscularity of the urethra. The *muscles* of the perineum encompassing the canal may at their points of insertion give rise to its compression, and impede the introduction of instruments. Then, again, the *lining membrane* of the urethra possesses remarkable *elasticity*, so that, on examining the body of a child three years old, the author could scarcely obtain the admission of the point of a knitting-needle into this canal.

The directions given by the author for the *introduction of instruments into the bladder* are brief and judicious. He believes we best succeed with an instrument the curve of which is rather less than the natural curve of the urethra; and much prefers placing the patient in the erect posture, as being that in which we can be most certain of the exact position of the point of the catheter. The weight of the instrument alone overcomes the resistance offered by the muscles surrounding the fixed portion of the urethra. It is more easily introduced by turning the concave portion downwards, care being taken that the point does not recede from the part of the canal it has reached, when the *tour de maître* is made. All force is to be abjured, and if this is necessary to secure the onward progress of the catheter, we may be certain that its point is not properly directed. In the natural state of parts it is usually at about 5½ in. from the external orifice that resistance is met with, this being the point where the urethra becomes somewhat narrower, the direction of the canal changed, and the anterior edge of the levater ani inserted—circumstances also explaining the frequent occurrence of stricture here. If the point of the instrument on reaching the looser and larger portion of the urethra corresponding to the bulb is too soon elevated, or pushed forwards too hastily towards the narrower part, this looser portion of the urethra is carried forward on its point like a pouch, giving rise to the erroneous belief that the catheter has entered the bladder. Mr. Briggs does not believe this difficulty can generally be got over by drawing the penis forward on the catheter, introducing the finger into the rectum, &c., and that such manœuvres are unnecessary, as the point of the instrument may be adjusted to any angle we wish by due management of the handle; the error indeed generally consisting in our raising this before it has entered the fixed or ascending portion of the urethra. Repeated attempts only irritate the parts, and, if the necessity of the case be not urgent, their repetition had better be deferred.

Treatment of Stricture.—Mr. Briggs disapproves of the treatment by caustic in any case, believing effective dilatation sufficient. He considers the common wax bougie and other flexible instrument as much less manageable and easily directed than an inflexible one, except indeed the stricture be situated at the anterior part of the canal. Moreover in its passage its point often becomes entangled and irritates the canal when the resistance of this is endeavoured to be overcome. Although not a proper means for

treating stricture alone, its flexibility often renders it a useful auxiliary. Mr. B. has long been in the habit of using a succession of wedge-shaped sounds.

"The conical part of the instrument is about an inch in length, extending from the bend to a short distance from its point, the rest being of equal thickness. It has a definite curve, and is ground on each side, so as to leave an obtuse edge or projecting line running on the concave side, from within half an inch of the point to the part where the curve terminates. The extreme point to the extent of a quarter of an inch is uniform. The point of one instrument corresponds in size with the thick part or stem of the next, or is somewhat less. The difference between any two of the instruments in thickness, or between the point and stem of each, is about $\frac{1}{48}$ in. or $\frac{1}{4}$ of the French line: the point of the smallest of these instruments measures in thickness $\frac{3}{48}$ or $\frac{1}{16}$ in., the stem $\frac{4}{48}$ or $\frac{1}{12}$ in.; that of the largest instrument $\frac{11}{48}$, the stem $\frac{12}{48}$, or $\frac{1}{4}$ inch. The series consists of 10 or 12 such instruments, but provided the principle of proportion in their construction be kept in view, the number of precise dimensions are, I think, immaterial.

"By giving an obtuse edge to a part of the curve of the instrument, which by the flattening of the two sides is nearly triangular, less resistance is made to its passage through the stricture, and a dilatation of the part is far more readily effected than by the mere conical sound; and more especially in the denser kinds of stricture, the removal of which cannot be effected without great perseverance in the ordinary and tedious process of treatment by the common bougie. * * * * * * * * * * By the introduction of such instruments in almost uninterrupted succession, the contracted part of the urethra may often be enlarged, almost at pleasure, to the extent of the rest of the canal, and very often at one time, especially where the stricture is confined to a very small portion of the canal, with as little pain as the patient would experience from a single introduction of a full-sized bougie."

These instruments, intended to produce the enlargement of the narrowing, cannot, however, be employed until the exact situation of the stricture is ascertained and a passage through it effected. To attain this, a very small bougie should be passed down to and pressed gently but steadily against the stricture, or if this is unsuccessful, a fine steel sound of uniform thickness ($\frac{3}{48}$ inch) is to be similarly used. In cases of difficulty, the attempt should be made immediately after the patient has urined. By these means, in a great majority of cases, the stricture can be penetrated, and even where this takes place only to a very slight extent the urine flows easier, and the instrument will pass on the next occasion. Where the stricture is so narrow as not even to admit the point of the finest wax bougie, Mr. Briggs has occasionally passed a whalebone bougie, with a point scarcely larger than a bristle. "In order to fit the whalebone for this purpose, it is necessary to soften it in warm water, and keep it for some time in a catheter, by which it will acquire a permanent curve." When once the stricture has been in this way passed, the author believes the contracted uretha may be enlarged by the conical instruments before-mentioned at option. Of course the number of times these may be required, as also their gradation, must depend upon the extent and closeness of the stricture; and one good piece of advice is given, viz.—that a smaller instrument should first be introduced at a sitting than the largest one which had been introduced on the previous occasion. When the instrument is firmly grasped and removed with difficulty, it is better to allow a few days to in-

tervene before repeating its introduction; and it is not desirable to allow it to remain within the urethra more than a few minutes on each occasion.

Stricture is most commonly met with about 5½ inches from the orifice, or at the point nearly corresponding with the arch of the symphysis. It occurs less frequently at about 3½ inches near the root of the penis, being much more commonly cartilaginous in this situation. It is here, too, more tedious in cure, more liable to relapse, and oftener attended with incontinence of urine. Straight catgut bougies are often advantageously employed in its management here; but the cure is much expedited by the use of the wedge-shaped wire sound already described.

We may conclude our notice of this work by extracting a few observations upon the management of *retention of urine from stricture*. After observing that retention usually occurs from inflammatory or spasmodic action being superadded to the mechanical obstruction requiring general measures, as bleeding, warm-bath, opiates, and large doses of calomel for its relief, Mr. Briggs continues:—

"In old and confirmed strictures the part is generally so much contracted as to render the introduction of a catheter even of the smallest size rarely practicable. The only means which seem to promise success are that of passing a fine bougie down to the contracted part, and endeavouring to force the point of it within the aperture. If this can be effected, and the instrument suffered to remain for some time, on its removal the urine will sometimes be discharged in a very small stream, or by drops; but at each successive effort a small quantity will be voided sufficient to relieve the most distressing symptoms, and by a perseverance in the same measures, the stricture may be gradually enlarged, and the necessity of puncturing the bladder avoided. Where the opening of the stricture deviates, as is often the case, from the line of the urethra, a fine bougie possesses an advantage over every other instrument, in adapting itself to the irregularity: but in proportion to its fineness it becomes more liable to bend on the slightest resistance, and when the point has entered the contracted part, the waxy coating is ruffled up so as to prevent its advancing further. The instrument proposed (the fine steel sound) is not only free from the latter disadvantage, but, by uniting the two qualities of firmness and tenuity, gives the operator a command over it which he cannot have upon soft or flexible instruments. On this account the two kinds of instruments will be often found subsidiary to each other, and more useful than when employed separately. In cases where the stricture has been so narrow as not even to admit these instruments, I have more than once succeeded with a fine curved whalebone bougie, so tapered that the point has scarcely exceeded the size of a bristle.

"The notion of the absolute necessity of introducing a catheter into the bladder in these cases, leads often to ineffectual attempts to perform what is impracticable, and thereby to render the use of other means more difficult and uncertain. The simple introduction of an instrument, however small, through the obstructed part, when the retention arises from this cause, will, I believe, be found sufficient to procure a flow of urine, and to relieve the dilatation of the bladder, and thereby allow time for the dilatation of the stricture. And it is worthy of observation, that, however small the quantity may be which is voided, it will, *pro tanto*, take off the distension of the bladder, and consequently the straining and spasmodic pain, almost as effectually as if the bladder had been emptied by drawing off the whole of its contents by means of the catheter."

Mr. Briggs does not of course intend his little work for a complete treatise on stricture, and hence there are many points he entirely passes over which would have yet furnished interesting subjects for comment.

His object is merely to state the means of relieving stricture which he has found most successful, and his observations seem to us useful and judicious. We cannot however agree with him in the entire proscription of caustic, and believe that the rapid cure he describes following dilatation by metallic sounds will seldom be found permanent. Moreover, we regard these fine wire sounds as dangerous instruments in the hands of any one who is not constantly exercised in their employment.

A SERIES OF ESSAYS ON INFLAMMATION AND ITS VARIETIES. Essay 1. The Natural History of the Disease. By *H. Clutterbuck*, M.D. Octavo, pp. 67. London, 1846.

OF all that falls within the province of the practice of medicine, Inflammation always has obtained, and seems destined always to demand, the greatest share of attention. Its external and more obvious characters have long been minutely described, and are now well known, but its intimate nature, even with the aid of the microscope, is a matter of keen dispute, and affords a field for investigation that promises fertile results. Independently of this, the observation of disease has long taught most men the vast influence which it exerts in the production, course, and management of the various phenomena of morbid manifestations; and although the time has gone by when these were regarded as mere varieties of expression of the same essential and identical fact, yet many will yet agree with our author in affirming the proposition these Essays are intended to establish—"*That most diseases either consist in inflammation, or are consequences of it, more or less remote.*" Few have advocated this doctrine more boldly or more successfully than he has heretofore done in respect to a class of affections, for the explanation of which it would to most seem defective or faulty; and we are glad to find so veteran a writer again in the field, armed, we hope, with additional facts and arguments. Whether this is the case or not, however, we shall not yet have an opportunity of judging, as the present part of the work treats only of the natural history of the disease. The *characters* and *causes* of inflammation are lucidly stated, but we will confine ourselves to the section upon its *nature*. Dr. Clutterbuck maintains that the humoral pathology is in no wise competent to the explanation of the production of the phenomena of inflammation, and makes the somewhat obscure statement, that a vitiated state of the *fluids*, when it does occur, is for the most part the *effect* of disordered action of the *solids*; and to be viewed rather in the light of an *exciting cause*, than as constituting the disease itself." It does not seem very clear how it can be both of these. In searching for the *proximate cause* in the solids, we must not limit our attention to the inspection of the effects of the disease on the dead, but likewise interrogate the phenomena in the living. Mr. Hunter and his followers maintain that the blood-vessels are more active in this state, and transmit the blood with greater force and velocity. They appeal to the comparative rapidity with which it flows

from an inflamed part, and to the enlarged condition of the veins issuing from it—increased redness and augmented temperature being the natural consequences of these conditions. The sensibility and irritability of the part are increased, its movements more energetic, and its bulk augmented by increased secretion or new growth.

"Notwithstanding these strong, and, one would think, conclusive proofs of *increased activity* of the blood-vessels in *inflammation*, and incompatible as they seem to be with the notion of *debility*, there are, nevertheless, not a few who still maintain the opposite opinion—namely, that, in *inflammation*, the action of the vessels is weakened, instead of being increased, and the circulation in the part carried on more slowly and feebly than in health. The chief argument adduced on this occasion, seems to be derived from the enlargement of the vessels of the part; *distension* being considered as necessarily implying *weakness*. Thus, Dr. Billing observes, 'that the natural action of the arteries is *contraction*. Now, as the arteries in an *inflamed* part are larger than before, they must have contracted less, and consequently have acted with less force.' Another distinguished teacher and writer of the present day, (Dr. C. Williams,) expresses himself to much the same effect. Speaking of *determination* of *blood* to a part, he denies that this is caused by *increased* action of the arteries, 'because,' he says, 'the only *active* property we know these vessels to possess is that of *slow* or *tonic contraction*; and this would *diminish* instead of *increasing* the motion and quantity of blood proceeding to the part.' 'The enlargement of vessels,' it is further said, 'where a determination of blood takes place, is effected by *arterial distension from behind*, acting on a tube that has already lost some of its contractile power. The arteries, thus enlarged, become channels for the conveyance of more blood, and with more force, into the *capillaries* and *veins* leading from them; these will become, in like manner, enlarged, and share the increase of force and motion thus supplied to them.' But if, as here alleged, the distended arteries of the part are in a *weakened* state, and the only active property of arteries is that of *slow* or *tonic* contraction,—whence, it might be asked, is derived that 'arterial distension from behind' which is said to produce the enlargement of the vessels of the inflamed part, seeing that the action of the heart is not at all increased in these cases? It is, besides, not easy to understand, how an enlarged tube, 'that has already lost some of its contractile power' in what is called *determination of blood*, should *thereby* become a channel for the conveyance of more blood, 'and with more force,' into the capillaries and veins leading from it, the contrary rather should be the case, according to ordinary hydraulic principles.

"The notion, that the only active property possessed by arteries is that of '*slow* or *tonic* contraction,' leads to a denial of their having any share in the general circulation of the blood, this office being supposed to be performed by the heart alone; as, indeed, has been of late contended for also by Dr. Marshall Hall and a few other physiologists. The fallacy of this, however, was so clearly pointed out by Mr. Hunter, that one wonders so unfounded a supposition—one so inconsistent with striking facts—should still be held."

After adducing proofs that the arteries really possess an *oscillatory* action, *i. e.*, a condition of alternate contraction and relaxation, Dr. Clutterbuck continues:—

"Such are the chief grounds that may be adduced in favour of the opinion entertained by Mr. Hunter and his numerous supporters, with regard to the *intrinsic* nature of *inflammation* considered as a *vital process*; viz., that it is a state of *increased activity*, chiefly observable in the *blood-vessels* of the part, these being the principal agents in effecting the various physical changes that are taking place. But while it is contended, that in *every inflammation* there is an increase

of *vital activity* in the part affected, it is of great importance to bear in mind, that it is not simply a state of *increased action*, but an action of a preternatural and morbid kind, and subject to new laws, widely differing from those of health; as is proved by the results above stated. Of the *intrinsic* nature of this, however, we are, and probably ever shall remain, ignorant. We know it by observation alone, and by comparison with the healthy state. In proper management, likewise as a matter of practice, is only to be learned in the same way, namely, by observation and experience. And even these are but uncertain and insufficient guides; because the disease is subject to be influenced by a variety of circumstances, many of which are unknown to us, and many beyond our ability to control. This acknowledgment of the imperfection of our art should inspire caution in the application of remedies; more especially, in the present day, when the *Materia Medica* consists, to so great an extent, of the most deleterious articles."

In his section on the *varieties* of inflammation, Dr. Clutterbuck characterizes many diseases as inflammatory not usually considered such, among which may be reckoned tic douloureux, ague, phthisis, &c., &c.; but we must reserve our objections to this summary procedure until we are in possession of the defence of his views in the future parts of the work.

The present part is a very able one, and fully maintains the reputation of its author.

OUTLINES OF THE COURSE OF QUALITATIVE ANALYSES FOLLOWED IN THE GIESSEN LABORATORY. By *Henry Will*, Ph. D., Professor Extraordinary of Chemistry in the University of Giessen. With a Preface by Baron *Liebig*. London, 1846.

TRANSLATIONS of the works of German chemists into English have lately been abundant, and it would be unfair to deny that they have been useful to the student; and the present work, though somewhat wanting in details, forms no exception to the general character of the class. As a translation we have no doubt that it is a correct one, but this is not all that is wanted or expected by the pupil; the original should have been accompanied with notes by the translator to have rendered it extensively useful; there are besides other deficiencies—for example, though we have abundant formulæ, there is no list of equivalents appended, to the book, so that we have to guess at their value. Baron Liebig, in the Preface, has stated that "the present work is designed for the use of the laboratory; consequently everything, which does not immediately refer to the processes of analysis, is very properly excluded." Let us admit the accuracy of this statement, and yet the praise is of a truly negative description. It would have been more to the purpose, if the Baron could have assured us that it does contain all or even a large portion of what *does* immediately refer to the subject treated of.

We shall offer a few observations on parts of this work taken entirely at random, and, if the translation be faithful, the original contains statements which surprise us as coming from so well known and justly appreciated an authority as Dr. Will. For example it is men-

tioned with respect to oxide of manganese, that "most of the salts of this oxide dissolve in water, yielding pale rose-coloured solutions, and all of them are soluble in hydrochloric acid." Now, though we have seen slightly reddish-coloured solutions of manganesian salts, we believe the colour to have arisen entirely from some of the metal being in the state of acid. Of this we are at any rate perfectly sure, that we possess solutions of protosalts of manganese which are perfectly colourless, and therefore to state that a pale rose-colour is characteristic of them is utterly fallacious.

Under the head of "Acids of Nitrogen," we have first nitric acid represented by HO, NO^5, and it is stated that "pure hydrate of nitric acid is a colourless liquid, diffusing white fumes of a pungent odour, when in contact with the atmosphere. Now the strongest nitric acid obtainable is a sesqui-hydrate, and ought therefore to be represented by $3HO, 2NO^5$.

It is moreover stated that, "with the exception of binoxide of tin, peroxide of antimony, (tellurous and tungstic acid,) all oxides are soluble in an excess of nitric acid;" this is surely a mis-statement, for binoxide of manganese requires the presence of a deoxidizing agent, as sugar, to become soluble in nitric acid; and binoxide of lead is also insoluble in it, and red oxide of lead is decomposed by its action. It would surely, therefore, disappoint and surprise the student to find no action whatever occurring in the two first cases, and that, in the third, he obtained a brown powder insoluble in the acid.

The author states that, "when ignited, carbonates lose their carbonic acid, with the exception of the carbonates of the alkalies and some alkaline earths (baryta and strontia.)" Now carbonate of baryta we know, and that of strontia we believe, to be decomposed by heat.

Some other statements have struck us as requiring correction, and these, we have no doubt, that the translator will discover to be erroneous; and by the time that a new edition of the work is required, we trust that the requisite attention will be paid to the alterations requisite to render it perfectly worthy of the confidence of the chemical student.

A TREATISE ON MEDIATE AUSCULTATION AND ON DISEASES OF THE LUNGS AND HEART. By *R. T. H. Laennec.* Translated by a Member of the College of Physicians. Edited by *Theophilus Herbert*, M.D. With Practical Notes, condensed from the Lectures of *F. H. Ramadge*, M.D. Octavo, pp. 862. London, 1846.

WE felt somewhat surprised at perceiving the announcement of a new translation of Laennec's immortal work, inasmuch as the profession is already in possession of one by an accomplished physician, well qualified to do justice to the task. Upon examining the book, however, we have been still more so, for we could not have believed that any man would have had the hardihood to dedicate a translation of the work of this great and scientific physician to a person of the calibre of Dr. RAMADGE—yet

so it is; and not only this, but preface and notes are filled with the most fulsome adulation of this personage; their writer evidently implying, that if we are to do honour to him who placed in our hands the means of distinguishing diseases of the chest, and left us the best description of their characters, we must surely be prepared to prostrate ourselves before the hero who tells us he can cure the most deadly of them. This is not the occasion we should choose for the exposure of the fallacy of Dr. Ramadge's views, and the curious statements they are attempted to be supported by: but we could not allow his former pupil and friend to bring his name into juxta-position with that of the great Laennec, without protesting against it, as, to say the very least, a piece of gross absurdity.

The translation (by another hand) is executed fairly enough, and several of the notes by Andral are valuable; but why the work was ever published, save for the laudation of the aforesaid Dr. Ramadge, we cannot imagine. To those who may wish to become acquainted with his views, it may prove acceptable, for they are reiterated at every opportunity, fitting or not: but we believe they are too generally acknowledged as erroneous to secure many purchasers on their account.

REMARKS ON MEDICAL ORGANIZATION AND REFORM, FRENCH AND ENGLISH. By *Edwin Lee*. 8vo, pp. 121. LONDON, 1846.

IN our own struggle for medical reform, Dr. Lee believes it may be useful to exhibit the systems of Medical organization which prevail in France, Germany and Italy. These, however, are for the most part far too elaborate, methodical, and despotic, to admit of being transplanted among us. Indeed, before admiring and wishing to imitate them, we should observe how indifferently they work in their own respective countries, among persons much more accustomed to submissiveness to authority than we are; and do we not find that the profession is everywhere in a very discontented and unsatisfactory condition? That this is the case in France is notorious, and much that Mr. Lee mentions as regards that country is rather what the Congress desires, than what really exists; and in respect to essentials, we do not hesitate to affirm that, as regards the mass of medical men, their condition is much more to be deplored than in our own. We are not advocates for the transplantation of any cut-and-dried system from one country to another, in which the manners and institutions are not fitted for its reception. Each country has its peculiar abuses to remove or diminish; for, after all, the great thing we want is to be allowed unrestricted means of improving our professional position. This can only be done by improving our education, by bringing us more together, so that we may discern and support our true interests, and by preserving us and the public, from the injurious competition of uneducated and ignorant persons. The two existing medical incorporations are quite competent to these ends, but are unwilling to accomplish them. It behoves, then, the *great mass of practitioners to unite boldly and firmly together*. Their forbearance has hitherto been met with contumely, neglect, and opposition. They have

the power in their own hands, either of bringing their adversaries (for we regret the conduct of the corporate bodies justifies this term,) to a capitulation, or of erecting themselves into so important a rival institution as to render this a matter of no consequence. Their organization is the first essential object. That effected, they may do all they can legitimately desire; without it, they will continue to be tantalized, derided, and deceived as they have hitherto been.

Much of Mr. Lee's work is taken up with an exposition of the abuses prevailing in the Colleges of Physicians and Surgeons; but we think the profession is by this time fully aware of the nature and extent of these, as also of the impossibility of their amendment by any means short of those we have adverted to.

BEITRÄGE ZUR OHRENHEILKUNDE. Von Dr. *Wilhelm Kramer*. Nebst 19 Statistischen Tabellen. 8vo. pp. 314. Berlin, 1845.

Contributions to Aural Medicine, with 19 Statistical Tables. By Dr. *William Kramer*.

THE name of Dr. Kramer is well known in this country as the author of an excellent treatise on Diseases of the Ear, published some years ago, and which was translated by Dr. Bennett. We reviewed the translation at considerable length, and reported favourably of it at the time of its publication—vide Med. Chir. Rev. for July, 1838. The present work consists chiefly of statistical tables and illustrations that serve to confirm the leading positions in his former one. The other articles are a paper on the Acoustics of the Human Ear, one on the application of Electro-Magnetism in the Treatment of Deafness, and one on Cerebral Otorrhœa. The writings of Dr. Kramer everywhere exhibit the spirit of an enlightened and judicious author, in whose statements we can place perfect reliance. Would that we could say the same of most aurists in this country! There is no branch of the healing art in which charlatanery is more rampant than in aural surgery, among those, at least, who profess to be ear-doctors. In short, they are little better than advertising quacks.

THE YOUNG STETHOSCOPIST, OR THE STUDENT'S AID TO AUSCULTATION. By *Henry L. Bowditch*, M.D. 8vo. pp. 276. Boston, 1846. Ticknor & Co.

WE have been much pleased on the whole with this manual, and can truly recommend it to the student of Auscultation as an excellent guide. We had hoped to have found space to have noticed its contents more at length; but the extension of some of the articles in the present number has put this out of our power. The very copious index adds very much to the value of the work It would be well that authors in general would pay as much attention to this important point as Dr. Bowditch.

Periscope;

OR,

CIRCUMSPECTIVE REVIEW.

Selections from the Foreign Periodicals.

On the Use of Turpentine as a Collyrium in various Diseases of the Eye. By Dr. Laugier, Surgeon of Hôpital Beaujon.

M. Serres d'Alais having mentioned to Dr. Laugier the success he had met with in the application of the Essence of Juniper to the eye in *chronic keratitis*, with an anormal development of the corneal and conjunctival vessels, it occurred to him that *Venice Turpentine* might have a like good effect in similar cases, and he therefore gave it a trial upon several cases of acute and chronic conjunctivitis, keratitis, &c., he had under his care. All these patients were already using the *nitrate of silver* lotion, and the substitution of the turpentine was attended with marked benefit. Other cases have also been treated, and justify the author in declaring the utility and harmlessness of this remedy. He has also found it of service in opacities of the cornea. M. L. allows that his experiments with this lotion have not yet been frequent enough: but they have been sufficiently so to justify his publication of their results. After various trials he has found the following formula that best adapted for use, although some patients can bear the pure essence without any admixture.

Venice Turpentine, 20 parts,
Essence of Turpentine, 10 parts,

Place the turpentine in a marble mortar and gently heat it; and when it has become fluid, add the essence gradually. Drop three or four drops into the eye night and morning.—*Archives Générales.*

Fracture of the Lower Extremity of the Radius.

M. Blandin, in a clinical lecture upon this accident, observed that authors erroneously represent *pronation* to be impossible, while in fact it can be effected, but induces great pain. Another gratuitous assertion is, that the *interosseous space* is destroyed, when indeed it can scarcely be said to exist at all at the lower part of the fore-arm. This it is important to bear in mind, in order to avoid the injurious, or at least useless, practice of applying graduated compresses for the purpose of maintaining the space between the two bones.

The history and appearance of the case usually suffice for the diagnosis, a fall on the wrist, or rather on the palm, being the most common cause of the accident. It is, however, not unfrequently confounded with *sprain*, in which deformity of the joint from effusion also takes place. There is, however, much less mobility of the part than in fracture, and the characteristic depression of the edge of the limb (*le coup de hache*) is absent. The mere absence of crepitation does not indicate anything, as it is not often perceived in this fracture. The

confounding this accident with *luxation of the wrist* has, in a great degree, arisen from the minute description given by surgical writers of this last, as if it were of common occurrence, whereas it is very rarely met with, and indeed never, except as the result of great external violence, when it is also accompanied by more or less injury of the soft parts or fracture of bones. At all events, it is never produced by a simple fall on the palm of the hand.

We have placed the limb upon its ulnar side. Some surgeons only continue the splints to the lower end of the fore-arm, but if we wish to act upon both fragments we must support the hand also in the apparatus. When there is antero-posterior displacement, the mere cubital splint will not suffice, and we have seen Dupuytren much surprised at its ill-success. Compression is required at the anterior and posterior faces of the fore-arm. To supply this, we employ broad, but not graduated, compresses, merely doubling them where most pressure is required. Thus, *e. g.*, if they are so folded as to present but one thickness opposite the lower fragment, and four or five thicknesses opposite the upper one in front, the disposition may be reversed behind, so as to give us a double purchase upon the broken ends of the bone. When inflammation and swelling have subsided, these may be covered with the starch bandage.

M. *Robert*, lecturing upon the same accident, observes that, like in fractures of the other pyramidal bones, the upper fragment is frequently impacted in the lower one, more or less completely. At the palmar face of the limb, near the carpus, the anterior edge of the upper fragment forms a projecting crest beneath the skin, while posteriorly there is a corresponding depression: on this account, and because of the obliquity of the fracture, the lower part of the limb much resembles the letter Z in form—furnishing a very characteristic mark of the accident. M. Velpeau represents it as a constant one: but it may be absent, until produced by the following manœuvre. Embrace the upper fragment with one hand and the lower one with the other, and exercise a certain degree of pressure at the posterior part of the arm where the fracture is supposed to be situated. This projects the fragments forwards upon the palmar surface, and exhibits the characteristic plainly. The displacement from within outwards, noticed by Dupuytren, is proved by the experience of subsequent surgeons to be a far less frequent sign than he supposed it to be.

It is important to remember that fractures of the radius consolidate very rapidly, *i. e.*, in from 20 to 25 days. At this period the apparatus should be removed and the joint exercised; for, if this be too long neglected, adhesions of some of the numerous tendons and ligaments of the part may ensue, producing a degree of stiffness or semi-anchylosis of the joint—a matter of especial consequence to a working man.—*Gazette des Hôpitaux*, Nos. 10 et 26.

FRACTURE OF THE LOWER END OF THE FIBULA.

M. Robert recommends the following means of distinguishing this accident from a *sprain of the ankle-joint*, after waiting two or three days for the swelling to subside, if necessary. Apply one thumb upon the external malleolus, and the other over the supposed fracture, and transform the fibula into a lever, having its fulcrum at the inferior perineo-tibial articulation. A certain degree of pressure is to be exerted by the thumb on the malleolus externus. If the fibula is uninjured, its entire length is felt to slightly and uniformly bend under the pressure; but if there be a fracture, the lower fragment moves more or less, and projects under the finger, so that even its form may be distinguished. The fracture in these cases is almost always oblique from above downwards and from behind forwards, occurring at only a short distance from the ankle-joint.

M. R. observes that the fracture may thus always easily be detected, and in

surprised the mode has not occurred to others. Dupuytren was not aware of it, but used to seize the leg with one hand and the foot with the other, which means will however only suffice to detect a fracture when situated at a considerable distance from the malleolus.—*Gazette des Hôpitaux*, No. 35.

STRABISMUS CURED BY AN ACCIDENTAL DISPLACEMENT OF THE PUPIL. ANÆSTHESIA OF THE IRIS AND MYOPIA OBSERVED IN THE SAME PATIENT. By Dr. TAVIGNOT.

The patient, a young girl, aged 17, and of weak constitution, was attacked by measles in 1842. From this period, the eyes, which had heretofore been healthy, became affected with strumous ophthalmia. The left one was much the worst of the two, and after 18 months of unavailing treatment, Dr. T. was consulted respecting it. He found perforation had taken place at the upper and inner part of the cornea, the iris having protruded through the corneal aperture and contracted adhesions to it. Diligent treatment by small but frequent local bleedings, purgatives, and emmenagogues, and by a great variety of local applications, eventually triumphed over the obstinacy of the disease, and restored to the patient sufficient vision for the ordinary occupations of her sex.

From her infancy she had been the subject of a double convergent strabismus, that on the right side being much the most considerable. Had her eyes not become inflamed, she would have been operated upon to relieve this. Upon examining them, however, more than fifteen months after the cure of the ophthalmia, not the slightest trace of the strabismus could be discovered. The cornea of the left eye was quite transparent, except at the point where it was perforated, where there was a semi-transparent leucoma, behind which the dark, adherent iris could be seen. The pupil was elliptical and directed from above below, and from within outwards. It acted in a normal manner, except that under a very strong light it became altogether closed. She could read small print with this eye at more than eighteen inches distant, as well as when brought nearer to her. Somewhat distant objects were, however, only confusedly seen.

The appearance of the right eye was natural, and the action of the pupil normal, when the two eyes were examined simultaneously. If the left eye were closed, we observed the right pupil, after oscillating for some time, gradually enlarge and then remain motionless in a considerable, but not excessive, state of dilatation. This state continued, however bright the light, provided the left eye was kept closed, and the cornea not irritated by frictions: but energetic contraction took place the instant that light was again admitted to the left eye. When both eyes were open, it was scarcely possible to decide what part either took in the exercise of vision, for then she was enabled to read fluently, and was neither myopic nor presbyopic: but when she endeavoured to read with the left eye closed, she was obliged to bring the book close to her face, and even then could not see distinctly. Believing the myopia to depend upon the dilated state of the pupil, M. T. desired her to read through a small aperture made in a card. Vision was more clear and distinct, and the myopia less distinct; but still there was a difficulty which did not exist on the opposite side.

"In this case the displacement of the pupil, upwards and inwards, has changed the relations which heretofore existed between the axes of the two eyes. The left eye, in order to direct the new pupil towards objects, is carried a little outwardly, and that the more so as being the best eye, the patient employs it the more willingly. This external deviation has produced a kind of *divergent* strabismus, balancing the convergent strabismus of the right eye, and in this way re-establishing the parallelism of the two ocular axes. We do not believe the myopia depends upon any particular condition of the humours of the eye or

convexity of the cornea, but upon a diminished sensibility of the retina, (for the perforated card does not completely remedy it,) and upon a preternatural dilatation of the pupil, as the patient can read better through the card than by the unassisted eye. But we still have to inquire into the cause of this dilatation. It is evident that, for the contraction of its pupil, the right eye requires the assistance of the left, and the optic nerve, or the fifth pair, must therefore be the agent of this reflex action. If it is the optic nerve of the left side which transmits the impression to the third pair of the right, which induces the contraction of the pupil, we may inquire why the optic nerve on the right side is incompetent to this, (seeing that the state of the retina proves it is not paralysed,) when in all ordinary cases one eye suffices to induce the contraction. If, on the other hand, we suppose that a kind of local paralysis exists in the sensitive ciliary nerves of the right side, we can explain the necessity of the intervention of the ciliary portion of the fifth pair of the opposite side. It is true that, in this hypothesis, we admit that the stimulus of light acts upon the fifth pair, and not upon the optic nerve, at least as far as the contraction of the iris is concerned, and that it is the fifth pair which transmits to the third the contractile movement which takes place. We consider this opinion very near the truth, and have elsewhere (*l'Expérience*, 1844) exhibited the arguments which may be urged in its favour. In this case, in fact, the patient was the subject of a severe and obstinate ophthalmia for 18 months, and it is easy to imagine that the ciliary nerves spread among the affected tissues may have participated in the pathological condition, and their vitality have become consequently modified. Besides this, there existed great pain of the eyes and photophobia, so that, during the long duration of the disease, a neuralgic condition of these nerves, ending in paralysis, may have easily been produced.—*Annales d'Oculastique*, t. XV., p. 27.

MIDWIFERY STATISTICS.

The Reviewer in the March number of the *Archives Générales* gives the following general results of *Midwifery Statistical Tables*, recently published in the Italian and English journals. In 47,116 labours, twins occurred 446 times (9 $\frac{4}{10}$ per thousand,) and triplets four times (1 in 10,000.) There were 40,233 *head presentations* (969 per 1000,) of which 40,046 were *vertex*, and 187 *face*. There were 1065 *breech* or *footling* presentations (27 per 1000,) and 154 *transverse* ones (4 per 1000.) Of these labours, 46,632 terminated *naturally* (989 per 1000,) and 484 (11 per 1000) *artificially*, viz., 321 by means of the forceps, 89 by craniotomy, 54 by turning, and 20 by vaginal or uterine hysterotomy.

CASE OF PELVIC ABSCESS OPENED THROUGH THE RECTUM. By M. AMUSSAT. Related by Dr. COMPÉRAT.

The subject of the case was a lady, 37 years of age, who had borne two children. She had enjoyed excellent health until 10 years since, when her husband infected her with gonorrhœa, from the secondary effects of which she suffered and recovered; a severe lumbar pain, however, then making its appearance, and continuing to annoy her at intervals. During the last 18 months this pain had increased in severity, and when first seen, 4th April, 1844, by Dr. Compérat, it had become very intense, occupying all the right side of the abdomen and genital organs. We need not pursue the detail of her sufferings, and of the measures which partially relieved them; but may observe that, by a recto-vaginal examination, a tumefaction was felt on the posterior part of the right side of the body

of the uterus, seeming to be seated exactly at the recto-vaginal partition. Passing the index-finger high up into the rectum, its pulp was just able to reach the projecting part of another large swelling at the right posterior portion of the gut. All the parts were hot and irritable, and the examination gave excruciating pain. MM. Amussat and Fouquier saw the case, and believed it to be one of abscess, which the great constitutional irritation would seem to indicate, although they could not detect fluctuation, which Dr. C. believed he had felt. As the point of the finger could only just reach the swelling, the surgeons wished to defer opening it, and ordered cataplasms, the simultaneous injection of the rectum and vagina with emollients, and other palliatives. On the 19th, fluctuation had become distinct, and as the patient's general condition was now truly alarming, they determined to operate. M. Amussat having passed his finger into the rectum as high as the fluctuating tumour, slid along its palmar aspect a pair of very sharp-pointed scissors, very like those contained in dissecting cases, but having the backs of the blades rounded and the branches very much longer. The puncture was made and the blades then separated, so as to enlarge the aperture by tearing rather than cutting. During this period, an assistant pressed upon the belly, so as to project the tumour as far towards the rectum as possible. A lithotrity catheter was now substituted for the scissors, and guided along the finger, still left in the rectum. A considerable quantity of healthy, and scarcely sanguinolent pus flowed out. The cavity of the abscess was carefully injected with tepid water, and the rectum cleaned out, and the access of air prevented by ascending douches of the same. These last gave great relief. In order to keep the aperture of the abscess patent, the index-finger was introduced several times during the first day. The next day, however, it had nearly closed, and the finger could not pass through the thickness of the walls of the abscess, which M. Amussat had observed seemed like cutting into the substance of the uterus itself. He enlarged the opening by means of a longer and stronger pair of scissors, the blades of which were notched near their extremities, so as to give the instrument a lance form, and prevent its escaping during the attempt at enlarging the wound. The edges were kept separate by occasionally expanding them by means of a *brise-pierre*. The case eventually did very well.

Dr. Compérat observes that, although numerous cases of abscess forming in the cavity of the pelvis are on record, (M. Bourdon gives an account of 23 of these in his paper "on fluctuating abscesses of the pelvis, and their discharge by an aperture practised in the vagina," in the *Rev. Médicale*, 1841,) yet the present, as far as he knows, is the first case in which an opening has been made for the evacuation of the pus by the rectum. The surgical procedure in each case must depend upon the direction which the projecting portion of the swelling takes. Of course, the difficulties to be surmounted are less in the vaginal incision. One of these, the danger of *hæmorrhage*, was met in this case by using very pointed scissors first as a trocar, and then tearing, rather than cutting, the parts with them. The edges of the aperture also were effectually dilated by means of the *brise-pierre* as long as requisite. The introduction of instruments, however, at last induced severe *irritability of the anus*, which continued for hours, and caused the patient to dread its repetition beyond everything. M. *Recamier*, who saw the case, promised her, if she could bear very severe pain for a short period, to effectually relieve her.

"Having well oiled his fingers and thumb he formed them into the shape of a cone, and thrust them altogether through the anal ring, impressing upon the entire hand slight movements of supination and pronation in order to facilitate its introduction. The margin passed, the fingers were bent so as to increase the size of the imprisoned mass, and then drawing them out at once in this position, he forcibly dilated the sphincter, thereby causing excessive pain for some minutes. The anus spasmodically contracted during several hours, and then all became quiet. From this period, the sphincter offered no obstacle to the intro-

duction of the finger and *brise-pierre*, and the patient suffered no pain whatever from it. I do not know whether M. R. has published his ingenious expedient; but all I can say is that, from the surprising manner in which it relieved the excessive sensibility of the anus and the anguish of the patient, it cannot be too strongly recommended in all cases where the permanent or frequent introduction of instruments irritates the sphincter."—*Revue Médicale*, t. I., 193.

ON THE SURGICAL TREATMENT OF CROUP. By M. GUERSANT, Jun.

I agree in the opinion of MM. Bretonneau, Trousseau, Guersant, Sen., Blache, &c., that to constitute croup the presence of false membranes in the larynx is essential. The disease may commence at the tonsils, in the bronchi, or suddenly in the larynx itself. In the first case there is more or less redness of the pharynx with swelling of the tonsils, and, what is of vast importance to notice, these last are covered with little white patches, which sometimes extend as far as the velum or uvula.

The *medical* means for treating croup are very limited. *Depletion*, once so freely employed, under the idea that the disease was a simple inflammation, is very rarely of any utility in croup, and oftentimes injurious. *Emetics* have proved far more useful as adjuvants, by favouring the detachment and expulsion of the false membranes; but alone they are not to be relied upon. *Mercurials*, especially when used early, have often exerted excellent effects upon the disease; but for these to be of service the dyspnœa must not be very urgent, or the patient very enfeebled, and when used alone they have effected but few cures.

It is upon *surgical* treatment we must usually most rely, and by it we mean the *application of caustics to the pharynx*, as well as the operation of tracheotomy. Various fluid or solid substances of this nature have been employed, but we prefer the *nitrate of silver*. Weak solutions suffice at the earliest stage of the disease, when there is little else than the pseudo-membranous deposit upon the pharynx. There are indeed cases in which these are not seen at all, the false membranes being at once deposited in the larynx, but these are rare; and frequently the membranes are not detected in the pharynx, because the first period of the disease has already passed away. At an early period the symptoms are but little urgent, and the physician, little accustomed to treat children, often neglects to examine the throat. With M. G., whenever a child manifests any febrile re-action, *such examination is an invariable rule*, and in this way he has frequently been enabled to detect the approaching disease, which would not otherwise have been suspected. He instances a case in which M. Trousseau was induced fortunately to examine the throat by observing the surface of a blister to be covered with a slight fibrinous layer—such false membranes being liable to form on the surface of any wound in those who are the subjects of diphtheritis. At first, and while the tonsils are covered with this plastic exsudation, the symptoms are not severe, so that attention is not directed to the throat. But this is a precious moment for the surgeon: for he may now frequently arrest a disease, which if allowed to go on is usually beyond his art.

While employing the solid caustic, the child should be held by a strong assistant; for it is rare that the practitioner can effect this operation unaided. The tongue must be held down by a very *large spatula*, or by the handle of a *large* spoon. If a small instrument be used you cannot effect your object. We generally employ a large wooden tongue-depressor. The caustic should, for fear of accidents, only project very slightly from its case; and many practitioners, on this account, prefer using solutions. We have already said that, at the earliest stage, the use of even a weak solution three times a day suffices. We should apply the caustic beyond the margin of the false membrane as well as to itself, as this

will prevent its extension. In serious cases the solution must be very strong (1 part to 3 or 4 water,) but then need only be used once daily. It may be applied by means of sponge fixed at the end of a piece of whalebone by sealing-wax. The caustic frequently dissipates the false membranes upon the amygdale, and yet they extend to the epiglottis. Caustic is still our best means. A larger sponge is now required, which must be fixed upon a strong whalebone, bent at an obtuse angle. The surgeon places himself on one side, and introducing the sponge right to the base of the tongue, executes some semi-rotatory movements. Sometimes the epiglottis is raised, and the fragments of false membranes detached from its inferior surface, which may be known by the paroxysm of dyspnœa this gives rise to. The caustic requires to be repeated three or four times in the twenty-four hours.

When, in spite of the energetic use of these means, success does not follow, we must have recourse to *tracheotomy*. M. Guersant has, next to M. Trousseau, performed this operation more frequently than any one else, and speaks unhesitatingly of the propriety of undertaking it, and believes that numerous failures arise from its being too long deferred. When the child will certainly die asphyxiated without, why should we hesitate to open the trachea? for cases have occurred in which the patient has lived, although false membranes have penetrated *even into the larger bronchi*. The vital point which cannot bear the slightest presence of these is the *cordæ vocales*. Practitioners who are aware that a certain number of these cases have proved successful cannot doubt the propriety of operating. M. G. usually employs a straight bistoury, and has several small sponges mounted on whalebone and a curved ring-forceps at hand. If the morbid productions do not reach into the trachea we have only to maintain the aperture patent; while, when they extend lower down, their removal may be attempted by means of the bent forceps. Among the numerous modifications of canula employed to maintain the wound open, that of M. Trousseau is an excellent one. It consists of a *double canula*, so that, when obstruction occurs, the *inner one* may be changed without disturbing that which remains in the wound. This practitioner, as well as M. Bretonneau, prior to introducing the canula, passes small sponges, moistened with a solution of nitrate of silver, into the trachea: but unless false membranes are obviously present, M. G. doubts the propriety of any such interference, and does not have recourse to it. The canula may usually be removed at the end of from eight to twelve days, but sometimes requires to be retained for 20 or 30.

The air of the chamber should not be kept too dry and hot. To render it sufficiently humid it is a good practice to evaporate some emollient decoctions in the room several times a day. It is a difficult thing to maintain an equable temperature about the child, but for a long period I have experienced the utility of wrapping around the neck, without tightening it, a light woollen comforter having its meshes very widely knitted. By this contrivance, the air, before it reaches the trachea, becomes sufficiently warmed. When the canula becomes obstructed, the inner canula should be removed and cleansed, instead of thrusting sponges into it, which may only increase the obstruction. When it is deemed proper to cleanse out the trachea, only the most delicate whalebones must be employed. When indurated concretions form, both canulæ should be removed and the patient encouraged to expel them by coughing. I have never removed the canula before the tenth day, but M. Trousseau has done so on the third or fourth. He advises us not to remove it suddenly, but for one, and then for several hours daily.

Finally, let us observe that croup is so grave and so constantly mortal a disease, that we should frequently have recourse to this operation before it reaches its last stage. "While tracheotomy was almost always a powerless weapon in my hands," says M. Trousseau, "I always recommended its performance as late as possible; but now I have met with numerous instances of success, I always

say it should be performed as early as possible, as soon in fact as no other chance of success remains." Of 136 children operated upon, M. T. has saved the lives of 32; and, without being so fortunate as that practitioner, in 36 cases I have met with four successful ones—a success sufficiently great for us to lay it down as a law that we should interfere rather than allow the infant inevitably to die.—*Gazette des Hôpitaux*, Nos. 48 and 52.

(The practice of promptly examining and cauterizing the pharynx may doubtless prevent the extension of the disease in some cases, and is not enough attended to in this country. So, also, an unreasonable prejudice against *Tracheotomy* prevails among us. Death stares us in the face, and why not resort to means which has rescued many lives, albeit these may be few compared to the numbers operated upon—many of whom were in *extremis*. Dr. G. does not notice the external application of heat (to rubefaction) so useful at the outset of croup.—*Rev.*)

PHOSPHATE OF AMMONIA IN GOUT AND RHEUMATISM. By Dr. BUCKLER, Baltimore.

Dr. Buckler thus states the grounds upon which he recommends this substance to notice:—

"Taking into account these two prominent facts above stated, namely, the excess of lithic acid found in the urine at the period of convalescence from an attack of acute gout or rheumatism, and the subsequent deposit of soda and lime in the white tissues, it occurred to me that, during the existence of these diseases, the lithic acid might exist in the blood in a state of combination with soda and lime, in the form of insoluble compounds, which the kidneys and skin refuse to eliminate. If then, any agent could be found capable of decomposing the lithates existing in the blood, and of forming in their stead two soluble salts, which would by voided by the kidneys and skin, we should thereby get rid of the excess of fibrin in the blood, the symptomatic fever and the gouty or rheumatic inflammation, wherever seated, which have been excited by the presence of these insoluble salts. It occurred to me that *phosphate of ammonia* might be the agent, provided it could be given in doses sufficient to answer the end without producing any unpleasant physiological symptoms. If our theory were true, phosphate of ammonia seemed to be the proper re-agent, for it would form, in place of the insoluble lithate of soda, two soluble salts—the phosphate of soda, which is remarkably soluble, and the lithate of ammonia, which is also soluble—and both capable of being readily passed by the skin and kidneys. The excess of uric acid would thus be got rid of in the form of lithate of ammonia; and the soda, floating in the round of the circulation (instead of being deposited, as it were, like an alluvial formation in the substance of the fibrous and cartilaginous tissues) would be taken up by the phosphoric acid, and eliminated from the circulation."

The particulars of thirteen cases of gout and acute rheumatism are briefly related, in which this medicine (*in doses of from ten to twelve grs. ter in water,*) seems to have wrought a speedy cure. Dr. B. has also found it an excellent remedy in old hospital chronic cases, which had resisted iodine and other medicines; and in those instances in which the deposits and deformities are such as to be irremediable, the acute attacks which supervene may be effectually relieved. "In reviewing the cases which I have published, it will be noticed that thickening of the white tissues, of long standing, has disappeared under the use of the phosphate. Now, it is in such cases that the lithic acid diathesis generally prevails, and this agent seems to act here by depriving the blood for a long time

of uric acid and soda, thus creating a demand for these elements in that fluid, and thereby bringing about an absorption, as it were, and solution of the superfluous lithate of soda which is deposited in the white tissues." In all the cases adduced, when little acid had been before observed in the urine, it disappeared under the use of the phosphate, while, instead of the lateritous deposit usually seen at the period of convalescence, this fluid continued limpid—the eliminated lithate of ammonia and phosphate of soda being soluble. The rapid disappearance of lithic acid from the urine in cases wherein this salt is administered, leads to the conclusion that it must prove an eligible remedy for *uric acid calculus*.

Dr. Buckler does not propose the salt as an exclusive and empirical remedy, but employs simultaneously, various means for combating serious symptoms, until the healthy condition of the blood is restored.—*American Journal Medical Sciences*, Jan.

ON THE SUBMUCOUS DIVISION OF THE SPHINCTER IN AFFECTIONS OF THE ANUS. By M. DEMARQUAY.

The division of the sphincter, as practised by Boyer, frequently fails, and is not devoid of danger; but all its advantages may be obtained and inconveniences obviated by operating upon it beneath the mucous membrane. MM. Guerin, Velpeau, Blandin, and the author, have employed this modification with great success, and it is the object of this paper to spread a wider knowledge of the proceeding by detailing several of the cases which have been treated by the two last.

Submucous myotomy is especially calculated for the relief of *spasmodic action and contraction of the sphincter ani*. It is often difficult to say where one of these affections ends and the other begins, for they are so intimately connected that the one may be said to be a consequence of the other: for if the same irritating causes which induce spasmodic contraction of the muscle continue in operation, this will then become permanent. When the contraction is well-marked the anus seems deeper seated than ordinary, the borders of the external sphincter being seen projecting and the plaits of the region well marked. If hæmorrhoids or mucous membrane has become engaged within the sphincter, the compression gives them a violet colour, and they may even become gangrenous. The finger is introduced with difficulty, and with pain to the patient, and is much constricted by the muscle.

Spasmodic contraction, requiring the operation, may manifest itself under various circumstances, but it is especially in its prevention of the *extraction of foreign bodies* from the rectum, that it comes under our notice. It may also manifest itself in two very important affections, viz., *prolapsus of the rectum* and the *sudden appearance of large hæmorrhoids*. In this last case, the constriction may be so considerable as to induce gangrene, or at all events prevent the return of the protruded parts.

Contraction of the sphincter plays an important part in three affections, viz., constipation, old hæmorrhoids, and fissure of the anus.

(1.) Prolonged and obstinate *constipation* not unfrequently gives rise to it. The indurated matters contained in the gut irritate it, and induce, first spasmodic and then more permanent contraction; and authors have certainly sometimes mistaken the effect for the cause, in regarding the constipation as produced by the contraction. In mentioning constipation as a cause of contraction, it is not intended to recommend combating it by a surgical operation. It is a complex condition of which this contraction only forms a portion.

(2.) When *hæmorrhoids* have long existed, and become frequently congested

and inflamed, it is by no means rare to find contraction induced. In this case they are forced out at stool, but can only be partially returned. They inflame; defæcation becomes more and more difficult, and fissures and hæmorrhages result. Several examples are given in which this state of things was effectually relieved by the submucous section.

(3.) *Fissure of the anus* is so intimately connected with contraction of the sphincter that Boyer regarded them as the same disease. But we have already seen contraction may exist without fissure, and the question is, when the two affections co-exist, which has preceded the other. In the great majority of cases a more or less obstinate constipation, a state often giving rise to contraction, has preceded the fissure. Authors, however, have stated that they have observed fissure without contraction, and if they are correct we may explain such cases by the varieties of the disease admitted by M. Blandin. He states that the fissure may be seated above, opposite, or below the sphincter. In the last case it may exist without contraction.

The author has analysed all the cases of fissure of the anus which have been published, as far as he knows, since the time of Boyer. They amount to 53, of several of which very insufficient particulars are given. Of these persons, 30 were men and 23 women, in opposition to Boyer's assertion that, females are most liable to the disease. In six instances the fissure was double, triple, or quadruple. The age is only recorded in 42 cases, the disease being most common between 20 and 30, and 30 and 40. In 26 of the cases constipation, and in six cases hæmorrhoids, are described as existing.

Seven cases of fissure operated upon by M. Blandin are given, in all of which complete and immediate relief succeeded to the most intense suffering. Prior to commencing the operation the rectum should be well cleared out, so as to obviate the necessity of going to stool shortly after it. An assistant must raise the side of the buttocks opposite to that on which the section is about to be made. This last is always practised at one of the sides of the anus, so that the sphincter may be divided through its middle. An ordinary *tenotome* may be used, or a bistoury which M. Blandin has contrived. The former is however not long enough, and its point is not sufficiently guarded, so that there is danger of penetrating the mucous membrane with it. M. Blandin's bistoury is protected by a moveable sheath, and he can either use it for puncture or incision, or as a flattened probe, accordingly as he wishes to divide parts, or merely to pass it between the muscle and mucous membrane. Whichever may be selected, the operation is simple. 1. Make a small opening in the skin. 2. Introduce the finger into the rectum, at the same time that the skin on each side of the anus is stretched. 3. Pass the tenotome between the mucous membrane and the sphincter. 4. Divide the latter. The puncture of the skin is most conveniently made at from 8 to 12 lines from the anus. The instrument must be passed in very gently so as to detach parts as little as possible. When the division has been made a kind of crack is heard, and the finger which is in the rectum feels the space between the two divided portions of the muscle. Although not absolutely necessary, the patient had better keep his bed for a few days after; and he should live very abstemiously, as it is desirable he should not go to stool for three or four days, after which there is no fear of rupture of the cicatrix. Occasionally the section of the sphincter on one side only is insufficient. The fissure does not after the section of the muscle require any special treatment.—*Archives Générales*, April.

(When the dreadful and prolonged suffering incident to fissure of the anus is considered, the substitution of this simple operation for that of Boyer's must be regarded as of one of the most valuable applications of myotomy.—*Rev.*)

QUININE IN ACUTE RHEUMATISM.

Three years have elapsed since M. *Briquet* communicated the success which attended the use of quinine in cases of acute articular rheumatism at the Hôpital Cochin. Several practitioners have resorted to it with the same result; but its employment has become by no means general, partly in consequence of too large doses being given at first, and various accidents in consequence ensuing. M. Briquet formerly gave from 4 to 6 scruples in the 24 hours, but now gives but from 1 to 4, discontinuing it when any sign of prostration manifests itself. He employs the neutral salt rendered soluble by sulphuric acid. From the first or second night sleeplessness disappears, and a little later there is a more or less marked diminution of the pain and swelling of the joints. From the third to the sixth day the rheumatism may become cured; but when the cure is so prompt as this there is usually some return of pain, with or without swelling, again requiring the use of quinine. As a general rule the patients are cured or notably relieved from the ninth to the twelfth day of treatment.—*Gazette Médico-Chirurgicale*, No. 17.

CONTAGIOUSNESS OF PUERPERAL FEVER.

Dr. *Kneeland* has contributed an interesting paper upon this subject to the *American Journal of Medical Sciences*. He does not bring forward any new cases in elucidation of the question; but, from a careful examination of facts and authorities on both sides of the Atlantic, arrives at the following conclusions:—

"1. From the confinement of cases to the practice of single physicians and nurses in populous cities; from the fatal results attending post-mortem examinations; from its ravages in hospitals; that puerperal fever is contagious; that it may have other modes of propagation in certain states of the atmosphere, and among strongly predisposed individuals; but that the fact of its conveyance by practitioners attests its contagiousness. 2. That it may be propagated by direct inoculation with the fluids of the living and dead; by the effluvia arising from the bodies of the sick, inhaled in the very chamber of death (as in the wards of a hospital) or carried about by the person of the physician; by clothes, bedding, which have been in contact with a diseased individual. 3. That the order of propagation from the physician to the patient, and the regular succession of cases, show that the epidemics of puerperal fever are, in almost all cases, the *effects* and not the *causes* of contagion. 4. The contagion acts according to the frequency of communication between the physician and nurse (in whose practice are cases) and lying-in-women, independently of insalubrity of places, wretchedness of patients, or the neighbourhood of dwellings—for, although poverty and misery seem to predispose to it, communication is none the less fatal to the higher classes. 5. A case, to all appearance sporadic, may communicate the disease; a mild case may communicate a severe disease, and *vice versâ*. 6. Immunity proves nothing against contagion; it may be the effect of an acquired or temporary inaptitude. It is equally inexplicable in all contagious diseases. 7. The rapidity of its propagation shows that it is contagious at the commencement; the fatal results of attending autopsies indicate this character after death. 8. That a physician should not make or be present at an autopsy of this disease; or if he does should take proper measures to cleanse himself and dress, for the safety of his next patient—that if a case (or several cases) occur in his practice, he should consider himself, in the language of Dr. Holmes, a 'private pestilence,' and regulate his conduct accordingly—that persons who have washed, or have otherwise handled the clothes or bedding soiled by the discharges of this disease,

should not approach, much less nurse, a woman after delivery. 9. That when the disease is prevalent, a prompt removal from possible intercourse with a 'pestilential' physician, and a strict attention to ventilation, cleanliness, quiet, proper food, &c., are the dictates of a reasonable fear."

(So overwhelming is the testimony in favour of the contagious character of this disease, and so truly frightful have been the consequences which have frequently resulted from neglecting the precautions this should have furnished, that we think a medical man persisting in attending successive cases of midwifery after a well-marked instance has broken out in his practice, should be made amenable to the criminal tribunals of his country. A woman has confided her life and safety to his skill and honour, and he enters her chamber, and approaches her person, the bearer of what he knows is reputed to be by some of the wisest of his profession a deadly poison, the escaping from the effects of which she is to owe to her own idiosyncracy and not to his humanity. Medical men are doubtless unconsciously the propagators of other diseases; but their instrumentality is here so well marked, and has been so often exemplified, and the consequences are so commonly fatal, that every one possessing a spark of conscience should at once resolve to relieve himself from this dreadful responsibility, at whatever sacrifice, while such as choose to persist in so criminal a course should not be allowed to do so with impunity. This is not one of the hundreds of instances of difference of opinion in the profession where each person is as likely to be as right in the long run as the other. The practical results are fearful, and it is of the last importance for the honour of our profession that we do not convert our blessed ministration into a curse and a scourge. It is to be regretted that an authoritative and combined declaration of opinion has not been expressed upon the subject, and a rule of proceeding laid down; for we every now and then hear of men consulting their brethren and temporarily abandoning their practice only *after* they have lost several patients in succession; and Dr. Jackson of Philadelphia, our author states in his paper, *began* to be "seriously alarmed on the score of contagion," when he had had seven cases of puerperal fever in uninterrupted and rapid succession (five of which were fatal,) and women whom he had been engaged to attend fled from him as from a pest. Mere ablution or change of dress will not suffice. Midwifery should be entirely abandoned for several months. The sacrifice would sometimes be great, but it would not be without its compensation.—*Rev.*)

ON THE QUESTION OF THE REMOVAL OF CANCEROUS BREASTS. By M. VELPEAU.

The patient before us is one upon whom we cannot operate, for, on examining the breast, we find little knots extending even to the edge of the great pectoral; moreover the nipple is implicated, and it is an instance of disseminated cancer, which for us is a true *noli me tangere*. We do not mean to say that the wound would not cicatrize and the patient recover of the operation itself, but we have acquired the desolating conviction that this species of cancer, as well as the ligneous scirrhous, invariably relapses.

Many descriptions of cancer should not be removed, and this is a very important point, as many practitioners, unaware of it, injudiciously undertake an operation. Among the varieties liable to return, all do not re-appear however with the same tenacity. Among those whose return is certain may be named that form in which the skin becomes horny or leathery, and in which the indurated integuments and the nipple are retracted; that ligneous variety in which the indurated parts are formed into layers like bark; and that in which the dis-

ease does not commence with one tubercle under the skin but with several at once, in which case we may be certain that there are others there also. In all these, relapse is certain. In this last-named variety we need not be surprised at a return, for how are we to be certain of the removal of cancerous grains so small as to elude sight and touch.

Melenic cancer is rare in the breast, or co-exists in other parts of the body. Nevertheless, if a simple tumour of this kind appears in this part, it should be removed, as it is not always reproduced. *Colloid* cancer may be placed in nearly the same category. *Scirrhus* also admits of operation, except when it is branching, *i. e*, sends out roots in different directions, whence its name *cancer*. In this case it is usually reproduced, its distinct limits being ascertained with difficulty. *Encephaloid* is a bad species, because it is usually simultaneously developed in one or several other organs. If, as far as we can ascertain, no other deposition exists, we have a chance of success by operation. We have cured some fifteen out of those we have operated upon; but when we recollect that these tumours may become developed in some internal organ without any symptom thereof, we can see how uncertain any prognosis must be. There are some curious facts connected with this subject. I recollect a man of robust habit and apparently in vigorous health, from whose lip I removed an encephaloid tumour. He died in four days, and at the autopsy we found thousands of little encephaloid tumours in the liver. Are we to suppose these became developed in so short a period after the operation? If we believe that they existed simultaneously with the disease of the lip, how do we explain the excellent health of a person having so large a quantity of cancerous tumours in his liver?

It will be seen that cancerous tumours in general, and those of the breast in particular, demand classification and distinction from each other; and until classification is perfectly established, and the same one employed by all practitioners, they will continue to misunderstand each other.—*Gazette des Hôpitaux*, No. 55.

This excellent surgeon published some interesting observations in the same journal last year upon *Neuralgia of the Breast*, so frequently mistaken by women for cancer of the organ. If the surgeon is not on his guard when he examines the breast, he observes, he may confirm instead of dissipating the error. The organ is composed of numerous lobules which are far from always offering the same consistence. In different women, too, there is great variety in the configuration of the thorax as regards its flatness or projection, which imparts a different sensation to the hand engaged in examining the mamma. Again, if we take hold of the breast at the side as if to compress it, and then feel it with the other hand, it will be difficult to persuade oneself it does not contain a tumour: and without having made the experiment the different density of the organ, according as it is pressed in one direction or another, cannot be conceived. If to this we add that the gland is more or less developed, and the breast more or less resisting, more or less pendent, in different persons, we can see there are many shades of difference met with.

Women who suffer severe pain always believe that cancer exists, and have submitted themselves unnecessarily to amputations and severe treatment. Attacked by a species of hypochondriasm, they are annoyed if you doubt the importance of their malady; and although you may deny that they suffer from the disease they suppose, it would be unjust and inhuman to forget they do suffer much. The pains felt are sometimes only the result of the attention being unduly directed to unpleasant sensations and the mind dwelling upon them; but at others result from a true neuralgic affection of the breast. It is of course important to distinguish this from cancer, and we must remember that in general true tumours do not cause pain at their commencement, and that, when the patient has long complained of violent pain without any tumour being apparent, there is no tumour at all in the case. In examining the breast we must never take hold of

it by its side, but on the contrary expand it by means of the entire hand; and take every opportunity of comparing the sensations derived from the lobules of this organ with those produced by pathological tumours. When, after believing we feel a tumour by pressing it laterally, we are unable to do so by pressing from before backwards, none in fact exists.

The means of treating this affection consists in part in modifying the form of the stays worn by the woman. The breast must be supported. As the fashion now leads to the breasts being widely separated from each other, the inner side often suffers pain from the traction exercised. The stays should be so made as to support the breasts towards the median line, bringing them nearer to each other, and carefully avoiding compressing them. This precaution alone often suffices, but we may also employ various liniments; and give iron, bismuth, valerian, &c., internally, according to the condition of the patient. It is not, however, in women of high life alone that we observe this affection, but examples are often met with among those of the lower classes.

GENERAL HISTORY OF THE SEVEN DIATHESES. By Professor GAILLARD.

By *diathesis* we understand a pathological modification which exhibits itself in affections variable as to their seat, but identical in their nature. It is a change in the organs and functions common to the entire economy; and when the morbid state is not so general as this, it affects at least some of the elements which enter into the composition of many organs. Thus rheumatism is seated in the fibrous tissue, and the herpetic diathesis occupies the skin and mucous membranes. Affections engendered by this cause put on special forms in relation to the diathesis whence they have arisen. Thus common ophthalmia is distinguished from syphilitic, and this from herpetic and scrofulous ophthalmia.

A diathesis is more than a mere simple *disposition to a disease*, and a person under its influence may be in a very serious condition long before any local symptom manifests itself. A person from whom a scirrhous tumour has been removed may remain for a long period without a relapse, and yet the cancerous matter eventually become deposited in some other part. It as it were hesitates, and if some external agent produces any local stimulation, it attacks this irritated part by preference.

Many persons look upon every disease as a mere *local lesion*, as bronchitis, arthritis, gastritis, &c., to which all medical appliances should be addressed; but the situation and extent of the anatomical lesion should not attract our attention so much as the *general cause*, which may give rise to a similar lesion in some other part of the economy. In this way a sprain complicated with scrofula may be allowed to degenerate into a white-swelling, or slight gastric irritation, under the rheumatic diathesis, may become developed into an obstinate gastralgia.

The diatheses constitute the *humoral vitiations* of the ancients, but they are not *cachexiæ*. A cachexia is a general condition of decay, a bad state of the general health usually consecutive to some serious or organic change. Certain diatheses may give rise to cachexia, but this term could not in general be applied to a gouty or rheumatic subject.

We must also distinguish a diathesis from the susceptibility to morbid causes, the *vulnerability*, which certain organs or apparatus may furnish. The vitality of the different organs may vary either above or below the normal mean, and the susceptibility in either case be increased. Such vulnerability may depend upon (1) congenital disposition: (2) hygeienic influences: (3) certain endemic or epidemic medical constitutions: (3) antecedent disease.

Considered in a general point of view the *diatheses offer these characters in*

common. 1. They act on several organs or regions simultaneously or successively. 2. They remain latent for a certain period, and then suddenly manifest themselves by various local lesions. 3. They perpetuate acute affections, interrupt their natural course, prevent the resolution of the disease, and lead to a chronic condition. 4. They obstinately persist, unless attacked by general measures. 5. They give rise to frequent relapses.

There are certain organs and tissues which each *diathesis especially* affects: thus rheumatism affects the fibrous tissue, syphilis the skin and mucous orifices, &c. But as these anatomical elements are common to many regions, the distinctive character of the diathesis is still maintained. That is, it produces at distant points local manifestations which are so identical that they alternate with, succeed, or displace each other, furnish analogous physical and physiological signs, and are modified by the same means of treatment.

Some diatheses are *fixed or adherent* to the organs which they attack. They may be met with at several points, but they leave with difficulty organs they have once attacked. This is the case with scrofulous, cancerous, and syphilitic affections, while others are *mobile*, and change their site with singular rapidity. This is the case with the rheumatic and gouty, and, in a less degree, the herpetic diathesis.

These affections are not all produced by the same *causes*. Thus, scrofula and scorbutus arise from the exertion of a profound and lengthened modification of the solids and fluids of the economy by external causes. Syphilis and cancer are produced by a poisonous principle, a material and organic cause, circulating in the blood. Rheumatism, gout and herpes may also be explained upon this last principle. Diatheses are often *hereditary*, *i. e.*, the new-born child possesses a particular vulnerability in certain systems, and an especial susceptibility in regard to certain modifying agents of the economy.

The *prognosis* of diatheses varies according to the nature of the diathesis, the intensity, seat, and duration of the local disease and the social position of the patient. This last is of great practical importance; for when the condition of life of the patient places at his disposal the resources of hygeiene and therapeutics, and his habits are temperate, the physician is much more assured of the results. He may then attack the most obstinate disease, and impress modifications upon the economy which meet it at its point of departure. Still, it must be allowed, that the favoured classes of society are more delicate and susceptible, and less amenable to the employment of medicines. Chronicity and obstinacy are characteristics of the diathesis. The general condition is modified with difficulty. The means employed are tedious and insensible in their action. A diathesis is slow in forming and in disappearing.

Diatheses are often *complicated with each other*. A patient, suffering from the herpetic diathesis may contract syphilis and rheumatism, and manifest symptoms characteristic of these different causes. In general the phenomena are characteristic, but sometimes the diagnosis is rendered difficult. Rheumatic and syphilitic pains may be confounded. Herpes of the prepuce has been mistaken for chancre. M. G. believes, with many other authors, that rheumatism may supplant syphilis, and that it is an error to treat all articular swellings and pains which follow blenorrhagia as venereal.

The *treatment* must, above all things, be general, and adapted to the nature of the diathesis. It comprehends—1. Special modifiers, or to speak more exactly, such as are oftener employed with usefulness and efficacy in each diathesis. 2. Evacuants, as purgatives, sudorifics, blisters, issues, &c. These last seem to replace the pathological local fluxion induced by the diathesis, and constitute, as it were, a supplementary function. 3. Narcotics which modify the nervous system and vitality of organs. 4. Hygeienic modifiers. These are the most efficacious. With them we may frequently do without medicines, but nothing will replace them.

The diatheses are *seven* in number, viz., the *scrofulous*, *scorbutic*, *herpetic*, *syphilitic*, *rheumatic*, *gouty*, and the *cancerous*. Each of these, to be properly treated, would require a special article, but we can only notice one of the examples adduced by the author. A *simple sprain* is always cured quickly or slowly according to the prudence of the patient, but still it always is cured; and it is only under the influence of a diathesis it becomes a grave disease. "In the majority of instances," says Lisfranc, "the external violence is only a determining cause which fixes in the articulation a morbid principle existing in the economy."

The *diagnosis* of diathesic diseases is established by several circumstances. 1. The disease is chronic and obstinately persisting. 2. The patient has already been the subject of rheumatic pains, herpetic eruptions, glandular swellings, or some other affections which announce the existence of a diathesis. 3. In the origin of the actual disease, the patient has been exposed to circumstances which radically modify the economy. Thus the prolonged action of cold and damp engenders rheumatism. 4. The local manifestations offer special characters which indicate the generative cause. 5. In cases of doubt we test the disease by special medications, and the advantageous or injurious results which ensue throw light upon its nature. A young lady, the subject of sub-cutaneous swellings and gnawing ulcers, which offered many of the characters of syphilis, was treated with Dupuytren's pills without advantage, but recovered shortly under the influence of bark, as the lesion was caused by a scrofulous diathesis. 6. If the antecedents enlighten us upon the existence of a diathesis, consecutive phenomena often demonstrate the reality of certain causes, which had been either only suspected or even misunderstood. A person labouring under gastralgia is seized with a pain in his shoulder, and the affection of the stomach ceases. A dry and irritating cough may disappear as soon as an eczema of the thigh manifests itself. Sometimes these diathesic symptoms appear several years after the cure of the disease, and explain to us its capricious progress, its unusual characters, and its obstinacy.

In fine, many, or rather the great majority of, chronic diseases are maintained by general causes, and it is to these we should address our principal medical appliances.

NEW MEANS OF DISINFECTING DISSECTING ROOMS.

M. Sucquet has presented the Academy of Sciences with an account of his new mode of impeding the putrefaction of subjects used for dissection. He observes, the means which at present imperfectly accomplish that object, as arsenic, corrosive sublimate, &c., either injure the edges of cutting instruments, or prove detrimental to the health of those who use them. He employs two substances, namely, a solution of *sulphite of soda* and one of *chloride* (*chlorure*) *of zinc*. The first of these is injected into any large artery, returns by the veins, and even fills the lymphatics. It exerts no chemical effect upon the blood, but effectually cleanses out the minutest capillaries, so that other injections may be used afterwards. The volume, consistence, colour, &c., of parts are all preserved, and remain so, if the integument be undisturbed, so that air does not obtain access, for a month as an average period, but something even for 40 or 50 days, and that in a moist atmosphere at a temperature of 54°. The sulphite absorbs the oxygen given off by the tissues, and prevents it taking part in the putrefactive process.

This substance, however, does not protect from putrefaction for more than 20 days, when the integument is destroyed and the tissues exposed to the air; but the parts may be preserved for ever free from putrefaction by the use of the *chloride of zinc*. Parts required for further dissection should be immersed in it:

and all those in which putrefaction is about to commence, should be daily sponged with it. Its action is instantaneous. Parts which have become green and softened are at once disinfected, all the tissues it comes in contact with becoming white and hard. It acts by precipitating the soluble portion of the animal fluids, as albumen, fibrin, &c., and it coagulates cerebral and fibrous tissue.

Both substances are expensive to purchase, but their preparation on a large scale, the formulæ for which the author supplies, may be conducted very economically; so that three or four shillings will defray the expenses of preserving each subject.—*Archives d'Anatomie*, 1846, p. 122.

(In the *Annales d'Hygeiène* for April is a short paper by M. Guerard, who was one of the Commissioners appointed to examine into the success of the experiments with these substances, which were carried on during several months at the *Ecole Pratique*. The field is a wide one, for during 1845 there were upwards of 1500 bodies received for dissection. At *Clamart*, where the process is also about to be adopted, the average exceeds 2800: so that the annual number of bodies dissected at Paris considerably exceeds 4000—an enormous number when compared with that furnished to the London Schools, where the difficulty in obtaining subjects seems to be annually increasing, and the supply of which is entirely inadequate for the proper teaching of anatomy. The more limited the supply, however, the more important the means here proposed for economizing it, providing it prove as efficacious as represented. M. Guerard's statement is most unequivocally favorable to it. He states that he has repeatedly visited the dissecting-rooms since it has been in use, and finds them utterly devoid of all offensive odours, and the complaints which used to be made on account of these, by the inhabitants of the locality, have entirely ceased. Bodies may be dissected for from 15 to 40 days, according to circumstances, without any attendant inconvenience.—*Rev.*)

ON TYPHOID FEVER. By M. BRICHETEAU.

M. Bricheteau has recently published some interesting papers upon this subject, of which our limits, however, forbid any prolonged notice. He maintains that the history of the disease proves that, forty years since, the intestinal canal did not present the lesions now so commonly remarked, for these must have been noted by such observers as Bayle, Dupuytren and Laennec. It may be laid down as a principle, therefore, that *typhoid fevers, without absolutely changing their nature, present at certain epochs notable modification, sufficient to effect a change in their diagnosis and treatment.* An illustration of this is also derived from the fact of no such considerable changes being observed in the epidemic fevers of Great Britain and Ireland. Even in France such lesions are absent in some epidemics, and are in others no-wise in proportion to the gravity of the symptoms; so that they cannot be considered the essential feature of the disease and its point of departure, although, from their dangerous character, they naturally have excited much attention.

M. Bricheteau observes that the contagious character of typhoid is not admitted in Paris, although believed in some of the provinces; but he is certainly in error in stating that the contagion of typhus is generally admitted in this country, and that a vigorous sequestration is consequently insisted upon. He is more correct as to the treatment which prevails here, for certainly the bleeding and antiphlogistics employed by many in Paris, would meet with few approvers in London. In explanation, he observes that inflammatory lesions are more common in Paris, as its climate is less debilitating than that of London. Sydenham, who had studied disease at Montpelier, acknowledged that his practice was too vigorous at the commencement of his career in London; and many

other practitioners have observed that bleeding is much less easily borne in that capital than in Paris.

E. Bricheteau believes, however, that typhoid cannot be looked upon as a *phlegmasia*, inasmuch as neither the season of the year at which it occurs, the treatment it requires, the order of sequence of the abdominal symptoms, or the measures which relieve these, favour this view. "By seizing on this inflammation as a basis of treatment, we commit the error of neglecting the cause for the effects of the disease." M. *Delaroque*, of the *Hôpital Necker*, and others, believe the symptoms of typhoid are due to the irritating effect of *vitiated bile and other secretions* which are thrown out upon the intestinal mucous membrane, and are then absorbed, poisoning the economy. Andral, Bouillaud, and Louis, are cited in proof of the enormous quantities of such secretions which are found after death, while the success of the purgative treatment pursued by M. D. is adduced in corroboration of this view. Other observers explain all the symptoms of typhoid by a primary *alteration in the condition of the blood*. The dissolved state of this fluid has been often observed by both the older and modern practitioners, and symptoms identical with those of typhoid have been induced by injecting putrid matters into the veins—the blood losing its plasticity as in typhus.

Conclusions.—The placing the seat of typhoid in the intestinal canal, the cerebro-spinal system, the blood or its secretions, seems to me of very little consequence, since this disease affects simultaneously or successively every portion of the economy. We find, first, the phenomena incidental to a disturbance of the nervous system, in which disturbance the circulation soon participates. Almost immediately afterwards, diarrhœa and other symptoms of intestinal irritation present themselves, as also the *râle* which proves the pulmonary organs are not unaffected. Other symptoms soon show that the various secretions are either perverted or temporarily suspended. The affection seems to prevail in some degree everywhere, and to have its special seat nowhere. It is truly the *morbus totius substantiæ* of the old physicians. It is often epidemic, and sometimes contagious, presenting much analogy with many pestilential fevers, typhus, sweating-sickness, and intense epidemic influenza—diseases to none of which a precise seat can be attributed. It follows that the *treatment* of typhoid should be mixed, being composed of measures as various as the lesions to be combated. Sometimes diluents and antiphlogistics, sometimes evacuants, at others alteratives, tonics, anti-septics, or sedatives may be required. In fact, to adopt any exclusively curative method, would be to apply a determinate medication to an unknown pathological condition.—*Gaz. Méd. Chir., Nos.* 13, 15, 17 & 23.

ENGLISH AND FRENCH MEDICAL PERIODICALS.

The *Gazette Médicale*, for June 13th, publishes an article upon the English Medical Periodicals. That it is replete with error will not excite the surprise of those who are aware how completely everything relating to this country is misunderstood in France, and who recollect the ludicrous mis-statements of M. Malgaigne, (*Méd. Chir. Rev., Jan.*, p. 284,) at the Medical Congress, in reference to the position and acquirements of our general practitioners. Still, writers who profess the instruction of their readers upon any point, should at least inform themselves of the facts, and we regret our space will only permit our indicating one or two instances in which the present one has disregarded these. He states that the Quarterly Medical Reviewers of this country confine themselves to the insertion of mere *verbatim* extracts from the works they examine, carrying these sometimes to the extent of a *third* or *fourth* of the publication in hand, without any attempt

at critical appreciation of its merits or demerits; and he complacently thanks his stars there are none such in France. The refutation of this statement may be safely left to our readers; but we must observe that, did publications such as these really are exist in France, the profession of that country would much benefit by exchanging the present interminable and wearisome original articles often by obscure writers, (worked up with a display of elementary information, a pedantic magniloquence, and an affectation of definitively settling the matter that would be insufferable here,) which often encumber their journals for prompt, impartial, critical, and copious accounts of every native or foreign original work that appears. Moreover, is not the high position which this writer allows our medical journals assign in their columns to notices of the progress of medicine in France due to the cosmopolitan spirit which has ever actuated the British Medical Press, through whose agency every English practitioner is as familiar with the names and labours of the continental practitioners as he is with those of his own country. Were the same copious notices bestowed by the French medical press upon the various works which issue from this country, the utter ignorance respecting the progress of medical science in England, which at present not only generally prevails among their readers, but too often disgraces their writers, would surely be diminished.

Again it is stated that the "Lectures" published in the weekly periodicals are generally mere reprints of works which have already appeared! We should be curious to see the medical journal that would venture to publish such. Admitting that these lectures must necessarily be very unequal in value, every one knows that several of the courses originally so given to the world, have since been re-published (reversing the order stated by our friend,) and now form works of standard and sterling value. The names of Cooper, Abernethy, Clutterbuck, Lawrence, Latham, Liston, Brodie, Murphy, Conolly, Watson, &c., &c., &c., are surely guarantees that the perusal of such lectures will not be without ample profit. In fact, there is scarcely a man of any celebrity in London whose opinions have not been thus published.

The writer is horror-struck at the great number of advertisements, quite unconnected with medicine, which are attached to several of our journals. The practice of advertising, like many others of equal utility, is very little developed in France, and therefore it is no-wise surprising that the medical periodicals do not largely participate in it. In England the advertisements are a separate concern from the professional portion of the journal, and their reception and arrangement does not devolve upon the editor, but upon the publisher—care being always taken that they do not intrench upon the specific space (in conscience large enough for the price) devoted to the proper object of the publication. Advertizers, aware that these publications circulate extensively among medical men, who often have little time for the perusal of those of a more general nature, and believing that the fact of a person pursuing a liberal profession does not exempt him from the ordinary wants of mankind, naturally take the opportunity of offering to his notice whatever they have to dispose of, and what there is reprehensible or ridiculous in this it would be difficult to discover. But we may safely assert, that if the advertisements are more *numerous* in London than in Paris, they are also more *select*; and that even the extra-professional pages of our medical journals will be never found disgraced, as are some of those of Paris, by advertisements of Morrison's Pills, Maisons d'Accouchement, Secret Pills, though approved by Academies; or by woodcuts of quack corsets, and women employing uterine irrigators, &c.

One thing, which is noticed by this writer, we do certainly feel somewhat ashamed of, viz.; the eagerness with which young men who have passed a few seasons at Paris display the "slightest honorary grade there acquired, as if it were a most powerful recommendation on the other side of the Channel." We are not disposed to underrate the advantages of a visit to Paris: but we are so

far from believing that a *prolonged* residence in that capital is an advantageous preliminary for an English practitioner, that we quite agree with our esteemed predecessor, the late Dr. Johnson, that the first thing persons so educated find on their return is, that they have as much to unlearn as to learn, before they can become safe and efficient practitioners.

LIGATURE OF BOTH CAROTID ARTERIES FOR A REMARKABLE ERECTILE TUMOUR OF THE MOUTH, FACE, AND NECK. By J. MASON WARREN, M.D.

A man, æt. 23, presented himself to the author's notice, the left side of whose face, neck, and mouth was occupied by an enormous erectile tumour of four years' growth. The tongue has been swollen, and the lip occasionally ulcerated, during this period. This last now threatens to take on a cancerous degeneration, and this, together with the fear of fatal hæmorrhage occurring sooner or later, induced Dr. Warren to perform the operation. On the *5th Oct.*, 1845, the left carotid artery was tied. The patient was about in ten days, the tumour diminishing in size, and the ulcer healing rapidly—his health seeming perfect. *Nov. 7th.* The swelling still much diminished and paler. The right carotid was now tied, with no other inconvenience to the patient than drowsiness, and faintness on raising the head. *Nov. 29th.* Ulceration of the lip had healed, but as it continued thick and everted by the erectile tissue, it was deemed advisable to remove the diseased portion by a V-like incision, the slight hæmorrhage which attended this operation being easily controlled. *Dec. 12th.* The discolouration of the face has become much paler, and the ear resumed its natural size. No pulsation can be felt in the temporal arteries. Just above the clavicle two arteries, nearly the size of the carotids, are seen pulsating powerfully under the skin. *Feb. 1st.* Patient is in perfect health, and has not had the slightest indications of disturbance in the brain from this interruption of its natural circulation.—*Amer. Journ. Med. Soc., April.*

ON THE GRANULAR DISEASE OF THE PHARYNX. By M. CHOMEL.

During a year or so that M. Chomel's attention has been directed to this affection he has seen, or collected accounts of, about 30 cases. Of these, none has been observed in persons younger than 15. Of 22 cases occurring in his own practice, 17 were males, most of them (like women subject to uterine granulations) being also liable to herpetic eruptions, and especially to acne. In most there is also a peculiar form of the palatine arch, narrowing the nasal fossæ, and contracting the lips, so that these are never completely closed. Such persons sleep with the mouth half open, and awake with it in a very dry state, the pharyngeal follicles becoming, under these circumstances, excessively developed, in consequence of the constant evaporation of the buccal fluids produced by this contact with the air. Thus, also, the persons in whom the complaint is most frequently met with, are those who employ their vocal organs considerably, as orators, singers, advocates, professors, &c.

The affection usually comes on slowly, and excites little uneasiness at first, save from the constant hawking it gives rise to. This sometimes becomes quite sonorous, and is accompanied by involuntary attempts at deglutition, and if the irritation is propagated to the œsophagus, a frequent desire to drink. There is a dryness or itching in the pharynx, and the voice is more or less affected. The expectoration consists of transparent viscous globules of a slight opaque tinge,

or streaked black or slate colour. On examination, we find the pharynx covered with little red points the size of hemp-seed, but sometimes being much more voluminous. They may be grouped into arabesque-like forms, into little discs, or as nipple-shaped projections. Granules are also usually found upon the palate and uvula, but then they are more discrete. The mucous membrane retains its normal characters.

The course of the disease is always chronic and remitting, it being especially troublesome in cold and damp weather. It never spontaneously disappears, and often obstinately resists treatment. Of 14 cases which M. C. had occasion to see at more or less remote periods after treatment, but 4 remained cured, the others being only relieved. The diagnosis of the affection is easy enough, as it obviously consists in hypertrophy of the numerous muciparous follicles of the pharynx. In *treating* the disease, M. Chomel has tried all descriptions of astringent gargles, &c., with but indifferent success, and when it proves obstinate, cauterizing the mucous membrane is the only means likely to prove serviceable, fluid caustics having been found more useful in his hands than solid ones.—*Gazette Médicale*, No. 16.

On Paracentesis Thoracis in Acute Pleuritic Effusion. By M. Bricheteau.

In reporting upon an additional account of this operation, recently presented to the *Académie* by M. Trousseau, M. Bricheteau adverts to the fluctuations of opinion which have occurred in reference to it, as applied to empyema, in both ancient and modern times. In was in 1841 M. Trousseau adduced two or three successful instances of its application to cases of recent pleuritic effusion, and in the present paper another is brought forward. The patient, æt. 24, had suffered from pleurisy about 20 days prior to admission. The side was notably dilated, and the effusion not yielding, a puncture was practised between the 7th and 8th ribs, and about 70 oz. of a clear fluid discharged with manifest relief, the patient being dismissed in about a fortnight afterwards. She has been since seen by M. Bricheteau, who found her cure confirmed. Two other successful cases have occurred since this paper was presented, the particulars of which are furnished by M. Bricheteau.

In considering whether M. Trousseau's success is such as to warrant the recommendation of the imitation of his practice by the *Académie*, M. B. observes, we must bear in mind that, even in the last century, when our diagnosis was so much more uncertain, examples of cure by this means were by no means rare. MM. Monneret and Fleury state that, of 66 instances in which we have accounts of its having been performed, in 56 the effusion was the result of pleuritic inflammation, and that of these 42 were cured and 14 died. M. Trousseau believes that we should not wait until the dyspnœa become very urgent and suffocation impending, as danger may exist, if the fluid have gradually accumulated, long before this. If the dulness upon percussion be perceived along the median line, as low as the 4th rib, the puncture may be made, although the dyspnœa be not urgent; while, if it extend daily across the median line, towards the opposite side, the necessity for interfering is still more apparent, especially if the effusion be on the left side, and tends to displace the heart towards the right. If, even when the dulness does not extend beyond the median line, there is orthopnœa, a small frequent pulse, anxious countenance, and especially any tendency to lithopymia, we should operate at once.

M. Trousseau prefers evacuating all the fluid at one operation, and guards against the admission of air (by disposing a piece of gold beaters' skin as a valve over the orifice of the canula,) which might oppose the re-distension of the

pulmonary structure in its now enlarged cavity. The difficulty with which a condensed lung re-expands is far less in recent than in chronic pleuritic effusion, as firm adhesions have not formed, and the elasticity of its texture is not yet destroyed. In M. T.'s cases, the Reporter ascertained that the air penetrated immediately into every portion of the pulmonary structure. The existence of pulmonary tubercle is not an absolute objection to the operation, as the removal of fluid in this case has sometimes at least prolonged life.—*Gaz. Méd. Chir.*, Nos. 17 and 18.

(In the brief discussion which followed, M. *Louis* observed that, he thought the operation should be more cautiously recommended. He had seen many cases of pleurisy, but none in which it appeared indicated; nor had he ever met with a case which terminated fatally when the disease was in its simple form. M. *Rochoux* stated that, although cases in which this operation could be called for must be very rare, that yet they may occur, and that more frequently in acute than chronic pleurisy. M. *Bricheteau* agreed in this last opinion. We believe with the above practitioners that, the necessity of an operation in simple pleurisy, properly treated, can never hardly occur: but the case is very different in more complicated instances, and in even simple ones which have been neglected. In such we doubt not of its propriety, and see no reason to hesitate about its performance. The extension of the operation has in this country met with many advocates of late, and we may refer among others to the papers of Drs. Hughes and Roe, in the Guy's Hosp. Rep. and Medico-Chir. Trans.—See Med. Chir. Rev., N. S. Vol. I.—*Rev.*)

MANAGEMENT OF THE PUERPERAL STATE.

In M. *Piorry's* ward at *La Pitié* are women suffering from fever producing eschars on the sacrum, and others brought from the *Maternité* labouring under puerperal peritonitis, and yet, although the ceiling is low and the beds crowded, no instance of phlebitis, phlegmasia dolens, or peritonitis, putting on the dangerous character so common in the puerperal state, has ever been observed among the women delivered therein during the ten years he has had charge of it. In this same ward five women have been brought from a *maison d'accouchement* suffering from utero-peritonitis, and of these three died. At the *Maternité* the mortality has been such as to lead to the distribution of the remaining patients among the other hospitals. M. Piorry, regarding any ill effects which may result from the admission of cold air as minor evils, has all the windows, however, near the beds, kept wide open and the bed-curtains drawn back, taking care, however, that the woman's exposed person do not become subjected to the currents. Immediately after delivery, he orders the vagina to be carefully injected with tepid water every hour or two. He has the abdomen rubbed with olive oil, orders good diet, and keeps the bowels open with aperients and enemata. He thinks lying-in women should be admitted into the general hospitals, and not congregated together in special ones.—*Gaz. des Hôpitaux*, No. 43.

ON THE PUBLIC HEALTH OF PARIS DURING THE FIRST QUARTER OF 1846.

The *Gazette Médicale* has recently published some interesting articles upon this point. From the Registries of the Paris Observatory, it appears that—1. The *temperature* was unusually high. 2. The *atmospheric pressure* more considerable

than ordinary. 3. A remarkably small quantity of rain fell in February. 4. South and west winds predominated. 5. The oscillations of the thermometer and barometer were neither considerable nor sudden. 6. All the above characteristics were more marked in February than in the other two months.

The *diseases* of the quarter may be considered in reference to their *form*, *severity* and *frequency*. 1. As regards the *form*, the most remarkable thing was the *rarity of the acute affections of the respiratory organs usually met with in winter* —bronchial catarrh itself being even far from common. Even those which did present themselves were very mild, sometimes assuming rather a neuralgic than an inflammatory character. The diseases *commonly met with in summer and autumn* were on the other hand frequent. Thus *gastric* and *bilious fevers* were very prevalent, and what is still more uncommon at this season several cases of *typhoid fever*, generally of an ataxic form, occurred. Others of a rare disease at Paris, *remittent fever*, were met with, the adynamic form prevailing. In February and March *puerperal fevers* abounded, women who were not pregnant or puerperal, and others who had been confined for a fortnight or so, suffering at the same period from induration of cellular tissue of the pelvis, and pains in the renal and abdominal regions. Finally, a malignant and fatal form of *measles* prevailed. *Adynamia and ataxia* formed the common character of these various affections. In fact, the season of the quarter from January to April resembled that of summer and autumn combined. To a very unusual and little changing elevation of temperature, were conjoined on the one hand southerly and westerly winds, and on the other, in January and March, considerable falls of rain; and the simultaneous inversion of the medical and atmospheric constitutions is remarkable. Medical constitutions do not, however, depend upon the absolute quantity of heat or moisture; and a given atmospheric condition will produce different effects according to the epoch of the year at which it is observed. Thus, the temperature during which these affections appeared was far less elevated than that which is observed at the time of their prevalence in summer.

2. The *frequency* of disease, as exhibited by the number of admissions into the hospitals was increased; for, while these amounted to but 18,807 in the first quarter of 1845, when the weather was much colder from northerly winds, and the atmospheric perturbations much more considerable, they reached 19,846 in the quarter of 1846. *Diseases put on different forms then, but increased in number.* In examining their distribution over different months, one is struck with the comparative immunity of February, during which there were 300 patients less than in January, and 850 less than in March. Of all the atmospheric conditions the *quantity of rain* alone considerably differed in this month. The total quantity of rain which fell during the first quarter of 1845 was less considerable than that which fell during the same period of 1846 in the proportions of 12·060 to 15 067. In both years, too, the amount which fell in February was considerably less than that which fell in the other two months of the quarter, and the number of admissions diminished in like proportion.

3. The *severity* of the diseases can only be imperfectly estimated by the number of discharges from the hospitals. From these, however, their benign character may to some extent be inferred—since of a hospital population of 37,056 in the quarter of 1846, there were 18,213 discharges; while of a population of 36,043 in 1845, there were but 16,695. The generally mild character of prevalent affections does not however necessarily imply a *small proportion of deaths.* In this ataxic constitution the majority of the patients do not exhibit alarming symptoms, but amid apparent security diseases from time to time put on unusual and unexpected fatal characters. Thus, in spite of the large number of dismissals that of the deaths was yet greater than in 1845. In that year, of 36,043 patients 1958 (1 in 18·4) died, while the deaths of 1846 were 2073 in 37,056 patients (1 in 18·1.)—*Gazette Médicale*, Nos. 18, 19, 20.

(It may be interesting to compare some of these *data* with those furnished by the Registrar-General's First Quarterly Report for 1846. By this we find that the mean temperature at Greenwich was nearly 5° Fah. above the average of 25 years, and 8° above that of the first quarter of 1845. South-west winds prevailed, and the fall of rain was as high as 5·73 at Greenwich, it being but 4·80 in 1845. With these meteorological conditions the deaths in a population of 6,579,653, amounted to 43,708, being less by 6166 than those (49,874) registered during the same quarter of 1845.

Confining our attention to the *metropolis* (1,915,104 souls,) we find that the deaths from all causes registered in the quarter ending with March, 1846, (12,376,) were less by 2152 than those registered (14,528) for the same quarter of 1845. Notwithstanding this gross result the following diseases *exceeded* the average mortality of the winter quarters of the seven preceding years of registration. *Measles* (viz., 401 deaths instead of 273,) *Hooping-Cough* (as 767 to 504,) *Diarrhœa* (as 119 to 72,) *Bronchitis* (as 758 to 320.) and *Rheumatism* (as 62 to 35.) The mortality of the following has diminished—*Small-pox* (as 77 to 291,) *Scarlatina* (as 221 to 357,) *Croup* (as 79 to 107,) *Convulsions* (as 511 to 727,) and *Pneumonia* (as 946 to 1178.) Taking the Endemic, Epidemic and Contagious diseases *en masse*, the mortality is very near the average (as 2222 to 2277,) which is the case also with regard to typhus (410 to 403,) and deaths from *Childbed* (101 to 104.)

It will be seen that the statement of the influence of rain in augmenting the amount of sickness and mortality alluded to in the French Report is not verified in that of the English one; nor have precisely the same diseases especially raged in London as in Paris. It is true we are here only speaking of deaths and not also of unfatal diseases; but every practitioner must know, to his cost, how much less numerous these latter have been during this quarter than usual; and most will have remarked the adynamic and neuralgic characters of many diseases not usually so distinguished.—*Rev.*)

ON THE EXCESSIVE MORTALITY OF MALE CHILDREN. By Dr. EMERSON.

Dr. Emerson observes that, ever since correct mortality registers have been kept, a larger proportion of males have been found to die in the early years of life than females. In Europe, the male *births* exceed the female ones by about 5 or 6 per cent., and in Philadelphia by more than 7½ per cent., and yet the number of boys and girls are found very near equalized by the 10th year, while by the 15th, the females outnumber the males almost as much as the latter did the former at the time of birth. In Philadelphia the deaths of boys to the 15th year exceed those of girls by about 15 per cent.

Upon examining the question more in detail, Dr. E. found that *inflammation of the brain* and its consequences, *convulsions*, *hydrocephalus*, and *inflammations* in general, were the diseases which especially proved fatal to male children, while *pertussis*, *scarlatina* and *phthisis* were those which proved generally fatal to female children. That is to say, that the males perish from the *sthenic*, the females from the *asthenic*, class of affections. On examining the London Tables he found the same disposition nearly prevailed, and the practical deduction he draws from the whole is, that the treatment of the diseases of males should be more prompt and vigorous than that of those of females.

The result of his observations upon *temperature* leads him to conclude that the hot months in America act as injuriously upon infantile life as do the winter ones; but that, after the child has weathered the first few months of its existence, neither the one nor the other seems to especially determine mortality.—*American Journ. Med. Sciences.*

MIALHE ON THE ASSIMILATION OF AMYLACEOUS AND SACCHARINE SUBSTANCES.

A Report has just been made to the Academy upon the continuation of M. Mialhe's researches upon this subject. He observes that it has been long acknowledged that *albuminous* or *azotized* aliments are digested through the agency of *papsine* which is a true ferment, and *fatty* bodies by that of the bile; but that it has been proved only by his own researches that *amylaceous* and *saccharine matters* are assimilated through an active principle of the saliva. This principle exerts no action upon azotized substances, fibrin, albumen, &c.; but it exerts a most remarkable one upon starch, especially when this is completely disintegrated by bruising, heat, water, &c. One part by weight suffices to liquefy and convert into dextrine and sugar more than 2000 of fecula. This transformation is not without its analogy in science, for there exists a body which exerts upon starch a specific power exactly resembling that of the salivary ferment, viz., *diastase* or the active principle of germinated barley, discovered by MM. Payen and Persoz. A long course of experiments lead to the inference that the two principles are identical, although M. M. contemplates performing others for the further elucidation of this point. In the mean time, he proposes terming the active principle of the saliva of man *animal diastase* or *salivaire*, in distinction of that of grain which may be termed *vegetable diastase*. The proportion of each existing in saliva and barley respectively, is 2000 parts. To obtain it, saliva should be treated with 5 or 6 times its weight of pure alcohol, adding this as long as any precipitate is produced. The diastasis is deposited in little white flakes, which are to be collected on a filter and dried between two thin plates of glass in a current of air at a temperature of from 104° to 122°, and kept in a well-corked phial. It is a white or grayish amorphous solid insoluble in alcohol, but soluble in water and diluted in alcohol.

The following are the conclusions. 1. The saccharifaction of starchy matters is effected by the influence of the diastase which exists in the normal state in the liquid secreted by the pancreatic and salivary glands. 2. This transformation of amylaceous substances into dextrine and glucose is not as some believed a pathological but a physiological fact; for without it they would cease to be alimentary, not being indeed absorbable until acted upon by the diastase. 3. The dextrine and glucose (*i. e.*, the newly saccharified matter) to become assimilated *must be transformed by the alkalis of the blood* into new products, the chief of which probably are the kali-saccharic acid, formic acid, and ulmin. 4. If the alkalescence of the blood is insufficient (the blood having become too feebly alkaline, neutral, or even acid,) the transformation will not take place; the sugar becomes a foreign body in the economy, and as such is excreted by the kidneys, giving rise to *diabetes* or *glucosuria*. 5. Saccharoid bodies play an important part in the great act of nutrition, and do not serve merely as materials for respiration, as some eminent men have supposed; but on the contrary it is certain they participate in the chemico-vital re-actions which preside over the incessant organic mutations, and it results that, if their assimilation is abolished, (chronic diabetes,) or simply vitiated, (acute diabetes,) the anormal molecular decompositions are effected at the expense of the living tissues and liquids, and it is hence we observe—1. A general disturbance of the humours of the economy from defect of alkalescency, which gives rise to feebleness of sight, capillary engorgements, and pulmonary consumption. 2. A profound alteration in nutrition, leading to debility, languor, and wasting. It is to these two physiologico-pathological facts we must attribute the constantly fatal termination of diabetic diseases, when by a methodical plan of treatment the power of decomposing the amylaceous class of aliments has not been restored to the chemical laboratory of the human body.

According to these views, M. Mialhe recommends the employment in the treat-

ment of diabetes of animalized regimen as little fecula as possible, and the use of the alkaline bases and their carbonates, as magnesia and lime. The number of cases improving under such a plan are not sufficient to induce the *Académie* to pronounce definitively upon this point; although it entertains a high opinion of the value of M. Mialhe's researches, and begs him to continue them. *Comptes Rendus*, No. 12, and *Gazette Médicale*, Nos. 18 and 19.

VARIA.

Thoracic Vibration.—M. C. Broussais observed that he is surprised this sign is so much neglected in practice. In health it is much less marked in persons having weak or sharp voices, and in whom the cellular tissue is infiltrated or adipose, than in those whose voices are grave, and the walls of whose chests are of little thickness and only covered by their muscular layers. In disease it becomes *diminished*, in pleuritic effusion (and perhaps in the case of false membranes) re-appearing when the fluid is absorbed. It is *increased* in the induration of the parenchyma by red or grey hepatization, in chronic pneumonia, and when the tissue of the lung becomes condensed by pleuritic effusion, providing this be not too abundant, when the lung becomes reduced to nothing, or too sparing, when it is not sufficiently condensed.—*Gaz. des Hôpitaux*, No. 46.

Rheumatic Carditis.—M. Chomel, in a recent clinical lecture, stated that he believed that affections of the heart as a consequence of rheumatism, are of exceeding rare occurrence.—*Gaz. Méd. Chir.*, No. 13.

Extemporaneous Vesication for Endermic Medication.—Place a piece of silver money on a plate, and lay upon it two circles of old linen of rather less diameter. Saturate these with *liq. ammon.* and then apply the apparatus, with its linen surface downwards, to the skin, and maintain an equable pressure with the finger. In 10 minutes the skin is observed to redden at the circumference of the money, and on the apparatus being removed vesication will take place.—*Bulletin de Thérapeutique.*

Compression of the Carotid in Epistaxis.—Two cases are related in which obstinate epistaxis was effectually arrested by temporary compression of the common carotid artery.—*Gaz. Méd. Chir.* No. 23.

Endermic Use of Iodine.—Dr. Diver recommends the following formula as an excellent application in scrofulous enlargement of the glands of the neck. R. *Iodin.* 1 p., *Bals. Canad.* 3 p. *Picis. Abietis* 3 p. Triturate the Iod. with the B. C., and melt the pitch at a gentle heat, mixing the whole together just as it is about to cool. Spread on kid for immediate use.—*Phil. Med. Exam.*

Quinine in Rheumatism.—In addition to M. *Briquet's* testimony in favour of large doses of Quinine in rheumatism, quoted at p. 267, we may cite that of Dr. Dunglison, who looks upon it as a powerful sedative, capable of promptly curing the great majority of cases. He gives it fearlessly in every case, never having seen any ill effects result, beginning with 18 or 20 grs. in the 24 hours, and gradually increasing the quantity.—*Philadelphia Medical Examiner.*

Local Paralysis from Pressure.—A woman under M. Guerard's care has complete paralysis of the fore-arm and hand from using crutches for a fortnight. During the few days the patient has kept her bed the fingers have somewhat regained their motility (*Gaz. des Hôp*) M *Pigeolet* relates the case of a person who having slept during a journey with his arm compressed between his head

and the edge of a carriage, it became paralyzed for six weeks. Every kind of counter-irritant was unavailingly tried until an embrocation, composed of *ol. oliv.* and a few drops of *tr. lyttæ*, was prescribed, which was speedily successful. An Italian surgeon has also of late met with several cases in which paralysis has been induced by lying upon the arm in bed.—*Gaz. Med.*

*** The intimation, which we have given at page 230, will account for our omission of any extracts from the French Journals respecting the recent very important discussion on *Plague* and *Quarantine*, in the French Academy.

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A masterly exposition of the principles of the Baconian philosophy as applied to medical science. It is written with great power of reasoning, and its language is excellent. It deserves to be reprinted in this country.

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Multum in parvo.

8. An Atlas of Anatomical Plates of the Human Body, accompanied with Descriptions in Hindustani. By *Frederick J. Mouat*, M. D., F.R.C.S.E., &c. Assisted by Moonshi Nusseerudin Ahmud, late of the Calcutta Madrassa. Fasciculus I., containing the Bones. Folio. Calcutta, 1846.

Very great praise is due to Dr. Mouat—who is one of the professors in the Bengal Medical College, at which there are 150 native students regularly engaged in dissection!—for this publication. The drawings on stone were made by Mr. Grant; and the letter-press has been selected from the writings of Quain, Meckel, and Bell. Four other fasciculi, to complete the work, are promised.

9. Chelius' System of Surgery. Translated by *South*. Part. II.

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By avoiding all minute details on unimportant matters, and carefully separating the wheat from the chaff, Dr G. has compressed into this edition—the sixth in the course of 25 years—a very large amount of useful information. No safer guide can be put into the hands of the young practitioner. It is eminently practical.

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These Observations deserve the attentive perusal of the pharmaceutical chemist. Part of them appeared in our numbers for January and April, 1845.

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This little work, consisting of well-executed woodcuts of the bones and muscles, will prove a useful portable companion for artists.

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THE MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1846.

I. Rapport a l'Academie Royale de Medécine sur la Peste et les Quarantaines, fait, au nom d'une Commission, par M. le Dr. *Prus ;* accompagné de Pieces et Documents, et suivi de la Discussion dans le Sein de l'Academie. Ire et IIe Parties. 8vo. pp. 663. Paris, 1846. Bailliere.

II. Correspondence respecting the Quarantine Laws, since the Correspondence last presented to Parliament. Presented by Command to the House of Commons, in pursuance of their Address of May 19, 1846. Folio, pp. 48.

It must surely be quite unnecessary to say a single word in the way of soliciting our readers' patient and most attentive consideration of the facts and reasonings which we are about to bring under their notice. The subject of the Quarantine Laws is one of public and very general interest. All persons are more or less immediately concerned in their operation and effects ; for whatever interferes with the free and unrestrained intercourse of one nation with another, cannot fail to affect the common welfare. To the medical man the subject is, as a matter of course, doubly and trebly interesting ; some of the most curious and important questions, connected with the natural history of epidemic diseases, are involved in its right adjustment. It is to medical doctrines and to medical opinions that we owe the present system of prohibitory restrictions, which so seriously interfere with the social comforts and commercial success of numerous countries ; and therefore for this reason alone, if there was no other, it well becomes the members of our profession to be foremost in making a calm and candid examination of those doctrines and opinions from which such grave consequences have followed. Now, every one who has made himself acquainted with the subject, be he physician or merchant, traveller or statesman, has of late years, without exception, come to the decided conviction that it is high time for a thorough revision and a very material modification of the quarantine laws, such as they now exist, to take place. The absurdly foolish and most ridiculous principles which they embody, the vexatious and oppressive restrictions which they impose, the wretchedness and suffering which they almost necessarily give rise to, and the great increase of mortality which, we have reason to believe, they often occasion, are surely sufficient grounds for the scrutinizing investigation that is

so generally demanded. For some years past the British Government and that of France have been using their best exertions to effect a change, and have been trying to get the other Continental powers to co-operate with them in their good work. With this view, they have proposed that a Congress of Delegates from the different States of Europe should be held in Vienna, or elsewhere, for the purpose of agreeing upon some general and uniform system of Quarantine regulations to be adopted in the ports of the Mediterranean. Many difficulties, we regret to say, have been thrown in the way of this most equitable proposal by Prince Metternich, on the part of Austria, and by the representatives of some of the minor powers.* Will it be believed that, in several of the Italian States, the quarantine boards of health are actually independent of the government?—they possess a sort of patent vested right in the profligate exaction of their fees of office! Still, we must not be dicouraged, nor diverted from the reform that is so loudly called for. Truth will assuredly prevail in the long run, despite the opposition of ignorant bigotry on the one hand, and of the basest mercenary intolerance on the other. We have only to keep the subject prominently and steadily before the public mind, by collecting accurate and well-authenticated facts from every quarter, and by not ceasing to expose the enormous fallacies and absurdities which prevail, and we may be confident that, ere long, the system which has so long existed to the serious detriment of commerce and to the disgrace of common sense, not to talk of science and humanity, will be made to undergo such changes as the present state of general enlightenment and sound knowledge requires.

It will be seen, from the following pages, that neither the French nor the English government has at all relaxed in their efforts for this most desirable end. Both have been accumulating materials for information, and collecting the opinions of competent authorities, in order that they may be ready to meet the objections or overcome the prejudices of their opponents.

In August, 1844, the Royal Academy of Medicine in France appointed a Commission to examine all the varied questions connected with the Plague and with Quarantines. This Commission was composed of the following members—men, we may remark, of the highest professional and scientific attainments—MM. Adelon, Begin, Dubois (d'Amiens), Dupuy, Ferrus, Londe, Melier, Pariset, Poiseuille, Prus, and Royer-Collard. M. Ferrus was named the president, and M. Prus the secretary and reporter. The Commissioners were engaged in their deliberations for upwards of twelve months, and had every facility granted them by the French government, to render their enquiry as complete and as accurate as possible. At length, the report was drawn up and read at the sittings of the Academy, on the

* It is only doing justice to the Austrian government to state that it has already made some very useful practical reforms in its quarantine regulations. While France has been talking and planning, Austria, in imitation of the example set by this country, has been acting. Passengers from Alexandria can now reach Paris, *via* Trieste or Southampton, considerably sooner than by going to Marseilles, in consequence of their detention in the lazaretto there!

5th, 10th, 17th, and 24th of March and the 5th of May of the present year. It is certainly a very elaborate and instructive work, replete with most valuable facts and data, which cannot fail to be truly acceptable to every enquirer upon the great questions under consideration, whether he admits the soundness of the conclusions adopted by the majority of the Commission or not. It is for this reason that we have thought it right to bring before the attention of our readers, with as little delay as possible, a faithful summary of its contents, in order that they may be able to judge for themselves of the value of the original.

Although the Plague has so often ravaged the world and there has been no lack, as a matter of course, of books and memoirs published at different times upon the subject, it must be confessed that the number of instructive and really accurate narratives of well-observed *facts* is by no means very considerable. The epidemics, of which we have the most trustworthy histories, are the following:—that of Nimeguen in 1635, described by Diemerbrock; that of London, in 1665, described by Sydenham and Hodges; that of Marseilles in 1720, by Chicoyneau, Verney, Deidier and Bertrand; that of Transylvania in 1755, by Chenot; that of Moscow in 1771, by Mertens, Orrœus, and Samoilowitz; and those of Egypt in 1798, 1799 and 1800, which have been so well described in the writings of Desgenettes, Larrey, Pugnet, and Louis Frank.

But, however valuable the records of the epidemics now mentioned may be, it must be admitted, we think, by all who have attentively studied the history of the plague, that it is only within the last ten or twelve years that we have anything like a positive and truly scientific acquaintance with the disease. Dr. Aubert-Roche was the first to display that brave and generous devotion to humanity and science, which has since been followed by so many of his professional brethren, when he brought himself in direct contact with his friend Dr. Fourcade, who died of the plague at Cairo on the 20th of February, 1835.*

Shortly afterwards, numerous plague patients were received into the hospital of Esbekiè, at Cairo. Clot-Bey, anxious to give the most complete authenticity to the observations which might be made of these cases, proposed to MM. Gaetani, Lacheze, and Bulard to join with him in forming a committee or board for the purpose of attending together upon all the patients in the successive stages of the disease, and of making *post-mortem* examinations. These four gentlemen carried through this task with the greatest zeal and devotedness. The infected were waited upon like other patients; they were freely touched whenever there was occasion to do anything for their relief, or for the investigation of their symptoms. The bodies of those who died were taken to the dissecting amphitheatre, and every organ was most attentively inspected. The results of each visit in common were carefully reported in a register, and each report was regularly signed by all four. This register (which was submitted to the perusal of the government Commission) is the chief basis of the works, which have been published by Clot-Bey† and Bulard.

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* De la peste et du typhus d'Orient. Paris 1840, p. 90.

† This indefatigable person has sent no fewer than 50 memoirs, at different

Subsequently to these researches, the professors of the medical school at Abouzabel (about four leagues from Cairo) personally attended upon 140 plague patients, of whom 38 died. Professor Perron has communicated a report of the observations and *post-mortem* examinations then made, in a memoir which he addressed to the Academy.

Drs. Aubert-Roche and Rigaud, attached to the great hospital at Alexandria, displayed no less courage and disinterestedness in their enquiries. The latter gentleman died of the plague, leaving behind him an account of 68 dissections which he had made of fatal cases.* The former has published an account of his observations, collected either by himself or in conjunction with his lamented colleague.

The conduct of M. Lesseps, the French consul-general at Alexandria, has been the theme of universal admiration. By his own example, he powerfully contributed to dissipate the exaggerated apprehensions of visiting and even touching plague-patients. His conduct towards Dr. Rigaud, up to the last moment of his friend's life, was a memorable instance of noble generosity.

Since 1835, the medical men resident in Egypt have continued their efforts to render our knowledge of the plague more and more complete. In 1837, an epidemic broke out at Adana, in the corps of the Egyptian army that then occupied Syria. In 1841, Damietta, Cairo, and a number of the towns or villages in the Delta were visited by the pestilence. It is also to be remembered that not a year has passed since the great epidemic of 1835, without a greater or less number of sporadic cases occurring every now and then in different parts of Lower Egypt.

But the plague has been studied of recent years in other countries besides Egypt. To confine our notice to modern works only, we may mention Dr. Brayer's *Neuf années à Constantinople;* Dr. Gosse's account of the plague in Greece during 1828 and 1829; and the reports of Dr. Morea on the plague of Noja in 1817, and of M. Hemso on that of Morocco in 1818.

M. de Segur du Peyron, although not a physician, has rendered great services to medicine by the publication of the three reports which he addressed, in the years 1834, 1839, and 1846, to the minister of commerce, and which contain a great mass of observations collected by him in the principal ports of the Mediterranean.

Lastly, the Academy has received a memoir on the plague and quarantine, published in 1845 by Dr. Moulon, physician of the lazaretto at Trieste; and also a printed report on the transmission of the plague and the yellow-fever, that was drawn up by a committee of the medical society of Marseilles, and unanimously approved of and adopted in August, 1845.

Besides the published works above enumerated, a number of very valu-

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times, on the subject of the plague to the French Academy. Many of these memoirs, written by able men who have had ample opportunities of studying the disease, well deserve to be published.

* In the very valuable pamphlet on Oriental Plague and Quarantines, published by Dr. Bowring in 1838, it is stated that Dr. Rigaud, after having been, during the most fearful crisis of the pestilence (1835), constantly engaged in visiting and assisting the living or in dissecting the dead, at length fell a sacrifice "just as the plague was ceasing, when its violence appeared wholly exhausted, and the season of its disappearance was about to arrive."

able manuscript documents have been submitted to the examination of the Commissioners.

Among these, we may mention the original papers respecting all the cases of plague that have occurred in the lazaretto of Marseilles since 1720, along with a letter and memoir from Dr. Robert, one of the physicians of this lazaretto;—the register kept in Egypt and Syria, during the years 1828, 1829 and 1830, by the plague commission, of which M. Pariset was the president;—the report, addressed in 1842 to the minister of commerce, by Dr. Delaporte of his mission to Constantinople, Smyrna, and Alexandria, for the purpose of studying the plague in these places;—a statistical statement of 506 epidemics of the plague drawn up by Dr. Rossi of Cairo, who, like Dr. Delaporte, nearly fell a sacrifice to an attack of the pestilence;—the statistic report of all the cases of plague observed in the lazaretto of Alexandria since 1835, by Dr. Grassi, who has been physician of that establishment since 1831;—a memoir on the plague in Persia by Dr. Lacheze;—one on the plague in Algeria, from the year 1552 down to 1819, by M. Berbrugger, corresponding member of the Institute, and conservator of the library and museum of Algiers;—a memoir on the contagiousness of the plague by MM. Pezzoni, Leval, and Marchand, members of the council of health of the Ottoman empire, dated June, 1842;—and lastly, a memoir on the antiquity and endemicity of the plague in the East, and especially in Egypt, by Dr. Daremberg, the learned librarian of the French Academy.

In addition to these numerous sources of information, the Minister of Foreign Affairs granted to M. Prus the privilege of consulting the dispatches of the French ambassadors and consuls in the Levant on all topics connected with his enquiries. The dispatches of M. Lesseps, (to whom we have already alluded,) during the frightful epidemic of 1835 in Egypt, were found to be especially valuable. The Minister of Marine also put all the official documents under his control at the free disposal of the Commissioners.

With the view of rendering their enquiry as complete and comprehensive as possible, the Commissioners invited to their meetings the attendance of medical men and others, who might feel inclined to give any verbal communication. In this way, they received much valuable and interesting matter.

The Report is divided into four parts or sections.

In the *first*, the following points are examined and determined:—the countries where the plague has been observed to become spontaneously developed;—the causes of spontaneous plague;—the disappearance of the plague, whenever these causes have ceased to exist;—the countries where the persistance of these causes renders the plague endemic, or at least makes the return of the spontaneous disease to be apprehended—and, lastly, the measures that are really and truly prophylactic against spontaneous plague.

In the *second* part, the three following questions are answered:—1. Has the plague always exhibited the characteristic features of epidemic diseases, whenever it has raged in Africa, Asia, and Europe? 2. What are the distinctive characters between epidemic and sporadic plague? 3. Does the plague spread after the manner of epidemic diseases; *i. e.* by the migration of certain atmospheric influences, and independently of the agency of those persons who are infected by it?

In the *third* part, the important question as to the transmissibility of the plague from one individual to another is examined. Is the disease transmissible by inoculation? Is it transmissible away from, as well as in, epidemic *foci* by immediate contact with the sick? by the contact of clothes, furniture, or merchandise? or by miasms exhaled from the bodies of the sick, and diffused through the atmosphere? This part closes with an examination of the following three questions. 1. Can persons affected with sporadic plague occasion *foci* of infection sufficiently active for the transmission of the disease? 2. Is the plague more or less readily transmissible, in proportion to the intensity of the epidemic; according as the disease is in its first, its second, or its third period; and, lastly, according to the organic susceptibilities of those who are exposed to the action of the pestilential miasm? 3. If the plague be transmissible away from epidemic *foci*, are there any grounds to apprehend that the importation of a few cases into France might occasion a pestilential epidemic?

In the *fourth* and last part, the question as to the ordinary or exceptional duration of the incubation of the plague is discussed. The general conclusions of the Report, and the application of these conclusions to the important subject of Quarantine are appended to this part.

FIRST PART.

CHAP. 1.—*What is the country, or what are the countries, where the Plague has been observed to arise spontaneously?*

In attempting to trace back the history of the plague, with the view of throwing some light upon this question, it would be little profitable to carry our researches beyond the sixth century of the Christian æra, as there is too good reason to believe that the terms λοιμος and *pestis* were previously used in a generic sense, to denote all epidemic diseases which caused great mortality. The "boils breaking forth with blains upon man and beast," recorded by Moses, need scarcely be alluded to. The famous plague of Athens, so graphically described by Thucydides, is supposed by the best authorities to have been a malignant form of typhus, complicated with a peculiar eruption and with gangrenous eschars. The Greek historian says that it was believed in his day that the pestilence had been imported from Egypt into the Piræus.

Whether we are to admit the genuineness of the passage from Rufus of Ephesus, a celebrated physician in the time of Trajan—discovered by Cardinal Angelo Mai, at Rome, in 1831, in the writings of Oribasius, who lived in the time of the Emperor Julian—is doubted by some learned enquirers; by M. Pariset, the accomplished secretary of the Academy, among the number. The passage indeed contains a remarkably accurate description of the characteristic symptoms of the plague,* and the writer refers to

* The following is one of several paragraphs that might be quoted:—

"A pestilential carbuncle is that which is accompanied with a severe inflammation, with acute pain, and delirium. In many of those who are affected with it, there occur also hard and painful bubos, and the patients soon die of these carbuncles. This is the case more especially with those who live in the neighborhood of marshes."

epidemics of the disease in Egypt, Syria, and Libya, mentioned by Dioscorides, Posidonius, and Dionysius, who (are supposed to have) lived two or three centuries before the Christian æra. It may be worthy of notice here that the writings of Cicero, Strabo, and Pliny afford evidence that Egypt was regarded, in their time, as a country that was fertile in the plague. There are allusions too in the works of Galen and Aretæus, not to mention Hippocrates, that would seem to indicate their acquaintance with malignant fevers accompanied with bubos and carbuncles.

But, without dwelling longer on the uncertain history of the plague, we shall at once come down to the year 542 of the Christian æra, when that terrible epidemic, the description of which by Procopius and Evagrius can leave no doubt as to the true nature of the disease, ravaged the city of Constantinople. From this period, the appellation has been very generally restricted to that form of fever that is accompanied with bubos, carbuncles, and petechiæ. If we are to believe that from the 6th to the 16th century the term "plague" has been properly applied, we are surely justified in assuming that, from the beginning of the 16th century—that is to say, subsequently to the establishment of lazarettos in Europe—this word has only been employed in its right acceptation.

In the 16th century, there was (as far as we know) but one epidemic of plague in Egypt, and we find no mention of any in Turkey in Asia, or in Syria: whereas, in the course of this century, there were no fewer than fourteen invasions of the pestilence in France, twelve in Germany, eleven in Italy, nine in Dalmatia, six in Turkey (in Europe), five in England, five in Spain, two in Portugal, two in Poland, two in Belgium, and one in Switzerland.

In the 17th century, we have the account of but two invasions in Egypt, and of not one in Turkey (in Asia) or in Syria; whereas there were nineteen in Germany, eleven in Italy, eleven in France, six in England, five in Russia, four in Turkey (in Europe), three in Spain, two in Holland, two in Switzerland, two in Denmark, one in Sweden and one in Poland.

It seems, therefore, impossible that any one, who will take the trouble of comparing these figures (always supposing that they can be depended upon—*Rev.*) should not be struck with this remarkable circumstance; to wit, that the plague has repeatedly and most destructively made its appearance in many points or localities in the world, more especially in Europe, at various epochs when either it did not exist, or was only very rare, in Egypt. If such has been the case, we are surely bound to admit that the disease has often arisen spontaneously in other countries, besides Egypt, Turkey, and Syria. This is the view which M. Littré has taken; for he observes (article *Peste* in the Dictionaire de Medecine) that "the plague was very frequent in Europe during the 16th and 17th centuries. Italy France, England, Holland and Germany were attacked by this pestilence; and Paris and London witnessed it spring up in the midst of them just as Cairo and Constantinople now do."

In the 18th century, epidemic plague occurred nineteen times in Egypt, seven times in Turkey in Europe, four times in Dalmatia, four times in Germany, thrice in Russia, thrice in Spain, twice in Poland, twice in Greece, once in Italy, once in Sweden, and once in France, viz. when Provence and Marseilles suffered so severely in the years 1720 and 1721.

In the course of the present century, the epidemic plague has broken out eight times in Egypt, six times in Turkey in Europe, thrice in Greece, twice in Syria, twice in Italy, twice in Russia, once in Turkey in Asia, once in Germany, once in Dalmatia, and once in Morocco.

The statistical data which we have given, more especially those which have reference to the 16th and 17th centuries, appear to prove most incontrovertibly that the plague has arisen spontaneously, at certain periods, in very many of the countries of Europe and Asia. Our convictions upon this point will be much strengthened, if they are found to be supported by facts observed in our own day.

Dr. Lacheze, in the account of his recent travels through Persia, informs us that the plague has been repeatedly observed to arise spontaneously in several of the cities of Asia Minor, and particularly at Erzeroum, situated near the northern source of the Euphrates and about five days' journey from Trebisond ; a statement which has been amply confirmed by the report of the Turkish council of health, that was established in the year 1838. The same remark may be made respecting Aleppo.

There are numerous facts also that seem to prove that the plague is apt to appear spontaneously upon the banks of the Danube, as it does on those of the Nile and the Euphrates.

The Russian army in 1828, while engaged in war with the Turks in Moldavia, Wallachia and Bulgaria, was attacked with a very malignant fever that was accompanied with bubos in the groins and axillæ. Dr. Witt, principal physician of this army, while he has acknowledged that the fever resembled in every respect the true plague, gave it, however, as his opinion that it must be distinguished from this disease, because it arose on the banks of the Danube, independently of any importation from abroad ! Dr. Schlegel, who had been sent by the Russian government, before the arrival of Dr. Witt in Wallachia, to determine the nature of the epidemic admitted that, although it showed great affinity to the plague, it differed from the latter in being attributable in that country to putrid emanations containing mephitic gas ! On the other hand, Professor Seidlitz of Petersburg did not hesitate to regard the fever as genuine oriental plague.* If it was really so—and surely there is no good reason to think otherwise—we have the authority of both Dr. Witt and Dr. Schlegel that the disease was truly endemic and of spontaneous origin in the localities where it prevailed.

Although the plague may therefore arise spontaneously in a number of different localities, it is no doubt true that, in recent times, Egypt, Syria, and Constantinople—more especially the first—have been the principal *foci* of the disease. It is right here to mention that, since the year 1839, it appears that there has been no case of plague observed in Constantinople. The board of health of that city attributes this exemption altogether to the quarantine measures, that have been adopted of late years. May

* This gentleman has shown that, whenever in times past the Russians have carried on war against the Turks on the banks of the Danube and on the coast of the Black Sea, their armies have almost invariably suffered from the plague. In consequence of this fact, and other considerations to be afterwards mentioned, he does not hesitate to affirm that, in these cases, the plague is to be viewed as only the worst form of the endemic fever of the country.

such be the truth! but let it not be forgotten that, before the terrible epidemic of 1812, not one case of the disease had occurred in that immense city for eight years; a fact that is proved by the registers of the French embassy at the Sublime Porte.

Syria also appears, according to the testimony of Mr. Lander the English Consul at the Dardanelles, and of M. Beclard the French Consul at Smyrna, to have been completely exempt from the plague since the same period (1839). Dr. Lasperanza, attached to the Constantinople board of health, informs us that, in consequence of various sanitary improvements that have of late years been introduced, the disease has ceased to be endemic in Jaffa, as well as in other parts of Syria. In the present day, it is almost exclusively from Egypt that the importation of the plague may be apprehended.

The general *conclusion* from all that has now been stated is that—

"The plague has been observed to arise spontaneously, not only in Egypt, Syria, and Turkey, but also in many other countries of Africa, Asia, and Europe."

CHAP. II.—*In countries where the spontaneous plague has been observed, can the development of the disease be reasonably attributed to any determinate hygienic conditions?*

To solve this question, the Commissioners examined with great care the history of the various localities in which the plague has arisen spontaneously, within the last fifty years. And first with respect to Egypt. Now the most competent observers assure us that there is nothing in the mere climate of this rich, and, in many respects, highly favoured country that will account for the generation of the pestilence; indeed, travellers have written in the most glowing terms of its beauty and salubrity. The year in Egypt may be divided into three periods or seasons. The *first* commences in August and ends with October; it is the period of the inundation of the Nile. The *second* comprises the next six months, from November to April; it is the season of the winter harvests, the ground being covered with trefoil, wheat, barley, flax, &c. The *third* begins in May, and terminates in August or September; it is the time for the cultivation of cotton, indigo, and rice. As we have already said, the natural climate of Egypt is on the whole a very salubrious one. Its drawbacks are but few; the chief being the coolness and humidity of the nights, the frequent and rapid variations of temperature in the day, the rains and fogs of the Delta during the winter months, the great heat and excessive dust in summer, and, lastly, the singular effects of the south wind, the *Kamsin*, upon the living body. Whence then comes the pestiferous atmosphere of some parts of this land? The answer is ready; man himself has given it birth; the inhabitant of the Delta, says M. Hamont,* who long resided in Egypt, has prepared the causes of his own destruction. The destitution, filth, and misery of the poor inhabitants are extreme. Their wretched hovels are so horribly disgusting as almost to defy description; they are not only surrounded by, but are actually receptacles of, heaps of ordure and putrid matters. Not

* *Destruction de la peste et des quarantaines.* (*Bulletin de l'Academie Royale de Medecine*,—Paris, 1844, t. x. p. 40.)

unfrequently the dead are buried immediately under the mud floors of these dwellings of the living; and many of the graves in the cemeteries (which are always within the villages), being left open, are continually exhaling a stench that is utterly intolerable to any stranger. Then, again, the food of the Fellah is always of the worst description, and often too of the most scanty supply. Rotten cheese, decayed vegetables, semi-putrid flesh, or fish; such are the articles that he lives upon. The very water that he drinks is filthy and impure. And then think of his mental and moral condition; the brutish degradation of all his faculties and affections, his hopeless servitude, his blank unmitigated wretchedness.

The hygienic state of the cities and larger towns in Egypt is not much better than that of the villages. Cairo, with its 200,000 inhabitants, is a very hot-bed of the most disgusting and pestiferous impurities. From the canal, which traverses it, there is constantly steaming forth a cloud of intolerable offensiveness; and yet this is the supply of water for the use of its people! There are no fewer than 35 cemeteries, of which 25 are within its walls. In the Copt quarter of the town, the dead are buried under the floors of the houses; and nothing but a few boards separate the living from the putrid bodies of the deceased. From 80 to 90 corpses have been known to be huddled together in these horrible *sub-domal* receptacles. Can we, therefore, wonder that Cairo should be a generating focus of pestilential disease?

That the circumstances now mentioned must tend to promote the development, and aggravate the intensity, of the plague, will be disputed by none; but then the question comes to be, are they sufficient to produce or originate it? This thing is certain, that the disease has never been known to appear spontaneously in Egypt, except in places and seasons when these most pernicious agencies were at work.

The plague does not arise in Upper Egypt, Nubia, and Abyssinia; nor does it ever extend above the first cataract of the Nile. The good quality of the soil, the ready efflux of the waters, the small number of the inhabitants, and the strong currents and agitations of the atmosphere appear entirely to counteract the morbid influence of the mode of life followed by the inhabitants.*

We are informed by Gaetani Bey†—first physician to Mehemet Ali, and who has resided in Egypt for the last 25 years—that the plague never extends beyond Assuan, in consequence of the difference in the situation, heat, dryness, and nature of the soil; whereas it readily finds its way into the localities where there is much stagnant water. It is for this reason that Bagdad and Bussorah are in the present day subject to invasions of the pestilence, from which they were formerly exempt when effective police regulations were in force in these towns.

The seasons exert a no less marked influence on the development of the plague. The dry heat of what is called in Egypt the second summer, the prevalence of the northerly wind that usually sets in about the summer solstice, and the first dews that commence about this time, change alike the condition of the atmosphere and the organic aptitudes or susceptibilities: the pestilence ceases.

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* Pariset, *Causes de la peste.*—Paris, 1837.

† *Sulla peste che afflisse l'Egitto, l'anno* 1835. *Napoli*, 1841.

What has been now said respecting the artificial insalubrity of Egypt, arising from man's own negligence and vice, is nearly quite as applicable to Constantinople as it is to Cairo. The filth of some of its environs is altogether intolerable and disgusting. It is usually in the month of July, when the north or tramontane wind ceases and is succeeded by a southerly sirocco, that the pestilence makes its first appearance. As a matter of course, the putrefaction of all organic matters goes on much more actively at that season, in consequence of the high heat on the one hand, and the moist relaxing influence of the wind on the other. The localities, that are first attacked, are those which are chiefly occupied by the poor Greeks and Jews: hence the village of San Dimitri is usually the place where the earliest cases are observed.

We need scarcely say that, if Constantinople be bad, Erzeroum is much worse, in everything that respects hygienic salubrity. Fortunately the frequent severity of the winter season there, as well as the high winds that prevail in Armenia, tend much to attenuate the existing causes of the plague.

If from the Euphrates we pass to the Danube, we shall find the same causes of endemic insalubrity prevailing in those localities, where the pestilence has been known to arise. The poorer classes in Moldavia and Wallachia live in the greatest misery and filth. After the heats of summer, almost all the prevailing diseases assume a character of marked gravity. Malignant intermittent fevers are always more or less prevalent in autumn; these generally precede the appearance of the plague, which in these countries is usually only sporadic. Professor Seidlitz has endeavoured, as we have already seen, to establish the intimate connection between these two forms of febrile disease.

Dr. Mirolanof, who treated the plague at Achial in 1828, says that "the soldiers and officers who had the intermittent fever, were affected with bubos and carbuncles. In the month of September the plague showed itself, especially in those who were convalescent from agues, and assumed the form of a tertian fever. The bubos appeared after the first or second paroxysm."

Dr. Rinx, who was at Adrianople during the whole course of the epidemic, remarks of the third degree of the epidemic, that "the least severe degree of the plague so much resembled an intermittent fever, that it was scarcely possible to distinguish the one from the other, before the appearance of the bones."

From all these various facts, it is abundantly obvious that the hygienic condition of the four distinct localities in which the plague has of recent years broke out spontaneously, is very nearly the same. It is a circumstance, too, of no trifling import, that wherever the producing causes of the disease are most abundant and concentrated, there it is always most severe and most readily propagable. The form most dreaded is that which appears in Egypt; next comes that of Constantinople, and after this that of Erzeroum; while that of the Danube, which has hitherto been generally regarded as of Constantinople growth, has not yet been sufficiently studied to enable us to decide respecting its relative severity.

Is it not also a remarkable fact that the four geographical points or localities now mentioned, are all subject to malignant intermittent and other

fevers? Are we to believe, with MM. Begin and Boudin, that the plague belongs to the family of marsh fevers? There are many circumstances certainly which seem to militate in favour of this opinion. Without dwelling on the geographical condition of Syria and other plague countries in the present day, we well know how prevalent intermittent fevers were in London during the 17th century, when that city was occasionally visited by the oriental pestilence. The readers of Sydenham are well acquainted with this fact.

The outbreak of the plague has not unfrequently followed upon wars, famines, and other wasting calamities; and, on the other hand, its ravages have invariably been observed to become less frequent and less desolating in proportion as the condition of the inhabitants of the affected countries, in point of civilization and comfort, has improved. The researches of MM. Papon* and Aubert-Rochet have satisfactorily proved the truth of this.

The general *conclusion* to which we arrive is that,

"In all countries where the spontaneous plague has been observed, its development may be reasonably attributed to certain determinate conditions acting upon a large portion of the inhabitants. The principal of these conditions are, residence upon marshy alluvial soils near the Mediterranean or near certain rivers, as the Nile, Euphrates, and Danube; the dwellings being low, crowded, and badly ventilated; a warm moist atmosphere; the action of putrescent animal and vegetable matters, unwholesome and insufficient food; and great physical and moral wretchedness."

CHAP. III.—*If the preceding statements be correct, the plague must be endemic in Lower Egypt, where all the conditions of insalubrity which we have pointed out are constantly present: Is such the case?*

All the most accurate and enlightened observers agree in answering this question in the affirmative. Not a year passes without the plague showing itself at Alexandria in a *sporadic* form; generally between the months of November and the following June. This fact cannot be disputed; it is incontrovertibly proved by the reports of the Council of Health that was established in that city 12 years ago. The same thing holds true with respect to Cairo, and other places in Lower Egypt; the testimony of Gaetani-Bey is unqualified upon this point. The *epidemic* plague, that which has so fearfully mowed down the Egyptian population, is happily more rare; although incomparably more frequent than in any other country of the world.‡ The number of epidemic invasions of the pestilence in Egypt from the year 1695 to 1834, have been (according to one statement, drawn up by an Arab cheik) 19 in all. The mortality caused by some of the invasions has been truly frightful. If we take account of those only

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* *De la peste et des epoques memorables de ce fleau.* Paris, an. viii. 2 vol. 8vo.

† *De la prophylaxie de la peste.* Paris, 1843.

‡ According to the calculations of M. Hamont, the population of Egypt, which was once, it is believed, upwards of ten millions, and was fully three millions at the commencement of the present century, does not now exceed a million and a half.

which have been very destructive of life, we find that Egypt has been visited with the scourge about once in every ten years.

Conclusion.—"All the producing causes of the plague being found united in Lower Egypt, the disease is endemic in that country, where it is seen every year in the sporadic, and about every tenth year in the epidemic, form."

The object of the next Chapter, the IVth, is to show that Egypt was exempt from pestilential epidemics in ancient times, and until about the commencement of the seventh century of the Christian æra. Certain it is, that we have no very authentic account of any wide-spread and destructive invasion of the plague at an earlier period. The cases, alluded to by Rufus, appear to have been only *sporadic;* at all events, this writer makes no distinct mention of any epidemic pestilence having ever prevailed in Egypt, as he does of one that ravaged Libya upwards of 300 years before the birth of Christ.

That Egypt was once a remarkably healthy country is expressly attested by Herodotus. The land was rich and very populous, abounding in all the necessaries of life, and the inhabitants were prosperous, enlightened, and happy. The custom of embalming the dead, not human beings only but animals of all sorts, may have had a salutary influence, by withdrawing so much corruptible matter from putrefaction and decay.* This "salutary practice" (of embalming) was abolished in A.D. 356.† Subsequently to this period, the ignorance and fanaticism of the Mussulmen have brought on that frightful state of moral degradation and physical wretchedness of which we have spoken in a preceding chapter. Is this lamentable state of things always to last, to the disgrace of the country and the injury of the world? There cannot be a reasonable doubt but that, if proper sanitary regulations could be established and duly executed in Egypt, the pestilence might be extirpated, and Egypt rendered as healthy as it was in days of yore. Mehemet Ali is well aware of the truth of this. His convictions on this point are so positive, and already he has acted so well in the right course, that Gaetani-Bey—who accompanies him twice a year in his tours of inspection across the Delta, from Alexandria to Cairo—has not hesitated to declare that, if the Viceroy was not thwarted in the execution of his plans, this great and desirable end might be accomplished.‡

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* There seem to be some inconsistencies between a few of the statements in this chapter and those that have been already made, respecting the existence of the plague in Egypt in ancient times. We may remark also that the testimony of Herodotus, respecting the salubrity of ancient Egypt, is said to be at variance with that of of other authors more worthy of credit. M. Daremberg informs us that Hæser (*Recherches historico-pathologiques sur les maladies epidemiques*) has collected together a number of texts to prove the unhealthiness of Egypt in ancient times. Compare also Lorinser, *die Pest* im *Orient*.

† M. Prus suggests, among other hygienic reforms necessary in modern Egypt, the re-establishment of the practice of embalming (!) or of some other equivalent method of counteracting the evils of animal putrefaction in that country.

‡ Sir W. Pym, in a letter addressed by him in Jan. 1845 to the Board of Trade, acquaints us that Mehemet Ali, on being informed that there was a very short quarantine in England against Egypt, replied: "There ought to be no quarantine, it is our own fault. *We must get rid of the plague!*"

The question proposed in Chapter V. is to the effect whether the present condition of Syria, of Turkey in Europe and Asia, and of the Barbary States, has become so much changed or ameliorated, since the time when pestilential epidemics have broken out in them, as to justify any rational expectation that such invasions might not recur. The answer, as might be anticipated, is decidedly in the negative. Wherever the Ottoman dominion has prevailed, civilisation and social improvements have retrograded rather than advanced. We have seen, indeed, that the recently instituted board of health at Constantinople has attributed the exemption of that metropolis from an invasion of the plague for some years past exclusively and entirely to the establishment of lazarettos and quarantine restrictions there; but we must not be too ready to yield our unhesitating assent to this opinion.

It is not necessary to adduce any details to show that the sanitary condition of such places as Erzeroum and the surrounding villages, of Tunis, Tripoli, &c., has not at all improved of late years, so that they should be less likely to be visited by the pestilence than they may have been hitherto. With respect to Algeria, (Chap. VI.)—which seems to have been less frequently the scene of spontaneous epidemics of the plague than any of the other Barbary States, in consequence probably of most of the towns and villages being built upon the slopes of hills, and neither crowded together nor over-peopled—there is good reason to anticipate that, under its present administration, it may become as seldom the theatre of the pestilence as almost any of the countries of Europe.

The answer to the question, proposed in Chap. VII.—*What are the means that should be employed to prevent the development of spontaneous plague?*—must be sufficiently obvious from what has been already said respecting the causes which promote, if they do not induce, the development of the disease in Egypt and elsewhere. M. Villermé has with great ability discussed the general question as to the origin and diffusion of epidemic diseases, and has very satisfactorily shown that they invariably become less frequent and less destructive in proportion as countries pass from the miseries and degradation of barbarism to the social comforts of civilised life. Dr. Aubert-Roche also has with much care examined this subject, more especially in reference to the plague; and he comes to the same conclusion. In all times, and in all places, this disease has disappeared before civilisation; it has returned with a country's decline and barbarism. Everywhere the same causes have produced the same effects.

SECOND PART.

CHAP. I.—*Has the plague always exhibited the principal characters of epidemic diseases, when it has raged with violence in Africa, Asia, or Europe?*

The characteristic features of epidemic diseases are these:—1. They generally manifest in their progress three distinct periods, of commencement, persistance or status, and decline. These periods often display neither the same symptoms, the same lesions, nor the same gravity. 2. During the prevalence of an epidemic, other diseases are less numerous than usual, and they receive the stamp or impression of the prevailing affection. 3.

When an epidemic disease prevails, even those persons who retain their health generally feel its morbific influence more or less. 4. Epidemic diseases not unfrequently return and cease at the same season (of the year); and they have usually about the same duration. 5. An epidemic disease is often preceded by other affections, more or less severe, and more or less widely diffused; these seem to be in some way its precursors.

Now the plague exhibits each and all of these features in a striking manner. Its severity or malignancy is usually most intense on its outbreak, and for the first few weeks afterwards. Pugnet says that, towards the end of the epidemic at Cairo, in the year 1800, almost every patient recovered notwithstanding the most opposite methods of treatment, whereas very few indeed recovered upon its first outbreak.* Not to accumulate authorities, we may state that Clot-Bey remarks that, "when an epidemic commences, almost all who are attacked with it perish. During the first period, death occurs within 24 or 48 hours after the invasion; in the second, on the 4th or 5th day, or it may be not till the 14th or 20th. There are scarcely any fatal cases in the third period;" the pestilence having by this time lost its malignancy.†

It must be obvious from this circumstance, how cautious medical men should be in estimating the value of any remedial means in the treatment of such a disease as the plague, and how important it is to pay great attention to the period of the epidemic visitation when these means have been employed. This is a great practical truth, which is far too little attended to in the present day. "At the commencement of the epidemic (1841)," says Dr. Penay, surgeon-major of a cavalry regiment in the Egyptian army, "I lost almost every patient, in spite of my best exertions. Subsequently several got well without my being able to determine what line of treatment seemed to be of decided benefit. During the decline of the epidemic, nearly all my patients recovered, and the greater number without any other remedy except local applications to the bubos and carbuncles." The following extract from a report of M. Masserano, one of the members of the Egpytian council of health, is highly illustrative of the same subject.

"While the plague was at its height, almost all the persons who were attacked sunk at the end of four and twenty hours; and such was the violence of the epidemy in some of these cases, that the patients died suddenly while engaged in their employments, as if they had been struck with lightning. The pestilential

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* *Memoire sur les fievres de mauvais caractère du Levant et des Antilles.* Paris, 1804.

† The following observations of Sydenham may be aptly quoted here:—

Observare insuper est quod, sicuti epidemicorum quilibet in subjecto particulari suas habet periodos (augmenti scilicet, status, et declinationis) ita etiam constitutio generalis quæcunque, quæ huic alterive morbo epidemice producendo favet, pro ratione temporis quo dominatur, suas etiam periodos habet, quatenus scilicet indies magis et magis epidemice grassatur, donec ακμην attigerit suam, atque exinde iisdem fere gradibus decrescat, donec tandem penitus exoluerit, alteri constitutioni locum cedens. Symptomatum enim quod attinet vehementiam, atrociora sunt omnia ubi primum se ostendit; quæ quidem paulatim mitescunt, et in constitutionis catastrophe tam sunt benigna atque ευφορητα quam patitur morbi natura in quo fundantur.— Observ. Med., sect. iv.

characters in the middle, and towards the end, of the epidemy were much less intense. The acute cerebral congestions and complete state of prostration were no longer observed; and petechiæ were of rare occurrence. The sick were distressed with restlessness and weariness; exhaustion and headache threw them into a state of stupor. They experienced more or less severe glandular pains, shooting uneasiness in those parts where bubos were expected to appear: these bubos passed readily into suppuration. When the epidemy approached its close, I saw many persons attacked with bubos, without discontinuing their occupations. Two of my servants, among others, were attacked with the disease in a mild form; they pursued their employment without saying any thing to me. At the time when the disease was most intense, we remarked that, out of 22 persons attacked, 10 died; whereas, towards the end, out of 60 seizures only two proved fatal."

What has now been said will abundantly show how truly the plague exhibits the *first* of the characteristic qualities of Epidemic Diseases. We proceed to examine the *second* one which we enumerated; viz. how far are other diseases, that may exist during the prevalence of the pestilence, influenced and modified by it. Diemerbrock, writing of the plague at Nimeguen in 1635–6, uses these words: *vix ullus morbus peste incomitatus fuit.* Pugnet says that "the plague stamps with its own peculiar character all other co-existing diseases." A proof and, at the same time, an effect of this decided influence of the pestilential constitution upon intercurrent diseases is the circumstance that these resume their own proper physiognomy (a remark made three hundred years ago by Prosper Alpinus*), as soon as the pestilence subsides. It would be easy to multiply authorities upon this subject, if it were necessary.†

All medical men, who have had an opportunity of studying the plague in Egypt or elsewhere, have remarked that, during the prevalence of an epidemic, those persons, who have already had an attack of the disease, usually feel pain or uneasiness in the scars of their old bubos and carbuncles, without their general health being much affected; and moreover that all those, who have escaped the disease and remain tolerably well, have still nevertheless experienced a certain feeling of *malaise;*—and even a slight degree of tenderness of the lymphatic glands in the groins and axillæ. This is the case equally with those who are in strict quarantine, or who enjoy free pratique. Dr. Delong, in his account of the Epidemic at Cairo in 1841, observes that, it may be fairly said that the entire population had the plague in its first and mildest degree. So impressionable and sensitive, are those, who have once had the plague, to a pestiferous condition of the atmosphere that, it has been supposed, they can generally predict the approach of the disease by the shooting pains and uneasiness in their old bubonic and carbuncular cicatrices. It is possible that in this way we may find out if a pestilential constitution (of atmosphere) exists, or is impending.

Does the plague exhibit the *fourth* character assigned to epidemic mala-

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* *Medicina Ægyptonem.* Lib. 1, cap. 16.

† Sydenham has remarked, in his description of the plague of London, that the ordinary endemic fevers of a country are apt to retain, for a season or two, after a severe attack of the pestilence, some of its peculiar and characteristic features or symptoms; *pestilenti aeris diathesi etiamnum ex parte perseverante, nec dum in aliam salubriorem penitus immutatâ.*

dies? viz. that of having in general nearly the same duration in different countries, and of appearing and disappearing at epochs which may be determined beforehand?

M. Levison, the Russian Vice-Consul at Alexandria, has drawn up the following statement from the data supplied to him by the Cheik Ibrahim-Bassi:

"The most intense pestilential epidemics in Egypt are those which, commencing *sourdement* in November, have reached their acme about the end of February or during March. On the other hand, those which have not displayed great violence, have always made their appearance in the course of this last month. In the month of June, both one and the other have often ceased.

"The malignant plagues of Egypt have usually lasted about four months, whereas the milder ones have in general not exceeded two months, or two months and a half."

It is a remark as old as the time of Prosper Alpinus, and one which is amply confirmed by the observations of subsequent writers, that the disease in that country almost invariably ceases in the month of June.

At Constantinople, epidemic plague habitually begins in the first or second week of July—during the great summer heats and the prevalence of southerly winds and thick fogs—after or before the arrival of the convoy of merchant ships from Egypt and other places, and usually ceases towards the end of the year. The great plague of 1812, which had been mild up to the end of August, became very malignant in September, carrying off in little more than three months no fewer than 160,000 persons. It entirely ceased by the end of December. At Smyrna, the pestilence generally reaches its height in May, and ceases about the middle of August.

The *fifth* characteristic feature of epidemic diseases—that of being usually preceded by certain precursory maladies—belongs without doubt to the plague. Its outbreaks have been repeatedly observed to be preceded by bad forms of intermittent and continued fevers. "During the winter of 1816-17, there prevailed in this place," writes M. Berbrugger, the learned librarian of Algiers, "a very fatal epidemic, that was termed in the bills of health a malignant fever. This has been remarked to be a usual precursory sign of an outbreak of the plague itself, when this reappears after a long interval of time." Many analogous circumstances might be quoted. That Typhus not unfrequently precedes, and coexists with, the regular plague is admitted by most of the medical men who have resided for some years in Egypt. Dr. Delong, who lives at Cairo, has made the remark, of the epidemic of 1841, that the plague often commenced under an intermittent form, intermittents having been prevalent for some time; and that quinine occasionally seemed to arrest the progress of the malady.

Such being the case respecting the frequent precedence of other epidemic affections to the outbreaks of the plague, it must be obvious that medical men may readily fall into a seeming error, if they happen to be consulted respecting the nature of an existing disease, before the proper pestilence has fairly manifested itself. Hence then the necessity of their giving a guarded opinion, whenever there are grounds to believe that a pestiferous state of the atmosphere is impending. Moreover, at the commencement of an epidemic, there are neither bubos, carbuncles, nor petechiæ in most

of the cases. Gaetani-Bey has pointed out a very useful diagnostic sign to direct the medical observer under such circumstances. The lymphatic glands, internal as well as external, should be most attentively examined; and if the patient has died of the plague, one or more of these glands will invariably be found to be enlarged and more vascular than usual. The dissections made at Abouzabel in 1835, by the medical men who did not then know the opinion of Gaetani-Bey, amply confirmed the justice of his remark.

The facts and observations adduced in this chapter, lead distinctly to the *conclusion* that "the plague combines in a very marked degree the principal characters of epidemic diseases."

We shall now briefly look at the causes of pestilential plague, considered exclusively in this point of view. These causes, like those of all epidemic diseases, are of two kinds. The first relate to the soil and the atmosphere; the second to the physical and moral condition of the inhabitants.

When Dupuytren enquired of the young Egyptian students who had been brought by Clot-Bey to Paris for medical education, what was the opinion of the most enlightened men in Egypt respecting the origin of the plague, the answer they gave was, "la peste vient de la terre." All that is conveyed by such an expression is merely that a humid and marshy soil, more or less covered with decaying vegetable and animal matters, is a powerful cause of the alteration of the atmosphere, and consequently of the disease. Now, nothing can better serve to show the importance of the conditions of the soil, in reference to the production of the plague, than the comparing together of two localities in the same country, inhabited by the same people, and governed by the same laws and customs, in one of which the disease is endemic, while the other remains entirely exempt from its attacks, even although the infected may die within its walls.

"Fayoum is elevated above the level of the sea; Damietta borders upon the shore. At Damietta, the air is hot and damp; at Fayoum, it is hot, but dry. Fayoum is free from marshes; Damietta is surrounded with ponds of fresh and salt water. While at Damietta the cemeteries are in the town itself; at Fayoum, they are at a distance from the dwellings. Here, the water, although not very pure, may be drunk without inconvenience, owing to the quantity of nitre it contains; at Damietta, the fresh water is either mixed with sea-water, or it is rendered impure by excrementitious products, and by animal and vegetable matter in a state of putrefaction. Fayoum is surrounded by the desert of Lybia; Damietta is enclosed by rice-fields, and situated in front of the pestiferous Delta."

Great atmospheric vicissitudes have also a decided influence on the development and progress of epidemic plague. Larrey, Pugnet, and all other physicians who have seen the disease in Egypt, agree that its attacks are more frequent, and its mortality greater, when the air is warm and moist, and when the weather has been stormy. At Constantinople, the same causes produce the same effects. We shall not enlarge upon this subject; but only add that it has been too generally supposed that it is to some causes, either actually or recently existing in the condition of the soil, the atmosphere, or the kind of food, that we must refer the morbific effects observed. And yet, as remarked, with his accustomed sagacity, by Baron A. Humboldt, the most favorable cause for the development of epidemic

diseases is to be found in a uniform and long-continued type of meteorological phenomena. For example, in the case of the plague, it is after a lengthened duration of the same temperature and of the same winds that the pestilence, in an epidemic form, has been observed to break out in Egypt, Syria, and at Constantinople. It may be readily believed that when a population has lived for a length of time in the same conditions of climate, atmosphere, alimentation, &c., the system of each individual becomes profoundly modified in the same manner, and may be disposed to receive, or even to develope spontaneously, the same disease. Perhaps it is in this way that we may account for what has been very positively asserted to be the case by some authors, but denied by others, viz. that persons, who have been long exposed to the same physical influences, may become affected with the same disease at a given period, although they are then far distant from each other.

The action of epidemic diseases is observed to vary much in point of degree or intensity in different races of mankind, when exposed to the same morbific influence. No fact has been more clearly proved than the very peculiar predisposition of negroes to contract the plague. To Dr. Aubert-Roche we are indebted for the following table of the relative mortality in the different races, during the great plague at Alexandria in 1835:

Negroes and Nubians lost . .	1528	out of	1800 =	84 per cent.
Malays	367	"	600 =	61 "
Arabs, not soldiers	10,936	"	20,000 =	55 "

The Negroes, Nubians, and Arabs were all living in nearly the same hygienic conditions, and were all in free pratique. With respect to the other residents in Alexandria, our conclusions must be more uncertain, in consequence of the very great difference in point of hygienic condition, isolation, &c., enjoyed by different classes of the population. Here, however, are Dr. Roche's calculations:—

Greeks	lost 257	in	1800 =	14 per cent.
Jews, Armenians, and Copts . . .	482	"	4000 =	12 "
Turks	678	"	6000 =	11 "
Italians and others from the South of Europe	118	"	1600 =	7 "
French, English Russians & Germans	52	"	1000 =	5 "

These figures carry with them their own signification. It is scarcely necessary to say that the liability to the attacks of the pestilence among all classes of the population, native or stranger, is almost uniformly observed to be inversely proportionate to their cleanliness, good living, and general comfort. An instructive illustration of the truth of this is afforded by Dr. Roche.

"On the banks of the canal, which leads from Alexandria to the Nile, lies a property belonging to the Greek consul, M. Tortizza, who received it as a present from the viceroy. The fellahs who work upon this property being better treated and better fed than the fellahs of the surrounding villages, only lost, during the epidemic of 1835, 12 out of 400; while their neighbours, placed in the same conditions with respect to atmospheric influences and free communications, lost one half of their number."

Having most satisfactorily shown that the Plague must be placed in the

first rank of epidemic diseases, M. Prus makes the following general reflections upon the subject:—

"The epidemicity of the plague is in truth the fundamental fact of its history, that which most merits the attention of the physician, and which can alone make him comprehend a number of points which, without taking it into account, remain in complete obscurity. The certainty that the plague is a disease which is epidemic in a marked degree, will suggest another consideration to the mind of the physician. It will furnish him with the means sometimes of preventing, and always of diminishing, the ravages of the pestilence. If the existence of epidemic *foci* of plague is satisfactorily demonstrated, things will not affect in the same manner those who remain in or who come into these *foci*, as they do those who are placed, or who remove themselves beyond their influence.

"Every person remaining in an epidemic focus of plague, is exposed to contract this disease. Numerous and authentic facts, observed in Egypt during the years 1835 and 1841, have proved that the most complete isolation and the most severe quarantine do not always preserve those who submit to them. The same remark had been made in as positive a manner at Marseilles and at Toulon, at the time of the plague of 1720.

"It requires sometimes but a very short period to be passed in an epidemic focus to become affected with the plague. The professors of the school of medicine at Abouzabel,—which, in 1835, was not attacked by the epidemic influence for more than a month after the capital was ravaged by the disease,—have seen inhabitants of that place, who have only remained a few hours at Cairo, return infected.

"Now, what will happen to persons in health, or already affected with the plague, who shall remove from or be taken beyond the epidemic focus? Before answering this question, we must receive it as a fact recognized by science that, in the most wide-spread and severe pestilential epidemics, experience has shown that all the localities of the same country have not been subject at the same period to the epidemic influence. It has been stated a hundred times that, by the side of a town ravaged by the plague, other towns in free communication with it continued exempt from the disease. Nay more, plague-patients out of infected towns have come either to die or to be cured in localities where the epidemic influence did not prevail, without the disease having spread. We shall find numerous examples in support of these two propositions, both in the works of modern writers on the plague, and in the documents annexed to this report. Observation has also taught us that it is often tolerably easy to determine the limits of the epidemic focus. This may be circumscribed within the limits of a single town, as Pugnet remarked at Damietta at the time of the plague of 1799, or as was seen in London in 1665, notwithstanding that in both instances the communications with the neighboring towns remained perfectly free.

"This being established, we may assert, in answer to the question stated above, that, when a population is struck with a pestilential epidemic, persons, whose duties and interests do not require them to remain in the midst of the epidemic focus, will escape the danger by withdrawing from the infected district.

"In 1835, when the epidemic constitution prevailed at Cairo, Gaetani-Bey advised that the 22,000 of the soldiers on active service, composing the garrison, should be sent some leagues from the city, and be encamped under tents in a dry and airy situation, leaving only 2000 invalids for the service of the city. The plague did not commit any ravages among the active troops, whereas it raged among the 2000 invalids as it did among the rest of the population."

Some time before this, Clot-Bey had given a similar advice respecting the fleet which was in the harbour of Alexandria. Although put and kept in severe quarantine, the pestilence made its appearance on board some of

the ships, while they remained exposed to the epidemic influence. Not a single case occurred, when the fleet was withdrawn from the focus.

In 1813, Sir Thomas Maitland, the Governor of Malta, finding it impossible, in spite of the most severe measures, to extinguish the plague which then prevailed at La Valetta, took the resolution to have barracks constructed fairly out of the city, and obliged the population forthwith to occupy them. From that moment the plague entirely ceased.

Dr. Masserano has related a similar instance with respect to his regiment in garrison at Damietta in 1641: removal to a healthy spot at once put a stop to the disease.

The enlightened portion of the inhabitants of Cairo and Alexandria has already begun to discover the inutility of quarantine within their own dwellings, and now trusts for safety only to removal beyond the sphere of infection. The Persians have long acted upon this principle, and have no doubt found its advantage.*

Removal from an epidemic focus has been found to be most useful, not only in preserving the unattacked from the pestilence, but also in promoting the recovery of those who have caught it. On this latter point, Dr. Delong observes in reference to the plague of 1841:

"When I had the good fortune to be called at the onset of the disease, I instantly ordered a change of abode; whenever it was possible, I caused my patients to be removed to situations that were elevated, dry and airy. The disease then almost always assumed a more favourable appearance, and the morbid phenomena were found less to resist the combined action of nature and of a sound method of treatment."

A similar remark was made by the medical men at Abouzabel in 1835, and it has been repeated by M. Penay in the history of his patients in 1841. The *conclusion*, to be drawn from the numerous facts and observations that have been adduced in this chapter, is surely that,

"Whenever the plague has raged with violence in Africa, Asia and Europe, it has always exhibited the principal characters of epidemic diseases."

CHAP. II.—*What are the differential characters between Epidemic and Sporadic Plague?*

The medical men residing in Egypt, as well as those at Smyrna and Constantinople, agree, almost without exception, that the sporadic form of the disease is not transmissible;† whereas, with respect to the epidemic

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* Lacheze, *Memoire sur la peste en Perse.*

† The testimony of Dr. Laidlaw, who has resided so long in Egypt, and seen so much of the disease, is very strong upon this point:

"I have no hesitation whatever," says he, "in expressing my decided conviction that, unless the state of the atmosphere is favourable to the spread of the disorder, as is undoubtedly the case during the epidemic, there is no danger whatever from these causes, that they were purely accidental, and that it is impossible to produce by them the spread of the disorder. I have never seen a case of plague occurring sporadically where any person about the patient or in contact with him was attacked; and I cannot find any one that has *seen* one, although it is talked of among the Levantines as a common occurrence."—DR. *Bowring's Observations.*

form, many of them hold the opposite opinion. This is, indeed, just what might have been expected, for it is in exact accordance with the views entertained by most observers on other analogous diseases (Dysentery for example), which occur at one time sporadically, and at another time epidemically: in the former case, the malady is not transmissible; in the latter, it is often so in a very high degree.

If we admit the accuracy of this opinion, it will be obvious how important it must often be for a medical man to determine if a case of plague be simply sporadic, or if it be connected with a pestiferous constitution of the atmosphere. As a matter of course, this point cannot be fairly determined by a mere summary or off-hand enquiry; things must be carefully watched for some time, before a decided opinion is given. If the disease be limited to a few isolated cases, and if these occur only in the localities where the pestilence arises spontaneously, there will be reason to believe that its type is merely sporadic. The disease too is usually less malignant in this than in the epidemic form. The sporadic plague does not exhibit in its course the three regular periods of commencement, persistance, and decline; nor is it preceded by other epidemic disorders. Moreover, other maladies are not less numerous than usual, nor do they display anything of the prevailing pestilential stamp or impression: persons in health do not experience the effects of an atmospheric influence acting in an especial manner on the lymphatic system; and lastly, whereas the epidemic form of the disease commences between November and February, and ceases about the end of June, the sporadic form is observed in every month of the year. The *conclusion* is therefore very fair and obvious that "Sporadic differs in some very important respects from Epidemic plague."

CHAP. III.—*Does the plague extend, like most epidemic diseases, by the migration of certain atmospheric influences, and independently of the agency of the sick who are affected with it?*

Whoever examines with attention and candour the histories of many epidemics of the plague, can scarcely fail to observe that the disease has often broke out about the same time in a number of different localities, distant from and having no intercourse whatsoever with each other. Always originating in unhealthy spots under the influence of those causes which have been already mentioned, the epidemic pestilence may be either confined within the circuit of a single town or city, although this remains in free communication with the surrounding district, as occurred at Damietta in the year 1799; or else it may become diffused over a number of countries, as in the formidable epidemics of 542 and 1348. It is scarcely necessary to adduce further instances to prove that the localities and districts, immediately adjoining to an infected spot, often remain quite exempt from the disease. Not unfrequently several towns are attacked about the same time, the intermediate villages remaining quite free; at other times, the pestilence advances in a more regular manner, attacking a number of places "de proche en proche" and in succession.

Clot-Bey and Dr. Roche are of opinion that the plague may traverse seas, and pass from one continent to another—from Alexandria to Marseilles, for example—through the medium of the atmosphere alone. On the other hand, that it may meet with insurmountable obstructions not

far from the spot where it has arisen, would seem to be the case from the well known fact that the plague of Lower Egypt never passed beyond the first Cataract of the Nile. In some malignant epidemics, indeed, the disease spread into districts that are very generally spared. This has been observed at certain periods in Upper Egypt and the Hedjaz.

It appears that the plague never extends to a great elevation above the level of the sea. There is a village about five leagues from Constantinople, situated on the mountain of Alem-Daghe at an elevation of about 500 *metres*, where the pestilence has never been known to exist: it serves, indeed, as a place of retreat to the inhabitants of the Turkish capital during the prevalence of an epidemic. On the same mountain, but at a less elevation, there is another village that by no means enjoys the exemption of the first. There is a spot in Malta to which the plague has never reached; from this circumstance it has received the name of safi (pure). According to the testimony of Desgenettes and Clot-Bey, the Citadel of Cairo, which stands upon a lofty eminence, has uniformly escaped during the worst epidemics that ever raged in that city.

A most important question here suggests itself to our consideration: viz: *When a pestilential epidemic prevails in a place, how many cases of the disease are to be attributed to the influence of the epidemic constitution, and how many either to the absorption of the miasms emanating from the sick, or to direct or indirect contact with them?* As may be imagined, the solution of this problem is attended with many difficulties. Dr. Lacheze, when physician of the hospital at Cairo in 1835, endeavoured to determine the point. According to the observations of this gentleman, not more than one person in 400 of those who were entirely isolated or in quarantine was attacked; whereas the pestilence of that year carried off no fewer than one in three of the general population, that remained in free pratique. Without disputing the accuracy of the figures given by this gentleman, it is the opinion of many able observers that they should be interpreted very differently from what M. Lacheze has done; at all events, it must never be forgotten that the very persons, who put themselves in quarantine during the prevalence of an epidemic, are precisely those who enjoy the greatest comforts of life and pay most attention to a hygienic regimen. The Commissioners were therefore desirous of obtaining if possible some less objectionable data for comparison, and it occurred to them that the best plan would be to ascertain the results in some large public establishment, either at Cairo or Alexandria, the inmates of which were living in very nearly the same conditions as the rest of the population out of doors. For this purpose, the Arsenal at Alexandria, which always contained 6000 workmen at the least, was selected for observation. Now, what has been the result of their enquiries? The word of the report are these:

"In this establishment, no attack could be attributed to an accumulation of pestilential miasms. Such an accumulation never existed; for, whenever an invalid was discovered to be infected, he was instantly taken into an hospital situated without the arsenal. Neither could contact with those infected be regarded as the cause; since, whether from the invalids having been removed at the beginning of the disease, or from quite a different cause, the neighbours of those who were seized with plague, as well as those who had touched them, were never attacked with the disease. The number of workmen at the arsenal, removed to the hospital on

account of plague, gives us then that of the cases attributable to the epidemic, apart from any other, agency. Now 300 workmen having been attacked with the disease in a total of nearly 6000, we may fairly believe that the epidemic influence alone acted upon one individual in every twenty. This proportion is without doubt very different from that pointed out by M. Lacheze; but it also varies very considerably from that furnished by the population in free pratique; for this at Cairo and Alexandria, as we have already seen, lost one in every three. Are we to believe, with Clot-Bey, that the difference of hygienic condition completely accounts for these facts, and that, if the mortality among the workmen in the hospital did not amount to one in every three of their number, this was solely owing to their having been kept cleaner and better fed than the rest of the working population at Cairo and Alexandria?

"While we readily recognize and publicly admit the very great power of hygiene in preventing and moderating the ravages of the plague, we must at the same time distinctly state that the deductions drawn by Clot-Bey appear to us to exceed what the facts warrant.

"Neither can we assent to his final conclusion, that all cases of plague should be attributed to epidemic influence alone. We must reject this conclusion, on the one hand, because it does not appear to us to be based upon positive and satisfactory proofs; and on the other hand, because, if it were rashly received, it would have the serious inconvenience of checking all enquiry into those causes, which, secondarily, tend to propagate the plague, and increase its fatality. In science, a false explication is less dangerous in that it extends error, than that it impedes the search after truth.

"From the facts and considerations contained in this chapter, we think we are fairly justified in drawing the following *conclusion:*

"The plague, abstractedly from the influence which the infected may exercise, spreads itself after the manner of most epidemic diseases, viz. by the action of general causes."

THIRD PART.

We now approach the question of the transmissibility of the plague, away from or beyond, as well as within, epidemic foci—a question, we need scarcely say, of the highest importance and, at the same time, of very difficult solution. The first point that we shall discuss is this:—

CHAP. 1.—*Is the plague transmissible by inoculation either of the blood drawn from a vein, or of the pus from a bubo, or of the serosity from the phlyctena of a carbuncle?*

"It is obvious," says M. Prus most justly, "that if the plague be truly a virulent disease (*à virus fixe*, to use the expression of certain writers), the possibility of the inoculation of its virus would approximate it to epidemic contagious diseases; whereas if it does not furnish any principle, liquid or solid, that is susceptible of being inoculated and of producing a virus similar to that which gave it birth, the disease must be withdrawn from the class of diseases that are properly contagious, such as Small-pox, and would approach in this respect the character of Typhus, which is propagated by peculiar miasms, but which gives out no inoculable element."

And here it should be noted as an important fact that, if the diseases that are indubitably contagious—Small-pox, Hydrophobia, Glanders, and Syphilis, for example—all present us with a palpable liquid which contains the poisonous principle, such is certainly not the case with the Plague. Hence medical men have operated, by turns and almost indifferently, with

the pus of bubo, the serosity of a carbuncle, or even with the blood itself of a pest-patient.

The experiments that were made in Egypt by Desgenettes, White, and a few other physicians, about the beginning of the present century, to ascertain the effect of the direct inoculation of the matter taken from plague bubos, are anything but satisfactory and conclusive. We shall therefore not dwell upon them, but at once proceed to notice those which were instituted in 1835 at the Cairo Hospital in the presence of Gaetani-Bey, Clot-Bey, and Drs. Lacheze and Bulard, and which are deserving of all confidence.

Five criminals, who had been condemned to death, were the subjects of the experiments. A lancet, wetted with the blood drawn from a pest-patient, was passed under the epidermis on the inside of the arm of one of these criminals, at two different points. On the third day afterwards, the man was affected with confirmed plague—so, at least, says Dr. Lacheze, who reports the experiment; Clot-Bey thought the case doubtful. Three days subsequently, the man was convalescent.

In three other cases, no effects followed the inoculation of the blood. In two cases, the serosity from a carbuncle, and in one the pus from a bubo, was used for the purpose of inoculation: in none of these cases, was the disease induced.

With respect to the single case, in which the disease (mild indeed) occurred after inoculation with the blood of a pest-patient, it must be kept in mind not only that the man was exposed, as a matter of course, to the epidemic atmospheric influences then existing in Cairo, but also that, for three days before the performance of the experiment, he had been living in a pest-hospital, which was necessarily a focus of pestilential infection.

Clot-Bey inoculated himself, in six different punctures, with the blood of a pest-patient: no constitutional effects followed. A few days subsequently, he inserted some pus from a bubo on the inner part of his left arm: this was followed by a slight indisposition, which he attributed to the absorption of the purulent matter, but which bore no resemblance to the symptoms of plague.

The results of certain trials made by Professor Pruner in 1829, and by Dr. Rossi in 1841, were altogether similar.

The general *conclusion* of the Commissioners upon the important point under consideration is to this effect:

"The results of the inoculation of the blood drawn from the vein of a plague patient, or from the pus of a pestilential bubo, have been equivocal; the inoculation of the serosity taken from the phlyctenæ of a pestilential carbuncle has never given the disease. It is therefore not proved that the plague can be transmitted by inoculation, even under the influence of a pestilential constitution.

"We are not acquainted with any experiments that have been made upon the same subject, at a distance from an epidemic focus.

"It is useless to observe that the study of the effects which might have been obtained from inoculation of the plague, a study so important for the knowledge of the nature of the disease and consequently of its transmissibility, presents, nevertheless, no direct application to the question of quarantine. There can be no fear that the mass of a population will ever allow themselves to be inoculated with the plague."

CHAP. II.—*Is the plague observed in epidemic foci, to be transmissible by immediate and direct contact with the sick?*

The Arabian physicians, as well as their predecessors, regarded the disease as purely and simply epidemic, and seem therefore not to have troubled themselves in seeking to determine if the disease be communicable from one person to another. We must come down to the middle of the 15th century, the time of Frascatorius, before we meet with any formal exposition of this doctrine. The celebrated physician of Verona recognised three modes in which the plague may be communicated:—1, direct contact with the sick; 2, the infection or contamination of goods, clothes, &c.; and 3, diffusion of morbific miasms through the atmosphere. The relative frequency of these three modes was believed to be in the order that they are here enumerated; the first being supposed to be by far the most common, and the last to be comparatively very rare. These opinions of Frascatorius prevailed almost universally down to the year 1720. In that year, Chicoyneau, Verney, and Deidier of Montpelier maintained with considerable eclat the doctrine of the non-contagiousness of the plague; they regarded it as purely epidemic. Their chief argument was, that they had touched the bodies of plague-patients without taking any precautions, and that they had not caught the disease. The opposite and older opinion, however, continued to be very generally held in the schools.

In 1771, Mertens, Orrœus and Samoilowitz, who had an opportunity of watching the plague at Moscow, declared their belief that it was propagable only by direct or indirect contact with the infected, and never through the mere medium of the air. Stoll, however—who is characterised by Dr. Prus as the most able observer, after Hippocrates and Sydenham, of epidemic diseases—was not at all satisfied with the prevailing opinions on the subject in question, and pointed out in the following (ironical?) passage the necessity of re-examining them with care and candour.

"He who would deny," says this truly enlightened physician, "the contagion of the plague, and attribute a very grave disease to an epidemic cause, acting equally upon all, but not producing equally upon all the same effects, and would ascribe it either to the constitution of the year, or to an alteration in the air more fit to produce putrid diseases than in other years, that person, I say, would assert (what would be considered) a paradox. But, at the same time, whatever truth he might utter, and whatever service he might render in the calamitous conjuncture, it were well for him to be at a distance. He, who would hold this opinion, might find abundance of arguments, which could not be refuted, in all the authors who have written on the plague, even in those who have defended contagion; unless, indeed, the love of the marvellous should make him despise or overlook the most simple causes, which he might find at his very feet."

All the medical men who accompanied the French expedition to Egypt, Assalini alone excepted, were of the opinion that the plague is propagated by contact with the infected. For nearly forty years after their return, this opinion has been universally received, and acted upon. It was not till 1835, that a change of sentiment began to be manifested among medical men on this most important subject. In the course of that year, as we have already seen, a number of European physicians had an opportunity of studying the terrible pestilential epidemic that ravaged Egypt. Impressed at first most firmly with the belief of the transmissibility of the

disease by contact with the sick, they have all, with scarcely one exception, completely changed their opinion; as, indeed, MM. Brayer and Cholet, who had observed the epidemics of 1819, 1826, and 1834 at Constantinople, had previously done. The writings of these last-named gentlemen, and subsequently of Clot-Bey and Aubert-Roche, have mainly contributed to effect this very remarkable revolution in medical doctrine. We shall briefly note a few of the most interesting facts, which have been of late years made public.

During the pestilence of 1824, upwards of 30,000 persons died in Cairo, while not more than two or three cases occurred in Alexandria, although the communication between these two cities was constant and uninterrupted. In 1834, on the other hand, the plague broke out and continued in Alexandria for a very considerable time, before it made its appearance at Cairo; and it had existed for fully eight months in the former city, before there was any sign of it in Mansoura and Damietta, although the daily intercourse between these places remained entirely free. Dr. Coch, principal physician of the Egyptian fleet, mentions an interesting fact observed by him in 1835. Ten men had gone from Sakkarah, a populous village, to Cairo, where the plague then existed. On their return home, every one of these men sickened, and died; yet not a single member of their families, who had assiduously waited upon them, took the disease. "Such a fact," it is emphatically added, "was observed hundreds of times during the course of this great epidemic." The same gentleman states that the viceroy, having ordered that all vessels in which the plague appeared should be subjected to a quarantine of 11 days, the sick were immediately disembarked and carried on shore by the sailors of the fleet; and, although these sailors returned on board and communicated freely with the rest of the crews, not a single case of infection was the result.

We owe the following facts to Dr. Roche. The ports of Suez and Cosseir on the Red Sea draw the chief supply of their provisions, the one from Cairo, the other from Keneh in Upper Egypt. In 1835, the plague broke out at the latter place about the same time that it made its appearance at Cairo. Suez was attacked by the pestilence; but Cosseir remained quite exempt. The first of these places was surrounded by stagnant marshes, a state of things not unlike to what exists in all the towns of the Delta; the second is built upon rocks, and is surrounded by bare arid hills. During this epidemic, Djedda, Yambo, and Moka enjoyed the same immunity as Cosseir, although the sick from Suez and other infected localities often died in the midst of them. Still more convincing is the following statement:—

Every year pilgrims depart from all parts of the country, subject to the laws of Mahomet, to go to Mecca. Caravans from Morocco, Darfour, Egypt, Constantinople, Persia, Asia Minor, and Syria converge at Djedda, at Medina, then at Mecca, the central point. They carry merchandise with them, for this pilgrimage is also a fair. Has the plague ever broken out at the place of meeting of all this population and all this merchandise, which have often, be it remembered, come from places infected by it? No. On the contrary, it is proved that, from time immemorial, the plague has never been seen in Arabia. The epidemic plague, which desolated a great part of Lower Egypt in 1825 and 1835, had not one victim in

Arabia, notwithstanding the daily and perfectly free communication which existed between these countries. This has also invariably been the case with respect to the pestilential epidemics of Constantinople, Smyrna, or Syria. The Arabian historians pretend that their country owes this immunity to the protection of the Prophet!

Nubia, Sennaar, and Abyssinia, notwithstanding their close connexion with Egypt, are not acquainted with the plague. If it may be said regarding Arabia, Sennaar, and Nubia, that the heat in these places prevents the condensation of pestilential miasms, the same reason cannot be alleged for Abyssinia, which is a temperate country, the thermometer varying from 16 to 25 degrees Cent. above zero. Here the salubrity of the climate alone serves to keep the disease at bay. Abyssinia is a mountainous country with inclined plains, where there are neither marshes nor stagnant waters.*

For the extract which follows we are indebted to the work of Clot-Bey; allusion has been made to it in a former page.

"During the five months that the epidemic of 1835 lasted, MM. Gaetani, Lachèze, Bulard, and myself at Cairo, MM. Duvigneau, Scisson, Perron, Fischer, at Abouz-Abel, and MM. Rigaud and Aubert, at Alexandria, visited the infected in the hospitals and in private houses. None of us took the least prophylactic precaution. We were in immediate contact with the sick during all the stages of the disease. We received upon our clothes and upon our hands the matter that was ejected by vomiting, the blood of those who were bled, the pus from the thousands of bubos which we opened. More than a hundred dissections were made at Cairo, and we passed whole hours in endeavoring to detect, in the bodies of those who had just expired, the pathological alterations which had hitherto been so little attended to. The same researches were made with equal care at Alexandria.

"Dr. Rigaud is the only one among us who fell a victim to the reigning epidemic.

"It is remarkable that many physicians, who scrupulously avoided all contact with the sick and with suspected objects, were attacked with the plague and died. Of this number are Drs. Mannucchi, sen., Leopold, and Lardoni."

The observations made by different medical men, during the subsequent epidemics of 1837 in Syria and of 1841 in Egypt, amply confirm these statements of Dr. Roche and Clot-Bey. M. Granet was the chief medical officer of the troops stationed in the province of Adana (Upper Syria), when the plague broke out there in 1837. He was entrusted with all the sanitary regulations ordered by the governor. At first, it was attempted to arrest the extension of the pestilence by establishing a cordon around the infected spot. This was speedily found to be wholly useless. Upwards of 15 new cases were received every day into the military hospital, in which there were usually from 40 to 60 cases at a time. No precaution to guard ageinst the risk of contagion was employed either by the medical men or by the other attendants of the sick; and yet not a

* Aubert-Roche, *ouvrage cité*, p. 100.

single case of the disease occurred among them, although the epidemic lasted for three months. "How can we believe," says M. Granet after relating these particulars, "that, if the plague was really transmissible by contact with the sick, we should not have had a single case of this transmission?"

The evidence of Dr. Ibrahim, a native physician of Cairo, is highly satisfactory and convincing. He adduces many cases where one member only of a large household was affected with the plague, although the patient had been waited upon by the whole body of the domestics. The case of the wife of Hassan-Pacha, who died on the 35th day of the disease, is more than usually instructive; she had no fewer than two dozen white and black slaves, two keios, two eunuchs, and four pages, in constant attendance upon her!

Dr. Delong also and M. Euzieres report many cases that occurred under their own immediate notice in Cairo, during the epidemic of 1841, in which the relatives of the sick, who had most assiduously nursed them up to the hour of death, entirely escaped. It seems unnecessary to give the details of any of them. In two cases, one of which proved fatal on the fifth day of the attack, the patients continued to suckle their infants; the children were not affected. In several instances, the disease attacked those who had sequestrated themselves by the most strict quarantine from all communication with the city; while others of their household, who were less timid, remained intact.

The testimony of Dr. Arnoux, surgeon-major of the 43rd regiment (Egyptian army) stationed in 1841 at Nabaro, of Dr. Dieterich of the 5th at Damietta, of Dr. Penay of the 5th cavalry at Neguille, and of Dr. Chedufau, chief physician of the central military hospital at Cairo during the same year, all serves to prove that the immediate attendants upon plague-patients are, on the whole, very rarely infected;—always provided due regard be paid to the ordinary hygienic precautions. The last-named gentleman informs us that, in consequence of there being no separate plague-hospital prepared when the pestilence broke out, the sick were received at first into the general hospital along with the other patients. There were no fewer than 182 cases of plague treated in this hospital; and, although the number of the ordinary inmates and attendants during this period amounted to 2,000, no instance of infection could be directly made out. None of the "officiers de santé," consisting of 92 Europeans and 300 Arabs, remained in quarantine, but waited upon the sick without taking any precautions. Only three of the Europeans were attacked; and, of these, two recovered. Three also of the Arabs suffered; they all died. M. Chedufau himself, besides his hospital duties, treated many cases of plague in the town, opening bubos, dressing carbuncles, and executing all the necessary medical duties to his patients. He performed, moreover, 17 *post-mortem* examinations. During the whole of the epidemic, he was in continual intercourse with the members of his own family; and yet, although no preservative means were employed, neither he nor any of his household suffered.

The experience of Dr. Perron, the director of the medical school at Cairo, confirms in every respect the truth of these statements. Not one

of the professors or pupils, who were in daily attendance upon the sick, was infected.*

After relating numerous other facts and statements, all bearing upon the question proposed at the beginning of this chapter, the *conclusion* is at length arrived at, that,

"On the one hand, immediate contact with thousands of plague-patients has not been followed by any dangerous consequences to those who have been exposed to it in the open air or in well-ventilated chambers; and on the other, that there is not a single fact which indisputably proves the transmissibility of the plague by mere contact with the sick."

CHAP. III.—*Is the plague transmissible by contact with the clothes or effects of the sick, in localities which are, or have been, recently exposed to the epidemic influence?*

After the plague of 1835 at Cairo, the clothing, effects, &c. of upwards of 50,000 plague-patients, who had been carried off by the pestilence, were sold in the public bazaars, without communicating, as far as is known, the disease in a single instance. More than 600 houses remained tenantless for several months after this frightful epidemic; they were then ordered to be visited, and an inventory taken of their contents. Not one of the persons engaged in this service fell sick.

Three thousand plague-patients were received into the large hospital of Esbequiè during the year 1835. On the cessation of the pestilence, the ordinary class of patients was admitted and put into the very same beds, which had been occupied by those who had died of the plague. The sheets indeed were changed, but the coverlets, which had never been subjected to any process of disinfection nor even freely exposed to the air, were used without alarm. No case of infection ensued. These facts were made known in 1840 by Clot-Bey, and have not since been disputed by any one.

Dr. Brayer informs us that it is a fact perfectly well known in Constantinople, that the Jews buy up the clothes, &c., of persons who have died of the plague, however virulent this may have been, for sale at Fit-Bazaar, where most of their stores are. No one dreams of using any means of disinfection. If the deaths be numerous, the bazaar is full; and *vice versa*. All the poorer classes resort thither for their clothing; and, generally, bundle after bundle is turned over and examined, before a purchase is made. In 1812, the "depouilles" of 150,000 victims of the dreadful pestilence of that year were brought together into that market! One portion was speedily bought by the inhabitants of the city; another portion was forthwith sent away into all the principal towns throughout the Turkish dominions; while what remained unsold was kept in close confined magazines, to be disposed of next year. Notwithstanding this dispersion of infected *fomites* to such a vast extent, nothing was heard of the spread of the pestilence thereby. It deserves also to be noticed that a smaller proportion of the Jews died than the Greeks, who, we may remark, have always had great fear of the contagion of the plague.

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* We shall afterwards however find it stated that most of the *pharmaciens* in one of the hospitals at Alexandria caught the disease and died, and also that very many of the pupils in the hospitals at Cairo perished.

It would be easy to mention many other instances where the clothing, goods, equipage, &c., of persons affected with the plague have been taken possession of by others, without any injurious results. But this is unnecessary; suffice it to say that, in Egypt, Constantinople, Smyrna, &c. an epidemic of the plague is almost invariably found to subside and cease at a certain period known beforehand, whether any sanitary regulations have been taken to arrest its course or not; and that then the clothes and other property of the victims freely circulate in the bazaars of the place. If these objects communicated the disease to those who handled them, it is quite obvious that it would last much beyond the period at which it disappears with a truly remarkable regularity.

Such are some of the facts that have led most of the medical men resident in the East to believe, that the plague is not transmissible through the medium of *fomites*. Even although we do not go so far as this, it may surely, with perfect truth be asserted, that in a vast number of instances, some of them indeed of almost continual recurrence, the pestilence has not been communicated by touching, or even wearing the apparel of those who have died from it, although no means of purification or disinfection had been employed. We are not prepared utterly to deny that the disease has been, and may be, sometimes transmitted in this way;* but surely, after the facts which we have mentioned, we may very fairly withhold our credence from many of those histories on record of the plague having arisen in places previously quite healthy, from the introduction of infected objects. Most of the reported facts of this description appear to have been dictated by prejudice and accepted with credulity. Nevertheless, it must be admitted that some medical men, who have had extensive opportunities of observation, still maintain the opposite opinion. Of these, M. Grassi, principal physician of the lazaretto at Alexandria, and who has been in the Egyptian service for upwards of 20 years, is the person whose opinion is entitled to perhaps most consideration. We may state, however,

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* Clot-Bey himself reports the following narrative:—

"On the 15th of April, 1835, (in presence of MM. Gaetani, Clot, Lacheze, and Bulard) two young criminals, Ibrahim-Assan and Ben-Ali, in perfect health at the time, were placed in beds which had just been left by patients affected with well-marked plague.

"In the night of the 18th, Ibrahim's pulse was slightly affected.

"The following day, he had the plague with bubos and carbuncles; he died on the 23rd.

"On the third day after he was in bed, Ben-Ali also felt the ordinary symptoms of an attack of the plague; but the disease abated, and convalescence began from the fourth day after the appearance of the characteristic symptoms.

"That Ibrahim-Assan died of the plague, after having slept in a bed recently left by an infected person, is a fact. But was it the sheets or other coverings of the bed which gave the disease? This is uncertain. Ibrahim-Assan was in a town where a pestilential epidemic raged; he was in a hospital which had contained and did still contain a great number of plague-patients, and in which several medical pupils and intendants had contracted the disease. It cannot then, in his case, be absolutely asserted that the plague developed itself by contact with contaminated objects, rather than by epidemic influence alone, or by miasmatic infection."

that the accuracy of many of the facts, which he has adduced in favour of his views, has been disputed or denied by Clot-Bey and others.

The general *conclusion*, to which the Commissioners have come, after a laborious investigation of all the particulars, is that

"Very numerous facts prove that the clothes and effects, belonging to plague patients, have not communicated the disease to persons who have used them, even without any previous purification. The facts, which seem to indicate an opposite result, can only be considered valuable, if they are confirmed by fresh observations made beyond epidemic *foci*, at a distance alike from *foci* of miasmatic infection and from countries where the plague is endemic."

CHAP. IV.—*Is the plague transmissible, in countries where it is endemic or epidemic, by merchandise suspected to contain pestiferous matters?*

It will be obvious that it must be very difficult to decide this point in places where the pestilence is indigenous, and where it is, therefore, liable to be developed spontaneously at any time; for what may be attributed by certain persons to handling some description of goods, may be altogether owing to the influence of an endemic cause. The case related by M. Sicard of Marseilles to M. Prus is unsatisfactory in several points of view, and nearly the same thing may be said of all the analogous examples which have been made public. It may, therefore, be very fairly set down that the transmissibility of the plague, under the circumstances mentioned, has at all events not been proved.

CHAP. V.—*Has the plague been observed, in epidemic foci, to be transmitted by the air being charged with pestiferous miasm?*—in other words, is the plague infectious through the medium of the atmosphere during the prevalence of an epidemic pestilence? Until of late years, the question of atmospheric *infection* had been altogether superseded by and merged in that of direct and immediate *contagion*. We need scarcely say that it can never be an easy thing to determine with exactitude the infectiousness of any disease, while a pestilential constitution of the atmosphere exists, and when consequently a whole population is exposed to the morbific influence. As we have previously remarked, scarcely a single person escapes *in toto* the effects of the malarious condition of the air; they are experienced by all to a certain degree during the prevalence of the epidemy.

We shall first enquire if the air of a plague-hospital has seemed to give the disease to those, who had most carefully avoided all contact with infected persons or objects. That great mortality occurred among the *pharmaciens* and attendants in the plague hospitals at Cairo and Alexandria during the pestilence of 1835, notwithstanding the use of all precautionary means to avoid direct contact with the sick, or of any thing belonging to them, cannot be disputed: of 20 pupils, that were sent from Abouzabel to the hospitals of Cairo, 19 caught the disease and died. The question then comes to be, was this mortality owing to the general epidemic influence, to local atmospheric infection, or to immediate contact with the patients? The best answer to this will probably be found by referring to what took place at Abouzabel itself—only a few miles distant, it will be remembered, from Cairo—when that place was attacked by the pestilence. The barracks of the sick were situated in the open country, at some

elevation above the plain, and well ventilated. Not one of the medical inmates or attendants, although in most frequent contact with infected persons and objects, suffered; and this too, be it remembered, in an epidemic focus. The free ventilation had prevented the formation of a "focus of infection." Dr. Laidlaw, the physician of the general European hospital at Alexandria, remarks most truly that "whenever a number of plague-patients are collected together in one space, they seem to create a pestilential atmosphere unless free ventilation be employed." Hence the importance of never having many plague-patients in one room or ward, and of maintaining a most thorough ventilation through every part of the building, which should always, if possible, be situated in an elevated open position and on a dry foundation, at a distance from stagnant water or any source of malarious exhalations. If these circumstances be not attended to, hospitals are apt to become positive foci of infection, and thus to increase rather than to diminish the general mortality.

That the disease, when it has attacked one of the inmates of a house, is, under certain circumstances, apt to extend to others who are dwelling there, cannot be disputed. Dr. Grassi tells us that, in 1835, no fewer than 57 deaths occurred in the house of Hingi Osman, the treasurer of the marine at Alexandria. This is only one out of many similar instances that might be quoted. The fair conclusion, therefore, seems to be, that pestilential miasms or effluvia emanate from the bodies of the sick, and that these powerfully tend to act upon the systems of persons whom the epidemic influence has already made liable to the disease. Almost all the resident medical men in Egypt at the present time, who believe in the transmissibility of the plague, are of opinion that the transmission is effected in this way. Dr. Grassi is the only one who maintains that the disease is communicated by direct or indirect contact with the sick, without any intervention of atmospheric agency.

Remaining long in the chamber of the infected is the thing most to be avoided by the medical and other attendants. What Dr. Rigaud said to his friend M. Lesseps, the French Consul-general, who visited him constantly to the last moment of his life—"Do come and see me twenty times a day, but don't remain more than five minutes at a time in my room"—abundantly shows what his sentiments were upon this point.

By a singular perversion, the common practice in the East is to keep a patient in a close and confined apartment, with as imperfect a current of air through it as possible. What wonder then that a disease like plague spreads wherever it makes its appearance: every sick chamber becomes a new focus of infection! There cannot, therefore, be a reasonable doubt that the diffusion and mortality of the pestilence are powerfully promoted by the contamination of the atmosphere with morbific emanations from the bodies of the infected.

Sometimes it happens that when a locality, which has contained a number of plague-patients, comes to be occupied by other persons (who may use nevertheless every possible precaution to avoid all contact with suspected objects), the disease re-appears to a greater extent among the new residents than can fairly be attributed to the sole agency of the epidemic influence which may be then existing.

In 1834 in the month of June, during the insurrection which broke out

in Judea, the insurgents pillaged and sacked Jerusalem. A number of Roman Catholics took refuge in the convent of St. Saviour in this city.

"At the end of ten or twelve days of close confinement, I remarked," says M. Delong, "cases of plague among this distressed population, huddled together in their dormitories, upon and under the stairs, in the courts and other chambers of this vast building. After twenty-five days of expectation, Ibrahim Pacha at length arrived, and the city was relieved. The holy Fathers, full of alarm, hastened to clear their dwelling of all this mass of people, and shut themselves up in most strict quarantine. What happened? Of all those who left the convent, three only died four or five days afterwards. But out of the 63 priests who thought to save themselves by isolation, no fewer than 22 died."

"Now," continues M. Delong, "what part did infection play in this instance? I will not positively decide. It seems, however, clear to me, that we should, in all similar circumstances, disperse the infected into different quarters, instead of shutting them up with others, as yet intact, in a confined locality where sanitary regulations have not always been attended to."

What occurred in the musical academy at Kanke in 1835 is still more deserving of attention:—

The plague having broken out in this school, although it was kept in the strictest quarantine, the pupils were sent into the desert, where they continued for upwards of a month. In the mean time, all the rooms were well cleansed and purified, and no person had remained in the building. Not one case of plague occurred in the desert: but no sooner had the boys returned to their old quarters, than several were taken ill; and each day several fresh cases were reported. Again were the boys sent into the desert: and again the disease ceased to spread. While they continued in the desert, 15 soldiers were employed to go daily to the village, where the plague was raging, for provisions: but none of these men caught the disease themselves, or gave it to the boys.

During this disastrous year, many striking circumstances, similar to those now mentioned occurred in the military barracks, all of which had been put into strict quarantine. Although every attention was paid to cleanliness, they seemed to remain foci of infection whenever the disease had once shown itself in them.

It will be obvious that the facts now mentioned, coupled with similar ones observed in epidemic foci and more especially on board-ship, must receive new and very useful applications in considering the question of Quarantine.

Admitting that morbific miasms are given off from the bodies of plague-patients, and thus render the surrounding atmosphere pestiferous, we have next to enquire whether these miasms are exhaled from the lungs, or from the surface of the body of the sick, or from both; and also when they prove infectious, whether they are (most probably) absorbed by the skin, the lungs, or in the way of deglutition. There can be very little doubt in the present day that very many cases, where the disease has appeared to be the result of direct contact with a patient, or with any thing he has used, may fairly be regarded as examples of infection through the medium of the atmosphere. Dr. Brayer has put this point very forcibly:

"A person finds himself affected with plague, although he has never left the house; straightway he tries to bring to mind where he was the night before, two or three days ago, the objects he touched, and so forth. If he has not been

out of doors for one or two weeks, owing to indisposition, it matters little; he was out three weeks, a month ago; and as the virus may be retained for months, nay, for years, it is not surprising that the disease should have shown itself. He will not for a moment consider that the skin is guarded by the double covering of the epidermis and of the clothes which he wears, and that therefore the virus can only with the greatest difficulty be transmitted from the exterior to the interior. As to pulmonary absorption, that is never spoken of. It is a received opinion among the Franks that the air is not, never has been, and never can be the vehicle of the plague. They refuse to believe that, in seasons of the plague, every individual breathes an air more or less deleterious, that the person who sees or touches an invalid is in the atmosphere of the invalid, and that when, by means of the air, a deleterious principle is conveyed into the minutest bronchial ramifications, and by the act of deglutition into the gastric passages, there is then much more than mere contact; for there is a real penetration in the first case; and, in the second, there is digestion and interior absorption. Infection therefore exists, in the strongest sense of the term; and the disease which it occasions is more or less severe, according to the quantity of miasmata introduced into the system, their intensity, and the individual predispositions or susceptibilities of those who have absorbed them."

The Academic Society at Marseilles, in the report which was unanimously adopted in August 1845, expressly recognises the following two propositions:—

"1. Writers, the most at variance in all that concerns the general history of the plague, are nearly unanimous in asserting that the simple contact of one individual with another is one of the modes of transmission the least favorable to the propagation of the pestilence.

"2. A lengthened stay in the atmosphere of the sick, and, above all, exposure to the pestilential miasms which contaminated objects exhale are highly dangerous."

The only difference, says M. Prus, which, upon this truly important point of doctrine, exists between the Academical Society of Marseilles and the Commissioners, may be thus summed up:—

The Academic Society asserts that the simple contact of individuals is one of the modes of transmission least favorable to the propagation of the plague.

The Commissioners think that no well-authenticated fact establishes the reality of such transmission. They are moreover not acquainted with any facts to authorise them to believe, with the Academical Society, in the dangers of miasms from contaminated objects.

The Academical Society and the Commissioners equally acknowledge that a lengthened stay in the atmosphere of infected persons, or, in other words, infection by pestilential miasms is that which is most to be feared.

Dr. Mead who, at the time of the plague at Marseilles in 1720, was ordered by the English government to draw up instructions for preventing, if possible, the introduction of the pestilence in England, and to arrest it if it was introduced, has much insisted upon the utility and necessity of promptly removing the sick from the seat of the infection, and transporting them to some distance.*

* R. Mead, M.D. *A short Discourse concerning Pestilential Contagion.*—London, 1720; and *De Peste Liber*, 1723.

He has mentioned as worthy of imitation the course pursued at Rome, during the plague of 1657, by Cardinal Gastaldy, at that time invested with full power to take every sanitary measure which he judged proper.

The Cardinal prohibited any infected person, and even any person in health who was suspected, to remain in their houses. They were promptly taken to the hospital, built on the island which divides the Tiber. With respect to those who had occupied the same house, they were placed in other hospitals near the city, from whence they were removed into the island if the disease showed itself. During this time, the Cardinal was very careful to have all the furniture taken out of the infected houses, exposed in the open air, and the apartments left open, in order to purify them.

By these means the Cardinal, in two months, caused the plague to cease, after it had raged at Rome for two years.

But that which deserves most attention, adds Dr. Mead, is, that, before these regulations, it was constantly observed that the disease rarely appeared in a house without attacking all its inhabitants; whereas, after they had been put in force, scarcely five out of a hundred of those who were removed from the proximity of the infected, were subsequently attacked with plague.*

Muratori informs us that similar measures had been adopted with equal success at Ferrara, in 1630.†

The Board of Health at Constantinople has, for the last eight or nine years, followed out the prophylactic method recommended by Gastaldy and Mead, removing the infected to a hospital, and emptying every house, in which a case occurs, alike of its inhabitants and furniture, having it well cleansed and purified, and not allowing any one to occupy it for the space of a month. It is to the adoption of these means that the board attributes the exemption of Constantinople and the principal ports in Turkey from the plague, since the year 1839. If, in place of acting in this manner, the houses of the infected are condemned with their inmates to a severe quarantine, the result will necessarily be to create fresh foci of pestilential infection, and thus increase the very evil that is vainly sought to be extinguished.

M. Seisson, principal physician to the hospital at Cairo and formerly professor of the school of Medicine at Abouzabel, observes that if, at the time of the appearance of the plague at Cairo, in 1835, after the arrival of the Maltese Giglio, who died of that disease which he had brought from Alexandria, they had dispersed the other members of his family in the country, it would probably have prevented the death of eight or ten persons who, kept by military force within the house, contracted the plague and died. Two persons fled from the focus of infection, of which the quarantine was the cause; they both remained free.

The case of Giglio, adds M. Seisson, does not prove, as it has been said, the contagion of the plague; it only proves the danger of shutting up, in a narrow space, individuals who have been in connection with an infected

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* Gastaldy, *Tractatus de avertendâ et prostigandâ* (?) *peste.* Bologna, 1684, folio.

† Muratori, *Governo della peste et delle maniere di guardasena.* Modena, 1714, 8vo.

person. It is therefore of the first importance to abolish what are called special quarantines for houses, in which persons have been seized with plague. On the contrary, they should be at once emptied, aired, and purified, to prevent every focus of infection.*

Dr. Mead has quoted from Gassendi a passage wherein this author attributes the frightful mortality of the pestilence that prevailed at Digne in Provence, in 1629, to the severe measures that were taken to prevent the inhabitants from leaving the town and retiring into the neighboring country.

In 1720 the inhabitants of Marseilles were prohibited, under pain of death, from leaving that city or its suburbs. Hence, doubtless, the terrible devastation that ensued.

"And yet," says M. Prus, with an emphatic force of argument that cannot fail to make a deep impression on the public mind, "what would be done in the present day according to existing regulations, if the plague made its appearance in any town of France? It would be isolated by a cordon of troops, for the purpose of preventing any of the inhabitants leaving it; in other words, the unfortunate town would be condemned to retain, and in a concentrated form too, within its bosom all the various causes which serve to develope foci of pestilential infection. Is it then impossible to reconcile the advantages of the public health with the most common laws of humanity! We think not; on the contrary, we are firmly convinced that perfect security may be given to neighboring towns and to the whole kingdom, by taking the necessary measures to remove the great majority of the inhabitants of the town, in which the plague should appear, from the danger. To obtain results so desirable, it needs only to know how to profit by all that time and experience have taught us of the epidemicity of the disease and of pestilential infection.

Conclusion.—"In epidemic foci, the plague is transmissible by the miasms which emanate from the bodies of the infected, and by the foci of infection thereby produced."

Chap. VI.—*Is the plague transmissible beyond, or away from, epidemic foci?*

This, it will be perceived, is the most important of all the questions respecting the history of the plague, in reference at least to the subject of Quarantines; for, upon its solution, the propriety of making any change in existing regulations must depend.

A considerable number of medical men, who have studied the epidemics of 1835 and 1841 in Egypt, will answer it in the decided negative, and for the following reasons:

When the epidemic constitution ceases, all or nearly all the sick recover, and no new attacks occur.

Numerous plague-patients have been, and still are, accumulated in the hospitals and houses; all the conditions favorable to the transmission of the plague by mediate or immediate contact, or to its propagation by miasmatic infection, continue to exist together; and yet, at a period that is almost known and determinate, the epidemic becomes extinguished, and with it the plague ceases entirely.

* M. Seisson, *Lettre addressée au consul general d'Angleterre à Alexandrie en* 1839.

An infected person coming from an epidemic focus is now not more to be feared than a sporadic plague-patient, who, by the consent of all the medical men in Egypt, never occasions any risk.

If it is doubtful whether the clothes and goods of infected persons can transmit the plague in the time of an epidemic, it is certain that, when once this has disappeared, such clothes and goods may be used with impunity.

An epidemic only appears in a country in the train of certain local and atmospheric influences, whose action has been prolonged for a greater or lesser period of time; very commonly too, privations, fatigues, physical or moral troubles have been experienced, in different degrees, by the inhabitants. From these united causes result more or less general predispositions to contract the prevailing disease. Now, when a vessel carries one or more infected persons beyond the focus of infection, she cannot take along with them all the causes, past and present, which are necessary to the development of an epidemic.

It must be confessed that the observations, made in Egypt during the years 1835 and 1841, seem to justify these propositions and the conclusions which flow from them.

Let us now see whether the facts observed on board vessels at sea, and in the lazarettos of Europe, are in accordance with these conclusions.

Since the year 1720 down to the present period, 25 vessels having the plague on board, have arrived in the ports of France or Italy; 10 at Marseilles, 5 at Venice, 8 at Leghorn, and one at Genoa. We shall confine our remarks to the circumstances connected with the arrivals at Marseilles, the official documentary evidence upon these being much more complete than in the other cases. The years in which these arrivals occurred in 1741, 1760, 1784, 1785, 1786 (*bis*), 1796, 1819, 1825, and 1837. The entire number of cases of plague (omitting all the doubtful ones), treated in the lazaretto of this port since 1720, is 32; and of these, 18 have proved fatal. Three of the quarantine surgeons caught the disease during their attendance on the infected; they all recovered. A fourth surgeon, who had arrived on board an infected ship, and subsequently acted in his professional capacity in the lazaretto, died. Four of the health-guards, who had been (most improperly) put on board infected ships, contracted the disease in the lazaretto; two died. A sailor who acted as assistant in the lazaretto infirmary, was taken ill and died. Two other sailors, belonging to an infected vessel, but who seemed to have caught the disease in the lazaretto where they had been confined for more than 12 days, died.

In the 11 cases therefore of plague, which might have been contracted in the lazaretto, 6 of the patients recovered, and 5 died: all the latter cases occured in men who had been on board infected vessels. Of the three health-guards, who had caught the disease on board, only one recovered. Indeed it would seem that, in all the fatal cases, the patients had been for a longer or shorter period of time on board infected vessels.

It appears, also, that not one of the cases, which occurred on board a vessel at sea during the voyage to France, recovered;—a circumstance that very emphatically shows the malignity of the disease when it occurs in a crowded confined space, and the great advantage of treating it in a large open lazaretto.

From the facts now alluded to, we are surely justified in maintaining not only that the plague may be transmitted on board-ship among individuals coming from the same infected focus, and living in the same hygienic conditions; but also that a plague-patient, received into a lazaretto in another and distant country, may become the cause of infection to others. The *conclusion*, therefore, of the Commissioners is this:

"It is indisputable that the plague is transmissible beyond or away from epidemic foci, whether on board vessels at sea, or in the lazarettos of Europe."

The question proposed in Chap. VII. is to this effect: *Is the plague transmissible, away from epidemic foci, by immediate contact with the infected?* It is at once answered in the negative; there is not a single authentic case on record to prove that the disease had ever been propagated in the way mentioned. The same thing may be said in answer to the question in the following Chapter, viz: *Is the plague transmissible, away from epidemic foci, by the clothes or other effects which have been used by the infected?* Instances indeed have been related, by some writers, of passengers appearing to be taken suddenly ill almost immediately after opening their bags or boxes, and handling their contents; but not one of the narratives of this sort is satisfactory in its details or conclusive in its evidence. Nevertheless the Commissioners, unwilling to commit themselves unqualifiedly on the point under consideration, suggest that new experiments and observations should be made with all possible precaution, at a distance from every focus of infection, and in a locality where the plague is not endemic.

Chap. IX.—*Can articles of merchandise transport the plague beyond or away from epidemic foci?*

The evidence and facts, on which the advocates (few in number as they are in the present day) of the affirmative side of this question chiefly rest their opinion, are the following;

The plague of London in 1665 is said by Hodges to have been imported from Holland into England in bales of cotton. But it may be fairly objected to this opinion, that the registers of that city clearly show that the disease had been endemic within its walls for some years preceding the outbreak of the great pestilence, and the destructive conflagration that occurred immediately afterwards. Since that period, the plague has never reappeared in the English metropolis.

The testimony, that has been adduced to show that the plague of Toulon in 1721 was owing to the reception by some of the inhabitants of some suspected silk that was stolen out of quarantine, is equally unsatisfactory. These and such-like statements cannot be fairly admitted, in the present day, to be entitled to much weight in the determination of the important question under enquiry. We are not sufficiently acquainted with the accompanying circumstances of either case to warrant us in laying much stress upon them. Does the following fact,—the details of which may be relied on as in every respect authentic and perfectly accurate—communicated by Dr. Laidlaw to the English Consul at Cairo, enable us to form any thing like a decided opinion?

In 1835, the epidemic plague raged at Alexandria among all the servants and employés living in the magazines of the Egyptian government.

Notwithstanding this, a vast number of bales of cotton, daily handled by the prisoners, were exported from January to June—that is to say during the whole continuance of the epidemic—to all the great ports of Europe.

There were exported this year

To England . . .	31,709	bales.
To Marseilles, . . .	33,812	"
To Leghorn . . .	424	"
To Holland . . .	150	"
To Trieste . . .	32,263	"
To other ports . . .	32	"

Now, although no precautionary means were taken in the way of disinfecting this immense quantity of an article that has always been deemed highly susceptible of retaining the infectious effluvia, not one person seems to have been infected in consequence.

Of sixteen English vessels laden with cotton, which sailed from Alexandria from the beginning of January to the end of June, eight had the plague on board; and yet their cargoes did not prove more dangerous than those of the non-infected vessels.

Besides this very conclusive evidence, the Commissioners mention upon official authority that, since the year 1720, not one of the porters employed at the lazaretto of Marseilles in discharging and landing the cargoes of suspected ships has ever caught the plague.

The *conclusion* is therefore fairly forced upon us that

"There is nothing to prove that articles of merchandise can transport the disease beyond epidemic foci."

The division of objects of merchandise in the French lazarettos into three classes, according as they are (believed to be) susceptible, doubtfully susceptible, and non-susceptible of infection, is the most arbitrary and ridiculous thing imaginable; nor is it easy even to form a conjecture what possibly could have led any set of reasonable men to adopt it. Tallow and wax, for example, are declared to be non-susceptible objects; but when made into candles (from the wicks, we suppose), they are susceptible! Pieces of old copper and other metals are conductors of the pestiferous poison; wood and other porous substances are not! Truly, as M. Prus remarks, the classification can only be regarded "as the result of most imperfect observation and of antiquated traditions prompted by fear and prejudice." It is utterly discreditable for any enlightened government to retain and act upon it.

The great point is to determine whether the clothing and baggage of plague-patients are capable of communicating the disease in our ports. If the decision upon this subject be in the affirmative, then certainly it will be right to ascertain what other objects possess the same property; but should it be in the negative, it is scarcely necessary to say that the entire catalogue of interdicted articles must be swept away.

It is quite unnecessary, as a matter of course, to say a single word respecting the comparative value of different modes of disinfection that have been proposed at different times. Most of the substances used in fumigation are utterly worthless; some of them are dangerous, and therefore inexpedient. Chlorine and its preparations are unquestionably the safest and best.

Conclusion.—"The study of the means best fitted to disinfect baggage, clothes, and articles of merchandise, remain still to be made.

"To be rational, before researches on this subject are undertaken, it should be proved that these different objects are really capable of becoming charged with the principle of the plague."

Chap. X.—*Can the plague be transmitted by pestilential miasms, beyond or away from epidemic foci?*

When, on board-ship or in a lazaretto, the plague is communicated from an infected person to one in health, it is obvious that it must be always difficult, and often impossible, to say positively that the disease has been owing to direct contact with the sick; for, whoever has been so near a patient as to have touched his body, must have inspired the pestilential atmosphere that is around him. How, then, shall we decide whether the infection has taken place by the skin or by the lungs? The task is indeed not easy, if indeed it be possible. But if we consider, on the one hand, that numerous observations have clearly shown that immediate contact with plague patients have not given the disease, and, on the other hand, that residence in a focus of pestilential infection, but without any contact with the sick, has imparted it, we are surely almost compelled to admit the following *conclusions*, viz.

"1. The transmission of the plague by pestilential miasms is a proved fact.

"2. The transmission of the disease by immediate contact with the infected is not a proved fact."

It has been already seen, from the experience of the Marseilles lazaretto, that, whenever there was plague existing in a ship, the stay of the vessel in the port became dangerous not only for the crew and passengers, but also for the health-officers who were sent on board, and who, it will readily be believed, most carefully avoided all contact with the sick or with any suspected object. Too often the vessel became a genuine focus of pestilential infection. The air, loaded with the miasms that always emanate from the bodies of the sick, acts as a poison to all who inhale it. This infected state or atmosphere on board a vessel may continue for some time, after every plague-patient has been removed from on board. We are therefore fairly warranted in concluding that "the plague is transmissible by infection (in other words, by the atmosphere being charged with pestilential miasms from the bodies of the sick) beyond or away from epidemic foci, as we have already seen that it is so in epidemic foci, and in countries where the disease is endemic."

To this general conclusion it may be useful to append the following three which are but as corollaries from it, as upon each of them certain quarantine measures are based.

"1. It follows from the facts adduced in the preceding chapters relative to the transmissibility of the plague, alike within and at a distance from epidemic foci, that plague-patients, by vitiating the air of places wherin they are confined, may create foci of pestilential infection that are capable of transmitting the disease.

"2. Foci of pestilential infection may persist in a place after the removal of plague-patients from it.

"3. Foci of pestilential infection once formed in a vessel, by the presence of one or more plague-patients on board, may be transported to a great distance."

CHAP. XI.—*Is sporadic plague transmissible either by the infected themselves, or by foci of infection which they may form?*

This question has already received a partial answer; but it will be useful to recur to its consideration in this place. The grounds upon which almost all the medical men in Egypt have adopted a negative opinion on the matter, are as follow:

After the epidemic of 1835 had ceased, the attention of these gentlemen —divided, be it remembered, in opinion as to the general question of the transmissibility of the plague—was naturally directed to the sporadic cases which occurred in the latter half of that year, and also in 1836, 37, and 38. From June 1835 to the end of December 1838, 649 cases of sporadic plague were observed in Alexandria. Now it is admitted by all that, of these 649 cases, 646 did *not* transmit the disease to any of the persons who had waited upon, and had frequently touched, the sick. It is therefore in reference to the three remaining cases only that any difference of opinion has existed; and with respect to these three cases, we think that no unprejudiced reader will hesitate to say, after carefully perusing the reports, that they are very far from proving that the disease was communicated, in a single instance, from one patient to another.

Here it deserves enquiry whether the cases of plague that have at any time been imported into Europe have, or have not, been brought by vessels which had left the producing countries of the disease while a pestilential *epidemic* was prevailing. The imported cases of the plague have never, we believe, been sufficiently considered in this point of view, although it bears very obviously on the question of the transmissibility of *sporadic* plague. The examination of the histories of the ten importations of the plague into Marseilles since 1720 has led the reporter to believe that, in every one of these instances, the pestilence was raging epidemically in the ports whence the vessels had come. This interesting fact certainly does not prove that sporadic plague is not transmissible; it only shows that none of the numerous vessels, which have left Egypt, Syria, or Turkey since the year 1720, at a time when these countries suffered only from sporadic plague, has ever imported the pestilence into Marseilles. This consideration, coupled with the remark of intelligent and trustworthy observers in Egypt that the cases of sporadic plague, which have occurred from July 1835 to the beginning of 1839, have in no instance transmitted the disease, deserves the serious attention of all medical men and legislators. M. Brayer also has come to the same opinion respecting the sporadic plague at Constantinople.

Conclusion.—"Patients affected with sporadic plague do not seem to be capable of producing foci of infection sufficiently active to transmit the disease."*

CHAP. XII.—*Is the plague more or less readily transmissible according to the intensity of the epidemic, the different periods of its course, and the or-*

* In a subsequent part of the Report, however, when treating of quarantine restrictions, it is stated that "the non-transmissibility of sporadic plague is not yet sufficiently determined by experience to warrant us in founding a sanitary measure upon it."

ganic susceptibilities of the individuals exposed to the action of pestilential miasms?

There cannot be a doubt for a moment that different epidemics of this as well as of other pestilences, exhibit very different degrees of intensity or malignancy, as evidenced by the varying amount of mortality produced in different seasons. Now, a question here arises, whether the risk of infection is at all proportionate to the degree of this malignancy. It will be obvious that it is scarcely possible to determine this point within the range or operation of the general exciting causes of the pestilences, or, in other words, of the epidemic focus. The observations, to be at all satisfactory, must be made on cases occurring beyond, or at a distance from, such a focus; for there, the miasms exhaled from the bodies of the sick will operate alone. The results of past experience seem unquestionably to indicate that the liability of such cases to propagate themselves is in direct relation with the intensity of the epidemic at its place of origin. The greater this intensity, the more readily the disease is communicable; and the reverse holds true, in proportion as this intensity diminishes. When the plague ceases to be epidemic, its transmissibility appears to cease altogether.

The period, too, of the epidemic has a decided influence on the force of the transmissibility of the disease. We have already considered this point, and need not dwell upon it here.

What has been said respecting the period of the epidemic in this point of view, is equally true of the period of the disease. Larrey was of opinion that the plague was not communicable during its first period or stage. Dr. Lacheze maintains that the disease ceases to be so after the second stage; that is, after the fourth or fifth day from its invasion. In the case of the disease, as in that of the epidemic, it is the second period which is most dangerous. Moreover, the influence of the organic dispositions or susceptibilities of individuals, in other words of their general state of health and their idiosyncrasies, either in promoting or in counteracting the liability to infection, cannot be disputed by any one. Much unquestionably depends upon the hygienic regime that is followed. Whatever tends to enfeeble or deprave the powers of life, renders the system more liable to infection. Hence excessive fatigues, the want of proper nourishment and the abuse of spirituous liquors on the one hand, and luxurious dissipation and excessive venery on the other, are all powerfully predisposing causes. Larrey observed that any tendency to the scorbutic diathesis rendered the system unusually susceptible, and that all such cases proved very rapidly fatal. We have already seen that race, nationality, sex, age, the circumstances of being acclimated or not, &c, have all some influence in aiding, or otherwise, the tendency to be affected by general epidemic causes. The same is the case with respect to the action of pestilential miasms.

CHAP. XIII.—*Is there reason to believe that the plague, when imported from the East into any European port, may be communicated to a sufficiently large number of persons to give rise to a pestilential epidemic?*

The medical men of Egypt answer this query in the negative. Their opinion is based on the often observed fact that, when plague-patients are transported to places not subject to the pestilential constitution, they

die or recover without transmitting their disease to any one. If the infected, as we have seen, cannot communicate the disease to the inhabitants of certain places in Upper Egypt, how shall we believe that, when transported from Egypt to France, it will possess a power of transmission so strong as to occasion an epidemic?

Some observers—and Dr. Lacheze is of this number—have indeed remarked that, in certain cases and in certain localities not subjected to a pestilential constitution, the disease has been communicated to a few individuals; but that these latter in no instance transmitted it to any one, notwithstanding the most free and intimate intercourse. Dr. Lacheze does not admit that the plague can ever, without a pre-existing epidemic influence, attack a sufficiently large number of persons to constitute a public calamity; and he insists that, wherever it has committed great ravages, there has uniformly been a pestilential constitution prevailing at the time. Sydenham, too, was of this opinion; for we find it distinctly asserted in his writings that, however frequent the importation of the plague might be into England the disease would assume an epidemic character only every thirtieth or fortieth year; because then only would it find the atmospheric conditions and the organic predispositions that are favourable to its development and propagation.

This doctrine is very generally received in Egypt in the present day and its truth has already been recognised by many of the most intellectual physicians in Europe. The legitimate inference from its public recognition would be that vessels arriving from infected ports, and having the plague on board, might be permitted to land the sick at any place not subject to the epidemic pestilential influence, without any risk to the people of that place, whether the patients recovered or not. But then it must be remembered that, until we are better acquainted than we are yet with what are the conditions of the soil and the atmosphere, which, in Europe, may give rise to a pestilential constitution, and what is the meteorological condition in which an imported plague may be liable to become diffused, prudence will authoritatively require that the very same measures should be taken in the ports of France against the possibility of the propagation of the pestilence by the infected, as if there was no room for doubt that they may become the cause and starting-point of a pestilential epidemic.* This caution is more particularly necessary in the case of Marseilles than of any other place in France, in consequence of its many local sources of insalubrity.

This great sea-port presents—in the circumstances of its climate, of its harbour being choked with filth of all sorts and containing an admixture of salt and fresh water, of its large working population, of its being hemmed in on all sides by mountains which prevent the free circulation of the air, and lastly of its proximity to large ponds—the very conditions that are favourable to the development of the plague.† It is a remarkable thing

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*This is one of the conclusions of M. Prus, the justice of which has very fairly been impugned.—*Rev.*

† *La topographie medicale de Marseille par M. Ducros, medecin en chef de l'Hôtel Dieu de Marseille*, 1837.

that the two cities of Europe which, after Constantinople, have suffered most from this pestilence are Venice and Marseilles:—Venice, which by its lagunes, its filthy canals, the moist heat of its climate, and by the wretched state of a large portion of its inhabitants, combines most of the causes which engender spontaneous plague, and may therefore, *a fortiori*, promote the diffusion of the disease when imported; and Marseilles, which in this respect approaches far too near to Venice.

Here we should not fail to remark, that sufficient attention has not hitherto been paid to the cases, certainly very numerous, where the plague conveyed to a country has become spontaneously extinguished, for very want of being able to transmit itself. it would be very useful to ascertain with precision what are the local causes which appear to resist the transmission and propagation of the disease. This work has only just been commenced in respect of a few places in Upper Egypt.

Conclusion.—"If it has been proved that the existence of a pestilential constitution in a country, into which the plague is imported, is necessary for the transmission and propagation of the disease, it seems nevertheless certain that imported plague will not exercise any great ravages, if it does not meet with, in the character of the climate, atmosphere and population of the place, the conditions that are favorable for its development."

FOURTH PART.

What is the ordinary or exceptional term of the incubation of the plague? —in other words, how long may the plague remain hidden, so to speak, in an infected individual, before it manifests itself by more or less distinct symptoms?

This period is believed to vary considerably according to the stage of the prevailing epidemic, and other less influential circumstances. The variations are nevertheless confined within certain limits, which it is important to ascertain; for upon them should depend, in a great measure, the duration of quarantines.

All observers have remarked that, when a pestilential epidemic commences in a town, the incubation of the disease is often extremely short. We read of attacks of the plague proving fatal within a few hours, nay within a few minutes: these are the cases, which have been truly called "pestes foudroyantes."

In the second period or stage of the epidemic, the usual term of incubation is from three to five days. It is about the same in the third stage.

Upon all these points there is little or no discrepancy of opinion. It is only when we endeavour to determine the longest duration or lapse of time that may be fairly admitted for certain exceptional cases, that we find the statements of different writers to disagree. Some, and these constitute the large majority, maintain that the term of incubation never exceeds eight days; others think that this term may be prolonged to ten days, and sometimes, although very rarely, a few days more. Dr. Grassi, in his reply to certain interrogatories addressed to him in 1839 by the English minister, says upon this subject:

"In the course of several years, some thousands of persons of every age,

sex and condition, were condemned to undergo a quarantine of observation of six days for having been exposed to infection. The disease made its appearance in many of them during their isolation, but in no one case beyond the sixth day. This is an observation which I have made with great attention."

On his representations to the Egyptian government, the quarantine, which had before been eleven days in the lazaretto at Alexandria, was reduced in 1842 to seven days.

The experience of other medical men in Egypt has, on the whole, confirmed the truth of M. Grassi's observations.

This gentleman assures us that, among the multitude of people that left Cairo during the epidemic of 1835 and fled into Upper Egypt, which continued healthy, the disease manifested itself in a few persons; but, in no one instance more than the eight days after their quitting the city.

The observations made, in the course of the same year, by the professors of the medical school at Abouzabel lead to the general conclusion, that the period of incubation never exceeds six days. According to M. Segur, it is never more than eight. If, too, we examine what has occurred in vessels that have had plague on board after leaving an infected port, we shall find that on every occasion the disease has shown itself within eight days from the time of sailing.

With respect to those cases where it has been alleged that the period of incubation exceed eight or ten days, M. Prus does not consider them at all worthy of acceptance, in the sense in which they have been adduced; for no account has been taken by their narrators either of the epidemic action, or of the miasmatic infection which always acts an important part in places that are badly ventilated; in a ship for example. When a vessel becomes a focus of infection in consequence of having had a number of plague patients on board, the persons, who remain in this focus and breathe the vitiated air, may, and often do, contract the disease at intervals of a longer or shorter space of time. It is obvious that, under such circumstances, the sailors and passengers may be seized 15, 20, 30 days and even upwards, the one from the other, without the inference being at all warranted that the incubation of the malady has existed in any one case for more than six or eight days. We in truth cannot make out when the pestilential miasms began to act on those who had received them into their systems, in such a manner as to occasion the development of the disease.

Conclusion.—"If it be true that a fixed and absolute term cannot be assigned to the incubation of the plague, it seems, nevertheless, to be clearly proved by well-established facts that, at a distance from countries where it is endemic and beyond or away from epidemic foci, the disease has never broke out in persons who have been exposed to its influence after an isolation of eight days. The few facts, which might be regarded as exceptional to this rule, are all susceptible of another interpretation."

The concluding part of the Report is occupied with an account of the Quarantine regulations now in force in the different ports of France on vessels arriving from suspected countries, and of the changes which the Commissioners propose to be adopted. We have no intention to allude to the first of these matters; and, with respect to the second, our remarks shall be very brief. It is suggested, and the suggestion is cer-

tainly a valuable one, that medical men should be appointed by Government to reside in the different places where the plague is most apt to exist, in order that regular reports as to their sanitary condition might be transmitted home, and for the purpose of officially examining the state of every vessel, with respect to her passengers, crew, cargo, accommodations, &c., about to leave the country for any French port. The resident consular agents would then be better qualified, by the accurate professional details thus acquired, to determine when to grant and when to refuse clean bills of health to the vessels of their own nations. The affixing of the medical certificate to the ship's bill of health would also enable the officers of the port, where the vessel arrived, to judge more correctly of all the circumstances connected with her past and present condition. The Commissioners propose that no clean bill of health should be granted when a pestilential epidemic prevails or appears to be impending in the place of departure, or even when the number of sporadic cases of the plague are so numerous and severe as to occasion apprehensions that the malady may spread. In all other cases, a clean bill might be granted at the port of departure.

Although believing that the clothes and other effects of the sick are not capable of transmitting the plague, the Commissioners suggest that, until further experiments are made upon this subject, all those articles should be duly ventilated during the voyage; or, what would be better, that the trunks and boxes of the passengers and crew should be secured and stamped (plombées) at the port of departure, and not be opened until this can be done in a French lazaretto, where they should be well ventilated for three days or so.

The following are the suggestions of the Commissioners in respect of the Quarantines which they deem advisable, in lieu of those now existing in French ports.

1. For vessels having a medical man on board, and coming from Egypt, Syria, or Turkey with a clean bill of health, the quarantine to be 10 full days, *to date from their departure*, provided no case of plague or of any suspicious disease has appeared during the voyage.

The quarantine to be 15 full days, *to date from their departure*, for the same vessels arriving with a foul bill of health, if neither plague nor any suspicious disease has occurred on board during the voyage.

2. For merchant vessels arriving with a clean bill of health, but having no medical man on board, a quarantine of observation to be for ten full days, *to date from their arrival.*

When the same vessels shall arrive in a port with a foul bill of health, but without having had either plague or any suspicious disease on board while at sea, a most strict quarantine to be for 15 days, *to date from their arrival.*

If a case of plague, or of suspicious disease has occurred on board during the voyage, or should occur after the arrival of the vessel in a French port, the vessel to be subjected to a rigid quarantine, the length of which to be determined by the sanitary authorities of the port. The passengers and crew to be transferred to the lazaretto, and detained for 15 days at least, and 20 at most; the cargo to be unloaded and freely exposed to the air; the vessel herself to be well cleansed out, purified, and left

empty for a month at least; and health-guards to be stationed near to, but not to be put on board of, the infected vessel.

With respect to the cargo, since it has not been proved by any authentic fact that articles of merchandise have the property of retaining pestilential miasms, and of transmitting the plague, the Commissioners confine themselves to merely recommending the employment of such means as are most simple and least oppressive or vexatious to commerce.

After exposing the barbarous absurdity of the plan followed at the Marseilles lazaretto, even up to the present day, in the treatment of any one affected with, or supposed to be affected with, plague, it is suggested not only that plague-patients should be treated and waited upon like any other patients, without using the cruel and ridiculous precautions that are still in force, but also that *post-mortem* examinations should be made in all fatal cases.

Should the plague break out in any house, town, or district, the rules to be followed are simple and obvious. The sick should be immediately removed from the dwelling, where the disease has appeared, to a healthy well-aired spot that has been fixed upon; while all the other inmates also should be compelled to leave it, in order that it may be duly cleansed, purified, and ventilated. On no pretext should the infected be confined and inclosed along with the healthy by sanitary cordons, or other compulsory measures, in the very place where the pestilence exists; this were only aggravating the mischief and rendering it more concentrated. The great object should be to attenuate the virulence of the atmospheric poison by separating the sick from the healthy, and by keeping both in airy, elevated, situations. If necessary, tents and simple barracks should be established, and the healthy compelled to dwell in them, at a distance from the focus of infection.

The Report, of which we have now given so extended an analysis, was elaborately discussed at eight or nine sittings of the Academy of Medicine, in the months of May, June, July and August. Notwithstanding the labour expended, we do not think that either much new matter was added to the details which the Report itself contains, or that any of its more important positions were successfully impugned. The majority of the speakers, as well as of the members of the Commission, declared their cordial concurrence with the results of M. Prus' researches, while the minority were divided into two sections that maintained very opposite opinions; the one advocating almost all the extravagant notions held by the ultra-contagionist party, and the other loudly proclaiming the non-communicability of the plague under almost any circumstances, and calling for the entire abrogation of all quarantines. The reader will be able to judge for himself of the value of the extreme opinions of both parties.

By far the most valid objection to the Report is, that the practical recommendations proposed in its concluding part are not altogether consistent with the doctrines professed before: the quarantines recommended are unnecessarily stringent and severe, if the conclusions in the body of the Report be correct.

From want of space here, we must refer the reader to page 525 for the review of the Parliamentary Correspondence.

NOUVELLES RECHERCHES SUR LES BRUITS DES ARTERES ET APPLICATION DE CES RECHERCHES A L'ETUDE DE PLUSIEURS MALADIES. Par le Dr. *J. H. S. Beau*, Medecin des Hôpitaux, &c, [Archives Generales, 1845–6.]

New Researches upon Arterial Murmurs, and the Application of such Researches to the Study of several Diseases. By *J. H. S. Beau*, M.D.

DR. BEAU has just completed a series of interesting papers upon the subject of arterial murmurs, a succinct but complete analysis of which we propose to offer in the present article. They are, indeed, but a continuation, or rather extension, of a former essay upon the same subject published in the *Archives Generales* in 1838,* in which the two following propositions were maintained: *viz.* 1. Arterial murmurs depend upon the excessive friction which the blood exerts upon the arterial parietes, when this liquid exists in too considerable a quantity relatively to the capacity of these vessels. 2. Whenever, in any disease, arterial murmurs are heard which cannot be explained by a local lesion of the vessel, we may conclude that this diseased condition is accompanied by a superabundance of the mass of the blood, *i. e.* by a *polycæmia*. The present series of essays is devoted to a fuller development of these propositions.

I. HISTORY OF ARTERIAL MURMURS.

This is divisible into a *descriptive* and *theoretical* portion, each of which occupies the author at some length, as fundamental to his examination of the various diseases in which the murmurs are heard. A. The *description* embraces that of the form, the sèat, and the rhythm of the murmurs.

1. *The Forms of Arterial Murmurs.*—The varieties of these are nearly the same as those of the heart. Thus there is the short, instantaneous murmur, giving to the ear the sensation of a true impulse, which may be termed the *normal* arterial murmur; but this may be more or less prolonged, imparting an idea of friction, possessing the same shades of difference as the normal sounds of the heart, and is then an *anormal* murmur. If, however, the *normal* sounds of the heart and arteries resemble each other in the sensations they impart to the ear, the perception of these latter is not very common. It is true we may usually hear normal sounds over the carotid and aorta, but, if we conduct our examination very accurately we shall find that these sounds are generally but the reflection (*retentissement*) of the first sound of the heart, heard sometimes over the aorta, and at others over the carotids. We can only be certain of the arterial sounds when we hear them beyond the reach of this reflection, as

* Vide Med. Chr. Review, Vol. xxix., p. 569.

e. g. over the femoral artery, or especially over the abdominal aorta, when the parietes of the abdomen are thin enough to admit of satisfactory auscultation. These vessels usually offer a pulsation or heaving, of a notable intensity, but this is very rarely accompanied by any sound. The normal sound is then exceptional, and even when it is well marked it is usually connected with pathological conditions which furnish also anormal murmurs—so that in such arteries we may have simultaneously the one and the other.

The *anormal* murmurs present many shades which resemble the sounds whose names they have borrowed, as blowing-sound, sawing-sound, &c.; but these differences are by no means so important as they might seem, as they do not indicate corresponding varieties of pathological conditions, but may be dependent upon the different intensity of the same cause, and may be transmuted into each other according to the diminution or increase of such intensity, and often under the influence of ephemeral circumstances, as will be presently seen.

A marked *vibration* or *thrill* is usually conveyed to the finger placed over an artery that furnishes an anormal murmur, varying with the intensity of the sound. Whenever such vibration is felt we are sure of detecting a marked anormal murmur by means of the stethoscope.

2. *Seat of Arterial Murmurs.*—Arterial murmurs may be divided into *Local* and *General*. The former are manifested over a very limited portion of the arterial system, and result from some circumscribed affection, such as aneurism, aneurismal varix, erectile tumours, or the compression of arteries by tumours. The latter may be heard over many parts of the arterial system, and result from a general affection; and it is to these *General Anormal Murmurs* that we wish especially to direct attention. Although the general murmurs may usually be heard over several portions of the arterial system, this is not always the case; for oftentimes the murmur induced by even a general affection may be confined to one particular artery, which in such instances of limitation is invariably the same, viz., the *carotid*. We may add that the general murmurs frequently coincide with an anormal condition of the first sound of the heart.

3. *Rhythms of Arterial Murmurs.*—The arterial murmurs present varieties of these which it is important to recognise. 1. The murmur is only heard at each arterial dilatation (*the simple intermittent murmur.*) 2. It may be heard twice during each revolution of the heart's action, the first sound being the stronger; between which and that which succeeds it there is a distinct pause (*the double intermittent.*) 3. The murmur may be heard throughout the whole continuance of the revolution of the heart's action (*continuous murmur.*) 4. It may not only be continuous, but at each manifestation of the pulse it may be redoubled or reinforced (*continuous* and *redoubled murmur*: *bruit de diable* of Bouillaud.) In general affections of the arterial system, in which several of the arteries are the seats of anormal murmurs, those which are nearest the heart usually affect one of the three last varieties of rhythm, while those which are more distant only manifest a simple intermission.

Circumstances influencing the production or modification of the Murmurs. —For the murmurs to be heard, the artery should be near the centre of the circulation. When they are heard in several vessels they are always louder in those of the neck and trunk than in those of the limbs, while, when they are confined to one vessel, that is never an artery of the periphery. For the murmurs to be heard the artery must be superficially placed, as the carotid, femoral, subclavian, brachial, or the aorta, in a retracted state of the parietes of the abdomen. When a deep-seated artery like the iliac furnishes a murmur, it is only through its being subjacent to a tumour, which conveys the sound like a stethoscope. The *Carotid*, being both superficial and near the heart, is that in which murmurs are most frequently heard. The stethoscope should be placed between the separations of the sterno-cleido-mastoideus, and the chin raised and turned to the opposite side, so as to impart a sufficient tension to the parts. The degree of *pressure* exerted upon the instrument should vary, being considerable when the sounds are obscure, and very slight when they are intense, for anything beyond simple *apposition* will then cause them to disappear. Whatever, too, operates a change in the more or less superficial position of the carotid will modify the murmur. Thus, it is heard with more intensity (and sometimes exclusively) on the *right side*, owing to the deeper-seated situation of the left carotid, except in those rare instances in which this is more superficially placed than the right. If we apply pressure against the artery above the point where the stethoscope is placed, so as to thrust the vessel into the deep angle at the side of the larynx, the murmur either ceases or becomes diminished in intensity according to the degree of pressure employed. The same results follow if we make due compression with the instrument itself. By another mode of proceeding, however, we may produce opposite effects. The carotid, and especially the left one, may lie deeply hidden in the angle at the base of the neck. If the index finger be passed in front of the anterior insertion of the sterno-mastoideus into the little fossa above the median line of the sternum, the vessel may frequently be raised by it into a more superficial situation under the mouth of the stethoscope, and sounds before unheard may be distinguished, and others of an obscure character rendered more distinct. The artery is, however, sometimes too deeply situated to admit of this manœuvre, and if it be employed upon a vessel which is superficially seated and already gives distinct sounds, the pressure it produces will be the means of diminishing or abolishing them. M. Donné has observed that traction of the larynx diminishes the carotid murmur, and attributes this to the point of support having been removed from the vessel; but the traction in fact withdraws the artery to without the sphere of action of the stethoscope, and the pulsations of the vessel may also be observed to be less and less perceptible in proportion to its amount. Donné has likewise remarked that when the patient makes any sudden muscular exertion, the murmurs are less distinctly heard or suspended. This, in fact, arises from the distended condition of the jugular and other veins during such exertion by reason of their prominence, leaving the artery in a less superficial position than it before held in its angular space. If the effort made be much less, the dimunition or alteration of rhythm is likewise proportionably less. In some subjects no difference in the murmurs occur in consequence of the

distension of the jugulars during exertion being insufficient to displace the carotids. Hope has noticed that the arterial murmurs are much more full and intense during and at the end of inspiration than they are in expiration. The veins, in fact, are flattened in the one action and distended in the other. It is, too, on account of the different degree of repletion of the veins that the murmurs are heard so much more distinctly in the erect, or sitting, than in the recumbent posture; and M. Vernois has observed, that if the head hangs down upon the trunk, they disappear altogether. Many individual differences, regarded as singularities in the auscultation of arterial murmurs, depend simply upon the variations of organization, or of the anatomical relations.

Other circumstances may modify the murmurs without causing changes in the anatomical relations of the vessels. Bouillaud observes, after Laennec, that when an individual engages in active muscular exertion the murmurs are increased in intensity, and often change their form from blowing to sawing sounds, &c. But it is also important to note the fact of such exertions likewise markedly *increasing the force of the pulsations*, not only of the carotids but of every artery. What proves the exactitude of the relationship is the fact that it holds good under reverse circumstances; for the more the pulse is diminished in volume the less intense are the arterial murmurs. If the large cupping-glass of M. Junod be applied to the lower extremity the diminution of pulsation and murmur will be found to be coincident with that of the volume of blood. Three patients, in whom arterial murmurs had been very audible, suffered from hæmorrhage, which much diminished the violence of the pulse, and, as long as this continued small, the murmurs were quite imperceptible in two cases, and nearly so in the third. Two old cachectic women, whose carotids furnished intense murmurs, were greatly reduced by diarrhœa, which led at once to a rapid fall of the pulse and disappearance of the murmur. When a chlorotic patient falls into a state approaching to syncope, during which a very small pulse exists, the murmur infallibly diminishes or disappears. The cases noticed by Laennec and Bouillaud in which the murmurs become suspended or changed from time to time, may mostly be brought within one of the above categories. Thus, a chlorotic and hysterical woman may have a paroxysm of hysteria, during which her muscular efforts may suspend the murmurs by distending the jugulars. Sometimes, again, the action of the heart is defective from mental emotions, the pulse is consequently small and the murmur absent.

B. *The Theoretical Portion.*—As a matter of convenience, we may divide the *anormal murmurs* into local and general. It is an arbitrary proceeding, inasmuch as they depend upon an identical condition; but the local affections of the arteries being more accessible and intelligible, the explanation of the anormal murmurs is more easily made by their agency. The local affections which give rise to murmurs are, *organic narrowing of the artery, its compression, aneurism, erectile tumours, and aneurismal varix.*

1. The mechanism of the production of the murmur in instances of *narrowing of the calibre* of the artery by thickening, concretions, &c., is simple. The fluid exerts an exaggerated degree of friction against the

constricted portion, inducing the anormal sound and a thrilling, perceptible to the hand; the mechanism being identical with that which induces the cardiac murmur in instances of narrowing of the orifices of that organ.

2. *Compression* of an artery will induce a murmur just in the same manner.

3. *Aneurismal Tumours* induce murmurs and thrilling in one of the above modes. Sometimes the flow of blood is impeded by the organised coagulum, and sometimes by the compression of the arterial tube by the tumour itself. Another obstacle has not been sufficiently noticed, *viz.* the blood which the aneurismal cavity habitually contains. The tumour is always full of blood, which in a healthy state of the tube should be propelled on towards the capillaries, and which presents an obstacle to any additional quantity of blood coming from the heart. There is then a sum-total of the fluid too great relatively to the capacity of the vessel, and an exaggerated friction against the walls, furnishing the murmur and vibration, results.

4. *Erectile Tumours* are usually the seat of marked anormal murmurs and vibrations. On dissection they are found to be pervaded by thin, dilated, flabby, arteries, containing coagula—they having become, as in aneurism, deprived of their middle coat and contractile power. The distension of each vessel is at its maximum as often as a new supply of blood arrives within them, and the conditions for the formation of murmurs are then analogous to those prevailing in aneurism.

5. *Aneurismal Varix.*—The murmur and vibration proceeding from this exists not only from the opening of communication between the vein and artery, but is prolonged along the whole length of the former. The vein by its organization is unable to re-act upon the arterial blood which enters it, and the murmur and vibrations result from its consequent distension. If these are less along the continuity of the vein than over the aperture, it is because the arterial wave which penetrated the aperture with violence, so to say, exhausts itself against the obstacle which the venous blood offers to its progress.

From the above observations we may deduce this general principle, that the *primary condition of the production of arterial murmurs is a defective proportion between the wave of blood and the calibre of the artery.*

MM. Andral and Gavarret endeavour to explain the existence of these murmurs by their dependence upon *a defective proportion of globules in the blood.* Thus, if the globules are but 80 in the 1000 parts, the murmurs are constantly heard; from 80 to 100 they are heard frequently; but they are never observed, in connection with changes of the blood, when the proportion exceeds the normal one of 127. There is doubtless some general coincidence between the murmurs and proportions of globules; but do the murmurs depend upon this condition of the blood itself, or upon a disposition of the arterial system which usually accompanies it? We believe the latter. In fact, if the murmurs depended essentially upon the diminution of globules, they should always co-exist with blood so impoverished, and then we ought to be able to diminish the volume of the pulse

in a chlorotic subject by means of Junod's cupping-instrument without diminishing or abolishing them, as by it no change in the composition of the blood could be effected. Numerous experiments, however, have proved that, by diminishing the volume of the pulse, we proportionally diminish the murmurs. On the other hand, MM. Becquerel and Rodier relate the cases of two girls who presented the *bruit de diable* in the carotids, the blood being of its normal composition. Similar observations will apply to the general rule laid down by M. Bouillaud, that arterial murmurs are found only in individuals *the density of whose blood is below* 6° of Beaume's areometer. The arterial murmurs, in fact, depend upon a condition of the arterial system which is found when the density of the blood is thus diminished.

Experiments, consisting of the injection of water into the veins also produce murmurs, and that in proportion to the degree of distension caused and the force employed. If the local affections of arteries and experiments agree in furnishing the same formula, viz. *a defective proportion between the wave of blood and the capacity of the vessel*, can it also be deduced in the case of murmurs connected with a *general affection of the arteries*? When we find the murmurs are much augmented by whatever distends the larger arteries and suppressed by diversion of the blood to the periphery, we may believe that in the mean of these two extremes there is a state of superabundance of this fluid, which constitutes the disease of which the arterial murmurs are a symptom. To illustrate this point M. Beau cites the phenomena attendant upon *insufficiency of the aortic valves*, and upon *chlorosis*; but as these are detailed in the second part of the Memoir we may defer noticing them. But before entering upon these we must advert to a long digression upon the

Alleged Jugular Murmur.—During forcible expiratory efforts the blood is driven from the central into the peripheral veins. In many persons, otherwise healthy, this reflux is attended with the production of anormal sounds and thrilling in the femoral vein or internal jugular. They are better heard when a sudden muscular effort is made rather than noisy expiratory acts, as coughing, &c. which may obscure them. These venous sounds, like the arterial ones, depend upon the disposition which exists between the volume of the blood and the câlibre of the vessel. M. Beau adduces various arguments in refutation of the opinion entertained by Drs. Hope and Ward, and adopted by M. Aran, that the continuous murmurs heard in the carotid region have their seat in the *veins* of the neck. He maintains that the carotid is their true seat, and that they become more or less audible accordingly as the flattened or distended state of the jugular vein allows this artery to become more or less superficially placed. In fact, the only true *venous murmurs* that are produced result from aneurismal varix, or are temporarily induced during acts of expiration in the femoral or jugular veins.

II. THE VARIOUS DISEASES WHICH ARE CHARACTERISED BY THE EXISTENCE OF ARTERIAL MURMURS.

It is intended to show, by the analytical examination of symptoms, or

by experiments, that the arterial murmurs in these affections depend upon the presence of a superabundant quantity of blood; and to enquire into the pathological cause of such superabundance. The various affections are considered without any attempt at methodical arrangement.

1. *Insufficiency of the Valves of the Aorta.*

Insufficiency of the aortic valves forms a natural link between the local and general affections; for though the lesion is local in its nature, it leads to general derangement of the arterial system. As a consequence of its existence, there is a reflux of blood into the left ventricle during the pause in the heart's action. This, together with the blood coming from the auricle, constitutes a superabundance of the fluid during the next action of the ventricle. This cavity becomes dilated and hypertrophied, and the blood is propelled with an exaggerated friction into the aorta. In this disease the pulse is full on account of this superabundance, and hard in consequence of the hypertrophy. Sometimes the thrilling which is perceived over the carotids is extended even to the radial artery, and induces a vibratory pulse. In insufficiency there is (1), an anormal murmur heard during the second sound of the heart, opposite the aortic orifice, and produced at the point of insufficiency by the reflux of blood into the ventricle by reason of the elastic re-action of the aorta. There is (2), also an anormal murmur heard in the large arterial trunks, synchronous with the first sound of the heart, and generally accompanied by thrilling and considerable arterial pulsation. We are here speaking of simple insufficiency; for, if this become complicated with *contraction of the aortic orifice*, the wave of blood is impeded, and this last sound is absent. The anormal murmur arising from the reflux of the blood however remains, and is therefore the characteristic symptom of insufficiency. Indeed, we may say that, whenever the heart offers an abnormal murmur in its second sound, this certainly arises (except in pericarditis) from insufficiency of the aortic valves.

"There is an interchange of cause and effect worth observing. The elastic reaction of the aorta is the determining cause of the dilatation and hypertrophy of the ventricle; but in proportion as these become more considerable, so is the quantity of blood which re-flows into the heart larger. The wave expelled into the arteries becomes also larger and larger, and the arterial pulsations more marked. Such amplitude of pulsation cannot exist without dilatation of the calibre of the principal arteries. Thus the hypertrophic dilatation of the heart, which is first produced by the arterial reaction itself, becomes a cause of arterial dilatation. The mass of the blood also does not continue the same. It is in excess; but as the portion in excess only executes a to-and-fro movement between the heart and arteries reciprocally, it follows that it is placed as it were beyond the pale of the circulation."

2. *Anæmia following Loss of Blood.*

The state of anæmia may seem to present difficulties for the explanation of the arterial murmurs by a condition of *polycemia*.

"Let us first trace the principal marks of anæmia resulting from loss of blood. It is characterised by paleness of face, colourless tissues, smallness of pulse, and arterial murmurs. It depends alike upon the blood containing a large

proportion of serum, and the entire mass of this fluid being less than in its normal state. This definition, which seems to be correct, is nevertheless inexact; for, in fact, it confounds under one name, as the same morbid entity, two conditions which should be essentially distinguished from each other. I will indicate these conditions as an anticipatory recapitulation of the facts which will presently come under our notice.

"1. A notable abstraction of blood at once produces paleness of face, small pulse, &c. There are however *no anormal arterial murmurs*. The symptoms are produced entirely by the entire mass of the blood being below its normal quantity—a condition of *true anæmia*. 2. After a certain time, varying according to the abundance of the loss of blood (one or two days, &c.), the pulse acquires a greater volume, and becomes much fuller than prior to the loss of blood, and from this period anormal murmurs may be heard. The fulness of pulse and arterial murmurs are both dependent upon a superabundant reproduction of serum. The entire mass of the blood has become more considerable. The condition is that of *polyæmia*, but of a *serous polyæmia*."

If the loss of blood be very excessive, so as to throw the person into a state of excessive prostration, the *thirst* so usual after hæmorrhage will not be present, *drinks* are not taken, and the *excess of serum* not produced. The pulse will then continue small, and the patient sink without any arterial murmurs being heard. There is only one case in which the murmurs are heard immediately after the loss of blood, and that is when they were already audible before it took place—and not then if the hæmorrhage be very excessive. There are other symptoms indicative of polyæmia after hæmorrhage besides fulness of pulse, such as pulsative cephalgia, vertigo, dyspnœa, palpitations, &c.—all of which are increased by exercise and temporarily diminished by abstraction of blood. With such symptoms we have pallor and debility, because, although the blood is superabundant in quantity, it contains much too large a proportion of serum. Dupuytren has noticed the state of plethora, with pallor and debility following the frequent loss of blood in hæmorrhoids.

To decide the nature of the *anatomico-pathological condition upon which the state of serous polyæmia depends*, the author instituted two series of experiments. The first of these, consisting in bleeding dogs from day to day, was only confirmatory of the observations already made, by the induction of murmurs after the fifth or sixth bleeding, and their disappearance after additional bleeding until the animals had the opportunity of recruiting the supply of serum by drink. The other series was performed on rabbits, and an endeavour made to compare the condition of animals who had been subjected to repeated depletion with others in their normal condition. An examination of the heart in these animals disclosed important differences, explanatory of the cause of the large wave projected into the arteries of those who had been frequently bled. In animals, previously strong and vigorous, and killed at once by opening the carotids, the cavities of the heart, and especially the ventricles, were found nearly effaced by the complete contraction of the organ. But in rabbits killed in the same way, but previously enfeebled by repeated bleedings, the organ was found of one-third more than its proper size, its cavities manifestly dilated and often containing blood. With the dilatation there was also hypertrophy, for, notwithstanding the dilatation of the cavities, their parietes were as thick as in the normal condition, while the weight of the organ was aug-

mented by one-fifth or one-sixth. The origins of the pulmonary artery and the aorta were likewise notably dilated. We thus see that dilatation with hypertrophy (in practice it is familiarly known that hypertrophy may be produced when contraction of the orifices exist, notwithstanding the blood is defective in globules) may be rapidly developed under the influence of a poor, serous, fluid. This condition of the blood leads to an atonic and relaxed condition of the structure of the heart. A larger quantity of fluid being received into the dilated cavities, a condition of hypertrophy is essential for its propulsion.

Several passages are extracted from the writings of *Van Swieten*, *Lieutaud*, and *Lancisi*, shewing that the production of plethora after free bleeding was not unknown to them. The author likewise observes that, if we admit the reality of serous polyæmia, we are led to *question the efficiency of Valsalva's mode of treating aneurism*, viz. the institution of frequent bleedings and low diet, in order to enfeeble the patient's strength, and that of the wave of blood entering the sac. Such treatment will effect the former of these objects, but will only increase the intensity of the arterial pulsation. To ensure success by this mode, we should have to debar the patient from drinks, but thirst would be insupportable, and the consequent debility be incompatible with the maintenance of life. But when we have reason to *fear* the *recurrence* of *bleeding from wounds of arteries*, *operations for aneurism*, *&c.*, the prevention of an undue development of the pulse by a temporary abstinence from drinks seems to be a rational practice.

There are *two symptoms common to true anæmia*, and to *serous polyæmia*, viz. *paleness* and *debility*. The first of these, in both cases, is derived from a diminution of globules. In *true anæmia* this coincides with a proportionate diminution of the other elements of the blood; but in *serous polyæmia* it is accompanied with a disproportionate increase of serum. In the latter, too, there is a puffiness with the pallor of the face, while in the former the features are sunken and contracted. In *polyæmia* the *debility* is much less considerable than in *anæmia*, showing that, although the reproduction of the blood is chiefly serous, such fluid will minister to some of the functions of the economy. *Syncope*, which is so characteristic of *anæmia*, is also sometimes observed in *polyæmia*, when, after muscular exertion, the blood arrives at the heart in too large quantities and surcharges it. It is then accompanied by palpitations and a sense of great weight, circumstances which do not prevail in anæmia. In both cases it arises from the heart being temporarily too feeble to propel its contents. The *duration* of these two conditions is very different. *Anæmia* is not of long continuance. At the end of a few hours the material taken as drink finds its way into the blood-vessels. We might almost say, "*Nature has a horror of anæmia.*" *Serous Polyæmia* may be of a much longer duration, but we may say, as a general rule, that in about a fortnight the normal proportion of globules will have become restored under the influence of aliments. On other occasions, however, it may be prolonged for months or even years, the digestive functions having undergone important and permanent alterations, and the consequent defective assimilation preventing the reparation of the impoverished condition of the blood.

3. *Chlorosis.*

The author presents a general view of the symptoms attendant upon this disease: but we will only extract those of them bearing reference to the condition of the circulation.

"There is a sense of weight at the heart accompanied by frequent palpitations: the organ is voluminous as I have assured myself by plessimetry. Its pulsations are increased in force, and frequently furnish to the ear the metallic tone, especially during the existence of palpitation. The normal sounds are clear and sufficently intense to be heard over the entire chest. There is often a blowing sound accompanying the first sound of the heart. There are anormal murmurs in the arteries which are accompanied by thrilling, the pulsations, and especially of the carotids, being energetic and visible at a distance. The radial pulse, usually described as small and feeble, presents on the contrary *a more considerable volume than in the state of health*, the veins also being now distended, but of a less deep colour than normal. It has been said that the veins at the back of the hand are small in chlorotic persons, but they are still smaller in such persons in a state of health; and we must bear in mind that all the vessels are of a smaller calibre in a girl than in a man; and indeed the sole means of judging of their normal plenitude is to compare them during and after the cessation of the disease. * * * * * * * The proportion of globules of the blood in this disease is diminished from 127 to 38, while that of the serum has increased from 790 to 868. Little attention has been paid to the lesion of the solids, but it is admitted that the tissues are pale, slightly imbibed with serosity, and relaxed. It is said also that the heart is small and atrophied, but here preconceived ideas rather than observations have prevailed, for it is impossible that a heart shall be found small at an autopsy which percussion has proved to be developed.

"The full pulse, arterial pulsations, pulsatory headache, &c. of chlorosis depend upon the superabundance of the mass of blooed, for (1). They diminish immediately, and for a while, if bleeding or Junod's cupping-glass be employed. (2). They are increased by exertion which throws a larger quantity of blood upon the centre of circulation. The heart re-acts upon this and drives the blood with additional force into the arteries, producing the sensations spoken of. At last it is unable to continue such exertion and the violent palpitations are then replaced by syncope."

Various authors, as Sennertus, F. Hoffman, Boerhave, &c., have noticed the superabundance of serous blood in chlorosis. The small proportion of globules is not only the cause of the colourless state of the tissues, but also of the languor and debility of chlorosis. It induces a relaxation of the principal tissues, as the muscles, skin, arteries, &c., and leads to the hypertrophic dilatation of the heart. In serous polyæmia the defect of globules arises from prior abstraction of blood, but in chlorosis it is due to defective assimilation. The disorder of the digestive organs, therefore, plays an important part in determining the condition of chlorosis, inasmuch as it is the point of departure of the changes which the blood undergoes: but in a case of simple chlorosis such disorder may itself be produced by prior derangement of the functions of the uterus.

"It is needless to observe that the various symptoms thus classed in their pathogenic order, are more or less masked in different individuals, and they may not even be constantly met with. I may add that they react more or less upon each other, so as to pass from the condition of effect to that of cause. Thus the affection of the digestive canal, which is the cause of the change in the blood, may itself become increased by the diminution of the globules. The different

alterations, functional or organic, too, whose succession and dependence I have shewn, may so combine that the effects are almost coincident with causes, so as to constitute an assemblage of phenomena which are exhibited and increased simultaneously.

"If we cast a retrospective glance at the symptoms of chlorosis and compare them with those of post-hæmorrhagic polyæmia, we find that the organico-pathological condition is identical in both. There is the same lesion of heart and arteries, the same change in the blood, and the same symptoms dependent upon the pathological condition of the blood and its reservoirs. In chlorosis the symptoms originate in an affection of the digestive and uterine organs; in polyæmia there is nothing of the kind, unless it becomes indefinitely prolonged. If the subject be a menstruating female, an uterine affection may be observed; but instead of this being the point of departure, as in true chlorosis, it is only one of the consecutive effects. As to nervous symptoms, such as neuralgiæ, they are seldom observed in post-hæmorrhagic polyæmia, unless this is very long in being dissipated, in which case they may become as intense and as numerous as in true chlorosis."

4. *The Cachexiæ.*

M. Beau enters upon the description of various forms of cachexia as the *Miners*, the *African*, the *Saturnine*, *&c.*—exhibiting the fact that all these depend upon the presence of a blood too poor in globules and too great in quantity—this, however, resulting from antecedent affection of the digestive organs impeding the assimilatory powers of the economy. The symptoms too are precisely analogous to those attendant upon chlorosis and post-hæmorrhagic polyæmia. He next offers several interesting *General Observations upon the Cachexiæ.* The term cachexia, or bad habit of body, was much employed by the older medical writers, during the reign of the humoral pathology, to indicate the condition of the system denoted by, among other symptoms, paleness of face, exhaustion of strength, and relaxation of tissue; but, even before the school of Pinel in its ultra-solidism had entirely rejected it, Cullen and Sauvages had completely perverted its original meaning, by applying it to a variety of dissimilar diseases in their classifications. Bordeu employs it almost as an equivalent to diathesis.

"At present the term is employed pretty frequently. This recurrence to an old name has taken place since the study of the pathological condition of fluids in disease has been resumed. Nevertheless its signification is not clear and precise, for by cachexia is often understood either a diathesis or some grave change in the organism consecutive to morbid causes which have long been inherent in the constitution of the individual. It is in this double sense we must interpret the scrofulous, syphilitic, tubercular, &c. cachexiæ so often mentioned in medical language. I wish to show that, if we desire to treat of the same affections as were heretofore indicated by this term, we must comprehend under it the same disease which is now called *anæmia* (Andral), *hydræmia* (Bouillaud), or *hydröemia* (Piorry)."

Several quotations are furnished from the writings of Cœlius Aurelianus, Paulus Ægineta, and other of the older authors, corroborative of the above views of the causes and symptoms of cachexia. From the period at which Lieutaud wrote, (1769,) however, we do not find the affection mentioned in treatises of medicine; but in our own times we may add to the above names in the history of cachexiæ those of Andral, Bouillaud,

Gavarret, and Piorry, who, under the names chlorosis, hydræmia, anæmia, &c., have described the same essential condition.

"To fix our attention upon the value and employment of these different terms, we think the word *anæmia* should be entirely restricted to designate the ephemeral state in which the quantity of the mass of the blood is considerably less than in the normal condition, as it is immediately after a great loss of blood. The term *hydræmia* should signify that alteration of the blood which consists in the diminution of globules and the acquisition of serum, with the preservation of the normal amount of fibrine. Lastly, under the term *cachexia* we may comprehend the disease itself, in which hydræmia shews itself with its characteristic symptoms. *Cachexia* is thus applied to the union of the alteration of the blood and the symptoms, while *hydræmia* designates the change of the blood alone. Every cachexia must necessarily arise from a hydræmia, but every hydræmia does not constitute a cachexia. We may define a cachexia to be an apyretic and usually chronic disease, characterised by the existence of a hydræmia which induces diminution of strength, and colourlessness and atony of the tissues."

Among the various species of cachexia thus defined we may enumerate the *syphilitic* (Ricord and Sennertus), the *paludian* (Hoffman and Bretonneau), that of *prisoners* (Sennertus), and that arising from *moral causes*. Certain *localities* will give rise to it, and thus, Sir. J. Clark describes a form of cachexia resulting from prolonged residence in London, and the same may be done in respect to Paris. A *verminous* cachexy is often observed in children and sometimes in adults. It may be induced by *bad food*, and become epidemic in time of famine. The workers of *lead* are liable to the saturnine form; and *cooks* are another class of persons largely subject to it. Certain *articles of diet* will produce it, and numbers die in a chlorotic state at Lille, and other parts of the north of France, from drinking corn or potatoe brandy. Smoking *opium* and the too long use of *mercury* lead to the same results. Cachexia may depend upon an *anterior pathological condition*, as cancer, arising not from this impregnating the system, but from the hydræmia it generates when it gives risk to hæmorrhage or enfeebles the digestive organs. Other cachexiæ result from excessive fatigue seminal emissions, undue lactation, and other exhausting causes. The term *chlorosis* is applied too indiscriminately, whether diseases of the genital or other functions be the original cause. M. B. suggests its abandonment and its replacement by that of *virginal cachexia*, adding the name of its presumed cause. Thus, it might be induced by dysmenorrhœa, asthænia of the generative organs, imperfect assimilation, moral causes, &c. In this way, too, we might dispose of the question of the existence of chlorosis in men.

Having attempted to render the application of the term more precise, the author next offers various general observations. We have already seen that all persistent cachexiæ become eventually dependent upon a disordered state of the digestive organs as their material centre—such disorder being either latent or evident, and arising from direct or indirect causes.

"It will be easily understood that the degree of atony necessary for the production of cachexia will not show itself in all individuals in the same degree from a like loss of blood globules. Individual susceptibilities vary much in this respect. Thus, man, with his strong-toned tissues and powers of nervous resistance, becomes cachectic in a smaller degree than woman. We see, too, why arterial murmurs do not always correspond in intensity with the diminution in

the amount of globules. It is only when the globules are reduced below 80 per 1000 that murmurs are invariably heard, because then the blood is so impoverished and unstimulating that, let the individual possess what tonic reactive power he may, the tonicity of the heart and arteries will still yield: although above this figure they may do so in very various degrees in different individuals. So, too, M. Bouillaud has found that the *bruit de diable* may be always heard in individuals, the density of whose blood is diminished to below 6° of Beaume's areometer. * * * * * * * * * * * *

"It is commonly insisted upon that a great resemblance exists between cachectic affections and diseases of the heart, and we are recommended not to confound them together. This is not surprising, seeing that there is really a material lesion of this organ in cachexia; but it does not furnish the same therapeutical indications, for we know that it is consecutive to the changes in the blood, and that it disappears when the normal composition of this fluid is restored.

"*Iron* is an agent almost exclusively employed in the treatment of cachexia, and its use has been in part recommended on the ground of the deficiency of its normal proportion in the blood. It is believed that the administration of ferruginous preparations may restore the normal quantity of the metal to this fluid, and on this ground the most soluble preparations are recommended, and, according to M. Mialhe, those chiefly should be chosen which are susceptible of decomposition by the alkalis of the blood."

M. Beau doubts the correctness of this view of the direct agency of iron, although the benefit of the substance is incontestible. The iron of the blood is contained only in its globules, and there will be more or less of it as these globules prevail. To cure hydræmia, therefore, and augment the proportion of globules, it can never suffice to introduce one element of the globule only. The diminished proportion of globules is maintained by the defective condition of the digestive functions, and iron acts only by restoring these to their integrity. In fact we daily witness cases of chlorosis treated by iron in its most soluble forms without any success whatever—the digestive organs in such cases being in a condition not admitting of their benefiting by its agency. The iron is abundantly absorbed into the blood, and yet the globule is not constituted. On the other hand, when the metal acts beneficially, the earliest effect it produces is upon the digestive organs. We see other patients again recover under the use of aloes, change of air, or the removal of moral causes, &c., without ever taking iron at all, or after abandoning it as useless—the dyspepsia having yielded to other means after the iron had failed in relieving it. Our primary indication then must be to attack *the cause which produces the dyspepsia*. M. Trousseau has already protested against this indiscriminate treatment of the cachexiæ by iron; and, in fact, we can only re-constitute the blood by re-establishing the digestive functions, and removing causes which operate injuriously upon them. It is not by the direct agency of this medicine, but by the greater amount of aliment it enables the digestive organs to master, that iron is useful; and cachexia induced by mere hæmorrhage may be at once removed by the rapid administration of aliment alone.

The observations already made will sufficiently show that the fulness of pulse and excess of blood in these cases are not to be met with bleeding, although this produces a temporary alleviation. But in exceptive cases, of paralysis from congestion of or hæmorrhage into the nervous centres, it

may be required, also when dyspnœa and palpitation become so urgent as to menace life. In these rare cases such congestion must be diminished either by the abstraction of blood or by the employment of Junod's cupping instrument. "This precious instrument, which acts more powerfully than the abstraction of blood, without causing that loss of the fluids always to be so much regretted in cachexia."

5. *The Menstrual Epoch—Amenorrhœa—Pregnancy.*

"The menstrual flux does not appear to be derived from the general mass of the blood, which in normal menstruation would then be proportionably diminished, but from an excess of blood which is produced during the interval of menstruation. This is proved, (1,) by the weight and painful sensation in the loins, groins, and hips, which the woman experiences just prior to the epoch. (2). This uneasiness, resulting from an uterine congestion, begins to dissipate at the same time that the flux appears. (3). It does not exist at the end of this epoch, when the woman feels better than ever. (4). When the menses do not appear it increases more and more, and generally persists until they are established.

"If the phenomena of menstruation are confined to the symptoms of uterine congestion just mentioned, there will be found no arterial murmurs, and this is commonly the case. But in other cases to these are added, other phenomena, which are manifested in the different systems, as cephalalgia, vertigo, dyspnœa, palpitations, swelling and heat of face, &c., the pulse is full, the pulsatory movements of the arteries very visible, the veins distended—most of which symptoms are aggravated by exercise. In these cases the murmurs are heard in the carotids, appearing or disappearing with the preceding symptoms. They exist three or four days prior to the sanguine flux, and are dissipated before the termination of the menstrual epoch."

The existence of the connexion between polyæmia and the anormal arterial sound during menstruation is also shewn by the fact of there being exactly proportionate to the fulness of the pulse, becoming less when it is small, or even imperceptible, except during the production of some temporary physical or moral excitement.

Amenorrhœa.—In those instances in which there are general symptoms there are arterial murmurs dependent upon the presence of an augmented quantity of blood, but without the serum being in disproportion, as in chlorosis.

"Other forms of amenorrhœa are observed, in which, notwithstanding that plethora is evident, the arterial murmurs are yet not heard. In fact, for the production of murmurs, polyæmia is necessary, but every polyæmia will not induce them. In order that anormal sounds may be produced by the excess of blood, there must be hypertrophic dilatation of the heart, when the blood sent forth by it cannot traverse the vessels without exciting an exaggerated friction against their walls. But if the excess of blood only exist in the capillaries of the lungs, brain, or other organ, there is no reason why the murmurs should be induced in the large arteries; and in this case we do not see the arterial pulsations which are observed to be so violent after exercise in persons affected by arterial murmurs. They may then be amenorrhœa with a dilatation of the heart, and an excessive wave of blood characterised by the arterial murmurs. In other cases, again, the plethora, is only manifested by local congestions of one or several organs, and as the wave sent forth by the heart is not more volu-

minous than normal, it traverses the arteries without exciting the vibrations and accompanying murmurs."

Pregnancy.—The polyæmia of pregnancy is a serous one, for Andral and Gavarret have shewn that the blood during this condition contains more water and less globules than are normal—the blood, however, being far less serous than in cachexia, properly so called. This excess of blood explains the occasional occurrence of cerebral hæmorrhage prior to, or during, labour. They may also occur subsequently to it, if the uterine hæmorrhage which follows child-birth generates a puerperal cachexia. M. Larcher observed, in 1828, that in pregnancy there was a notable degree of hypertrophy of the heart. This condition need not surprise us, seeing that the existence of anormal arterial murmurs presupposes dilatation of the heart with more or less hypertrophy. Still, at the author's request, M. Ducrest made additional investigations upon the subject in 1843, and furnished him with a statistical table of the results.

"This table is composed of the particulars of 100 women, for the most part between the ages of 20 and 30, who died during child-birth. In all of them the measurements of the heart were taken at the thickest part of the left ventricle. The maximum was 0018 millimeters in five cases, rising in one to 0022m. The lowest figure was 0011 m. in eight cases: in most it was 0016 m; the mean of the whole admeasurements being 0015 m. If we compare these with that assigned by Bizot (0010 m.)as the normal mean of this portion of the heart in the female, we have an excess of 0005m.

M. Beau believes M. Bouillaud's explanation of the cause of the murmur in pregnancy, viz. *the compression of the iliacs by the distended uterus*, to be the correct one.

6. *Fevers.*

Certain febrile affections present arterial murmurs, which are explicable upon the same pathological principles already stated. The murmurs in *Typhoid Fever* have been attributed to its accidental complication with chlorosis, or to the loss of blood during the treatment of the disease. The author, on the contrary, believes that these arterial murmurs are an essential feature of the disease, and he examined this point carefully in all the cases he met with last year. Having separated all the women who might have been chlorotic, and such of the men as had undergone bleeding, he found of the 25 recent cases which remained, the murmurs were heard in all but four. In these patients there was also a fulness of the pulse and enlargement of the heart—the state of this organ being compared in some cases during the fever and after its cessation. In those forms of the disease in which copious evacuations were induced, the pulse, volume of the heart, and arterial murmur, simultaneously diminished. The symptoms and the cadaveric lesions alike testify to the existence of a state of polyæmia. The heart and large arteries are found dilated, and various organs much injected. The best observers differ as to the change in its composition which the blood has undergone in this disease. *Typhus Fever* and *Yellow Fever* offer like evidence of the same condition of full pulse and voluminous heart, whence the author infers they must furnish the murmur. Since he has turned his attention to this subject, he has met with seven cases of

variola in men, in all of whom the murmurs were heard. They were intense during the preliminary and the suppurative fever, but much less so during the interval occupied by the coming out of the eruption. The polyæmia which exists in variola does not exist in *scarlatina simplex* or *rubeola*, in neither of which are the murmurs heard. In *ague* the murmurs are to be heard, especially during the hot stage.

7. *Scorbutus and Purpura.*

Four cases of *Scorbutus* observed by the author plainly furnished the murmurs, and these explain the existence of certain symptoms, such as dyspnœa, palpitation, cephalgia, vertigo (all increased upon the slightest exertion) which, together with hæmorrhages, have been considered as characteristic of this disease. In these cases, too, there were fulness of pulse and arterial vibrations, so that a state of polyæmia existed. Andral has shown that the blood is deficient in fibrine, which accounts for its insufficiently exciting the various tissues, and the consequent atony and dilatation of the heart. *Purpura*, with the exception of the affection of the gums, is analogous in all respects to scorbutus: and when it is generalized we observe the full pulse, arterial murmurs, vertigo and the like. Although hæmorrhage in typhus, scorbutus, &c., may in some degree depend upon the defibrination of the blood, they also depend upon its superabundance.

8. *Dropsies.*

It is intended here only to allude to those forms of Dropsy termed essential, and which depend upon an altered condition of the blood. 1. *Dropsies with Albuminuria.* In these the arterial murmurs are habitual. Of 12 cases, occurring in men who had not been long under treatment, the author observed them in 10, the other signs of polyæmia being also the same as those seen in typhoid fever and cachexia. Nearly all observers have indeed noted in these cases a dilatation of the heart with or without increase of its substance. In 100 autopsies Bright only found the organ in its normal state in 33. The polyæmia existing in this disease has been directly adverted to by Martin Solon alone, who admits a serous plethora in cases wherein the pulse presents fulness. 2. *Dropsies without Albuminuria.* These are much more common than the others. The author has met with four, however, in which the murmurs and other symptoms of polyæmia existed. The symptoms present often lead to this dropsy being considered as arising from disease of the heart, while in fact this organ is not more frequently diseased in these cases than in Bright's disease. Dropsies are, however, here alluded to only which are not complicated by venous obstruction or asthma, and in which the heart is not diseased at its orifices—a simple hypertrophic dilatation of its walls only existing. The author believes that the derivation of albumen to the kidney or its insufficient secretion may give rise to these two forms of dropsical effusion; but he also attributes much to the mere mechanical extravasation which results from the state of polyæmia. At the commencement of the disease a free bleeding suffices to dispel the œdema, which returns as soon as the state of polyæmia is reproduced.

9. *Hypochondriasis.*

Leannec first observed the arterial murmurs in this disease, and it is singular that it should be the only affection in which he proved its existence, although there are so many others in which it is of far more common occurrence. M. Beau cites at considerable length the description given by Highmore, and has been enabled to confirm its accuracy both in private and hospital practice, it being met with in the latter more frequently than is believed, but some one of its predominant symptoms is confounded with the names of other diseases, as gastralgia, flatulent dyspepsia, anæmia, hypertrophy of the heart, chronic gastritis, cerebral congestion, &c. An increased fulness of pulse, enlarged volume of the heart, and arterial murmurs have been observed in all the cases furnishing symptoms as detailed by Highmore. The blood, too, is *serous*, having lost its globules during the mal-assimilation by the digestive organs. The *hydræmia* is, however, much less considerable than in the true cachexiæ, and should perhaps be rather regarded as the first stage of this, the blood possessing an analogous composition to that of a pregnant female. The blood possessing a less tonic property than normal, the same series of changes in the heart, so repeatedly referred to, take place; the nervous debility of these persons assisting this impoverished blood in inducing atony of the organ. M. Beau agrees to the pathogeny of Highmore, who places the original seat of the disease in the enfeebled digestive powers, which cease to elaborate properly; a serous blood resulting. Highmore also shows that there is an excess of such blood in the system, and attributes to the distension of the vessels, which he supposes this to cause, all the subsequent symptoms of the disease.

Hypochondriasis much resembles *Cachexia* in its nature, chiefly differing from it in the lesser amount of impoverishment of the blood. Hypochondriasis has been said to be especially met with in man, but in women the same form of symptoms is often confounded under the terms amenorrhœa, dysmenorrhœa, and especially chlorosis. Indeed, the greatest difficulty sometimes exists in distinguishing between the two affections, and far more than exists in distinguishing between hypochondriasis and hysteria, which has occupied so much attention.

Among the cortege of symptoms enumerated by Highmore and others, the fears, dreads, and delusions of hypochondriacs respecting their health and the gravity of the diseases they may be the subjects of, always figure; and, by the more modern writers upon this disease, these have been especially seized upon as its essential and almost sole characteristic.

"Dating from this epoch (the end of the 18th century) hypochondriasis has become, so to say, entirely absorbed and effaced by one of its habitual symptoms—the peculiar perversion of the understanding, which consists in exaggerating the gravity of a disease, or in considering an imaginary one as real. The authors of our own time have followed in the traces of preceding nosographists, and especially of Cullen, who has most insisted upon the importance of the disorders of the mind observed in hypochondriasis. It is thus we find Frank, Georget, Falret, Brachet, Dubois, Michea, the authors of the Compendium, place the origin or point of departure of the disease in the brain or nerves. When we peruse these authors, we find the symptom *nosomania*, under the ancient name of hypochondriasis, governing the entire description of the disease; and the different hypochondriacal symptoms, described long ago, as sensation of weight at the heart, intestinal gas, dyspnœa, arterial pulsation, lypothymia, anorexia, &c., are only considered as the

sympathetic and more or less immediate effects of the cerebral affection which induces this special mental perversion.

"More than this, the symptoms referrible to the abdomen and thorax, such as eructations, palpitations, &c., which have always been considered to constitute hypochondriasis, and which are even included in the definitions of the nosographists who place the horror or exaggerated fears of disease in such high relief, seem to be considered as no longer necessary to constitute the hypochondriasis of the moderns. It is sufficient for a person to believe himself the subject of a serious disease which he has not, or to be excessively frightened at some slight one that exists, for him to be considered as a hypochondriac. * * * * * * That such an affection exists among the diseases which afflict mankind, cannot be doubted; but it (which we may term *Nosomania*) should be carefully distinguished from the Hypochondriasis of Highmore, Willis, and Hoffmann—although it is yet usually found allied with this latter. In a word, it seems to us we may best understand the relations of *hypochondriasis* and *nosomania* in the way we do those of *canine madness* and *hydrophobia*. It is true that the madness has hydrophobia for a symptom; but we also know that hydrophobia may exist independently of madness, and that even the latter is not necessarily accompanied by it. The individual affected with hypochondriasis is so likewise with nosomania, which is no-wise surprising; for as the symptoms of hypochondriasis indicate affections of almost every system, and to all appearances very grave ones, and as the diseased condition may become very prolonged, the hypochondriac may easily become the victim of exaggerated terrors. But at other times, the nosomania, however well characterised, is no longer connected with hypochondriasis. It may exist as an idiopathic affection, or as a symptom of some very different disease. At other times, all the symptoms of hypochondriasis, as flatulent dyspepsia, arterial murmurs, vertigo, &c., may exist unaccompanied by nosomania. The older physicians made the same distinctions between melancholia and hypochondriasis, which I make between the latter and nosomania. They had a hypochondriacal melancholia and melancholia properly so called."

Nosomania is in fact a species of Vesania, which may exist in a very slight degree, simply inducing the fear of disease and excessive care of health (*nosophobia*), or become a confirmed mania. In *primary nosomania* the general health may continue good, and no arterial murmurs will be heard; but if this state is prolonged, it may, as any other moral cause, re-act upon the digestive organs, derange their functions, and give rise to confirmed hypochondriasis.

Symptomatic nosomania is more common than idiopathic, but it is not exclusively, though usually, observed in hypochondriasis; for it may also exist in scorbutus, cachexia, &c. It may be met with alone or conjoined with melancholia. Its definition may be thus expressed. "*A peculiar form of mental alienation, in consequence of which the individual who is the subject of it is continually and minutely occupied about his health, either believing himself the subject of a disease the presence of which is indicated by no real symptom, or interpreting very slight symptoms into indications of an incurable disease.*" Hypochondriasis may also be thus defined. "*A flatulent dyspepsia necessarily accompanied with arterial murmurs, and frequently with nosomania.*"

III. GENERAL CONCLUSIONS.

It results, from all that has gone before, that arterial murmurs are pro-

duced by reason of a disproportion existing between the calibre of the vessels and the amount of blood to be propelled through them. The excess of blood arises from a dilatation of the cardiac cavities, induced mechanically, as in the case of insufficiency of the aortic valves, or by atony of the walls of the organ in all the other affections in which arterial murmurs exist. The dilatation is much less in these than in the case of insufficiency, and the existence of præcordial dulness on percussion is therefore only ascertained by comparing its extent during the continuance and after the cessation of the murmurs. (For this comparison, M. Piorry recommends the circumscription of the space over which the dulness extends by means of a black line drawn with ink or nitr. arg.) In this way we may perceive the coincidence of the existence and intensity of arterial murmurs with the increase and volume of the heart, and *vice versâ*. In insufficiency the left ventricle is alone dilated, in diseases dependent upon atony of the four cavities are so. In insufficiency, prolonged cachexy, pregnancy and dropsy, the dilatation is accompanied with *marked and evident* hypertrophy; but in affections of a shorter duration, as typhoid fever, variola, &c. it is much less marked, the cavities however still retaining their normal thickness notwithstanding their dilatation. It may be termed *latent* or *diffused*.

"*Hypertrophy*, whether *evident* or *latent*, depending upon atony of the heart, is generally easily cured when the cause of the atony is removed. We can understand this by considering how rapidly the gravid uterus becomes reduced in size after the expulsion of its contents. So in *atonic cardiac affections* the excess of muscular substance disappears, the dilated cavities become narrowed, and at the same time the wave of blood to be propelled is much less considerable. We must not then confound, in regard to its gravity and curability, this form of hypertrophy with that arising from contraction or insufficiency of the cardiac orifices. This last is usually incurable, because the valvular affection which has induced it is beyond the resources of art. In fact, the disease in this case does not consist in the dilatation and hypertrophy of the organ, however considerable they may be, but entirely in the lesion of the orifices. The despairing epigraph affixed to Corvisart's work (*Hæret lateri lethalis arundo*) is thus solely applicable to the affections of the heart connected with disease of its orifices, and does not concern that numerous class derivable from atony of the organ.

"It is necessary to be very chary of bleeding, even when we have to do with a disease of the heart dependent upon lesions of the orifices. Dr. Corrigan has already laid down this precept in respect to cases of insufficiency of the aortic valves, and it should extend to those of narrowing. In both cases, the diminution of the globules by depletion brings on a state of atony, which aggravates the dilatation and hypertrophy dependent upon lesion of the orifices. The same result is observed when the diminution of globules arises spontaneously. The symptoms of disease of the heart, such as dyspnœa, palpitation, &c. are increased in proportion as the organism presents the characters of the cachectic condition. But when this cachexy complicates contraction of the orifices, it is no longer characterised by carotidean murmurs and fulness of pulse; for the wave of blood is now broken in its passage through the narrow orifice, and does not penetrate the arteries with force or volume enough to produce these effects. When, however, the dilatation is excessive and the wave very considerable, it may exert the exaggerated degree of friction against the arterial parietes, notwithstanding this obstruction. At the autopsy of a person who has died of a lesion of the orifice complicated with dropsy, cachexia, &c., we must regard the degree of dilatation and hypertrophy there observed as resulting from a double influence, the lesion

of orifice, and the change in the composition of the blood; the first having acted mechanically, the latter by atony."

After noticing the increased volume of the pulse and pulsations of the arteries which coincide with the arterial murmurs, M. Beau offers this cautionary remark.

"The radial pulse is augmented in the same proportion as that of the great arteries. But to appreciate this properly, we must not be content with judging absolutely of the volume of the pulse, which in different individuals varies much according to the calibre of the vessel. Its volume should be compared during the existence of the carotidean murmurs, and after they have ceased. We then find that, however small the radial pulse may have appeared during the continuance of the disease furnishing the murmurs, it becomes still less after their disappearance upon its cure. I may here remark how little care is taken to estimate the volume of the radial pulse. Whenever it is below a certain type with which it is instinctively compared, it is at once concluded that the patient's pulse is small. But this person, whom you see for the first time, may have the radial of a small and even very small calibre; and then the wave dilating it may be as strong and even much stronger than in the normal state, without the pulse losing its conventional character of littleness. The radial artery, like all the others, varies considerably in volume, not only according to the sex, but in persons of the same sex. It is also very often of unequal volume in the two sides of the body. These are facts, the reality of which our anatomical studies prove to us. The carotidean murmurs relieve us from all difficulty in deciding whether the pulse is of normal volume. If the murmurs exist, the volume of the pulse possesses an abnormal plenitude and *vice versâ*."

The radial pulse, when carotidean murmurs are present, is (1) always of an augmented fulness. 2. It may be (according as the murmurs are intense and the volume of blood propelled considerable) soft, hard, vibrating, or double. 3. The carotidean pulse may be hard, vibrating, or double, but it is never soft. 4. When the pulse is hard, vibrating or double at the radial artery, it presents the same characters still more markedly at the carotid. The double pulse here mentioned (*dicrotus* of Galen) is not felt in the radial artery in women, although it is in the carotid. The author attributes it to the elastic re-action of the arch of the aorta in its most forcible degree. The same atony, which produces the dilatation of the cardiac cavities, operates also upon the arterial parietes; so that in the interval of their pulsations, however hard and vibratory these may be, the vessels offer little or no resistance to the finger. As the malady becomes cured, however, and the volume of the pulse diminished, the arteries present more tonic resistance to the finger in the interval—the heart propelling less blood into them, and they contracting in a more continuous manner upon that which they contain.

It is owing to this atony of the arteries also that we find them, in the autopsies of individuals dying from diseases in which arterial murmurs had manifested themselves, containing a notable quantity of blood; such congestion arising from their not having possessed at the time of death sufficient tonic power for the expulsion of the blood they contained. Atony likewise in these cases affects various other organs of the economy, as the skin, muscles, &c., but the *iris* is the organ in which it is especially manifested. *The pupil is dilated in all the diseases* (except valvular insufficiency) *in which arterial murmurs exist:* but, as the diameter of this

aperture varies much in different persons, to judge of its dilatation we must also examine its condition after the murmurs have disappeared.

But it may be said that, if the excess in the quantity of blood suffices to induce arterial murmurs, these should be found present in *plethora*. Plethora, however, may be *general*, and dependent upon a superabundance of normal blood propelled by a dilated heart, and in such case we shall have the murmurs and other symptoms so often adverted to; but, again, it may be only *local* or *capillary*, congesting the face or various viscera, but not influencing the condition of the general circulation, and therefore not giving rise to any of these phenomena.

Having noticed the diseases which do furnish *arterial murmurs*, the author next briefly glances, by way of contrast, at such as *do not;* and among the foremost of these may be placed the *Phlegmasiæ*. He however confines his observations to *Pneumonia* as the type of other inflammatory affections. He remarks that this is erroneously said to occur only in the plethoric, because the persons whom it attacks may be robust. Moreover, the *pulse* in this disease has been erroneously said to be full or vibratory, which conditions do not exist until the patient has undergone several bleedings and a state of serous polyæmia is consequently induced. The only symptom common to pneumonia, and the diseases characterised by arterial murmurs, is the anxious respiration; but this depends in the one case upon inflammatory hyperæmia of the lung, and in the other upon a non-inflammatory hyperæmia affecting that organ under the influence of the general excess of the blood. In the first or even the second stage of *Phthisis*, we may have the carotidean murmurs, because it is then frequently complicated with a cachexia or hypochondriasis; but when the final stage of the malady arrives, the anormal sounds, with the full pulse and other concomitant symptoms, disappear; as might have indeed been expected from the fact that, at the autopsies of this disease, the heart is found small and contracted.

Therapeutical Indications.—Our endeavours should be directed to the remedying that morbid condition of the blood which has induced the atony of tissues, under the influence of which the other symptoms have become developed. When this condition cannot be ascertained with precision, or we have not the means of modifying it, we must content ourselves with watching the progress of the disease and combating its predominant symptoms or any attendant complications. Nevertheless, polyæmia does not demand *blood-letting*, unless a dangerous congestion occur in any of the important viscera; and this may often be preferably treated by Junod's cupping instrument. The only exceptions to this rule are formed by *pregnancy* and simple *amenorrhœa*, in which bleeding produces advantages which are not attended by future ill effects, that is, if it be not too often repeated. It will be seen then that we *proscribe bleeding just in those affections in which the volume of the pulse is most considerable;* and although this may seem at first sight contradictory, yet it will be found that the best practitioners have always forbidden bleeding, except under special circumstances, in the diseases we have enumerated as characterised by arterial murmurs. Depletion in such cases undoubtedly relieves for the moment, but only to afterwards plunge the patient into a state of ady-

namia and exhaustion. The development of the pulse and other symptoms in these diseases are due to atony; and, if we bleed, we shall add to the atony causing the disease in question, that form of it which results from the diminution of blood-globules attendant upon loss of blood. The proposition may seem to have a less paradoxical appearance and yet be no less true, if we say that we *must be very careful in detracting blood in those diseases, in which the radial artery does not offer resistance to the finger during the intervals of its pulsations.* Such a condition is always connected with unusual fulness of pulse and arterial murmurs. In *phlegmasiæ*, on the contrary, the pulse is not full at the onset of the disease, and may be even small during its progress, until free bleeding develops and amplifies it. After its anormal elevation under the influence of depletion, it insensibly diminishes in volume with the approach of convalescence, and becomes as small (or rather normal) after the cure as before the attack—the artery recovering, too, its power of resistance to the finger during the intervals of pulsation, which it had lost under the influence of the bleeding.

"The common and precise characters which we have attributed to the diseases in which arterial murmurs are habitually heard, and especially the causes which give rise to these characters, must induce us to look upon all these affections as constituting a separate class in pathology. We then bring to mind certain systems of medicine, which have already more or less signalized such a class under different names. Is it not evident that the *laxum* of Themison, the *alkalescency* of Sylvius, and the *asthenia* of Brown, are terms especially applicable to the diseases which are characterised by arterial murmurs? For these murmurs and their accompanying symptoms depend necessarily upon an *atonic laxity of tissue*, which laxity may often itself result from an *alkaline fluidity* of the blood, and to these are necessarily conjoined an *asthenia*, which, as a general rule, is opposed to the evacuation of blood. And on the other hand, the *strictum* of Themison the *acidity* of Sylvius, and the *asthenic* condition of Brown, embrace the diseases which are characterised by an absence of arterial murmurs, especially the phlegmasiæ. These three authors, treating the first on the state of the solids, the second on the composition of the blood, and the third on the consideration of the powers of the economy, had then vaguely prefigured the two general classes of diseases which are physically distinguished by the presence or absence of carotidean murmurs."

Our desire to comprise the account of the whole series of these interesting papers in one article has obliged us to render this entirely analytical, and we must defer to a future opportunity any comments we may have to offer upon the doctrines set forth. In the mean time, however, we believe our readers will agree with us in regarding the Essay a valuable one, and not destitute of important practical bearings.

I. CLINICAL ILLUSTRATIONS OF THE DISEASES OF INDIA, AS EXHIBITED IN THE MEDICAL HISTORY OF A BODY OF EUROPEAN SOLDIERS IN A SERIES OF YEARS FROM THEIR ARRIVAL IN THAT COUNTRY. By *William Geddes*, M.D. &c. 8vo. pp. 492. Smith, Elder, and Co. London, 1846.

II. REMARKS ON THE DYSENTERY AND HEPATITIS OF INDIA. By *E. A. Parkes*, M.B., late Assistant Surgeon of H. M. 84th Regiment. 8vo. pp. 271. Longman and Co. London, 1846.

DURING the last eighteen months we have had the opportunity, on more occasions than one, of directing the attention of our readers to the subject of tropical diseases, and of describing and illustrating some of them at considerable length, in our reviews of the recent works of Webb, Macgregor, and Wilson. When we think of the numbers of young medical men, whom their fate or good fortune is continually sending to our East Indian and West Indian possessions, and at the same time reflect upon the (too often) conflicting opinions that have hitherto prevailed, and still prevail, as to the management of some of the most common and destructive diseases of these climates, not another word need surely be said in the way of argument for the extended analysis which we propose to make of the two new volumes, whose titles stand at the head of this article. Both are the productions of able and experienced men, who faithfully describe what they have seen, and religiously avoid all unnecessary speculations.

Dr. Geddes served as Surgeon of the Madras European Regiment for four years, from the month of May, 1829 to the same date in 1833. Almost all the facts, observations, and conclusions contained in the volume now before us, have been derived from this quadrennial period of service. The regiment consisted of nearly 700 men; more than one-half of them were of Irish extraction; the rest were chiefly English, with not more than 42 Scotchmen. The age of the recruits, when they joined the regiment, was usually from 20 to 23 years. Little more than one-fifth of the entire number had been, the year before Dr. G. was appointed surgeon, above three years in India; so that the great bulk of the regiment consisted of young lads. It was at first stationed at Masulipatam, on the sea-coast of Bengal, and remained there until the following December, when it marched for the inland station of Kamptee, near Nagpore, where it arrived in the beginning of March, 1830. It continued there up to the close of our author's service. Dr. Geddes gives some interesting details respecting the geographical position, and so forth, of the two stations, as well as respecting the meteorological characters of the successive seasons, while he was with the troops; these will be read with chief interest by the Indian practitioner. We must not, however, pass over what he says about the food of the soldiers; as this point of hygiene seems to us to have a most important bearing on the amount and severity of the prevailing diseases, and the results of their therapeutic treatment. Let us therefore carefully note the particulars with which Dr. G. has furnished us in the following passage:—

"The food of these European troops may be described as being substantial and abundant. It differs from that made use of by the same class in their native country, chiefly in the greater consumption of animal food, which generally forms a part of every meal throughout the day. In the preparation, also, of their victuals, there is a much greater employment of the capsicum and of various aromatics,—particularly in the curry, which is an invariable accompaniment to every dinner,—than in the colder climate; and the frequent use of the preparation just mentioned, with other reasons, leads to the article of rice becoming a prominent portion of almost every meal. Acidulous liquids, as sour coagulated milk, butter-milk, tamarinds, or lime-juice, and the like, are also abundantly employed.

"The subject of drink forms an important one for consideration in the life of an Indian soldier. A system has been in force in that country (the Madras presidency and the year 1833, are here particularly alluded to) based, apparently, upon an idea of the necessity of the use of ardent spirits by the European troops, whereby a certain portion is allotted daily to each recruit from the period of his arrival in India, the price of which is deducted from his pay. Many circumstances lead to the recruits becoming thus accustomed to such a stimulus; and there are few individuals, accordingly, who omit during the remainder of their service, to swallow their daily allowance of arrack: a spirituous liquor, in strength, as supplied to the troops, little inferior to brandy: and of this two measures, of forty to the gallon, form the daily appropriation to each European soldier."* P. 15.

The enormous quantity of 10,000 gallons of arrack was consumed in the space of twelve months! But, as if this were not sufficient, the men

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* Mr. Parkes says upon this point:—

"The diet of European soldiers in India, varying necessarily at different places, is as a general rule far too rich and stimulating; hot curries, carelessly made by native cooks, are used several times every week for dinner; and vegetables in many places are scarce, or of indifferent quality. Soldiers often refer the origin of their complaint at once to their diet, and to my own knowledge many men have supplied the place of the curries by rations purchased out of their own scanty funds. It often happens that an European regiment, quartered with one or two companies of English artillery, will show a much greater per centage of sickness; the habits of both corps are the same, with one exception: artillerymen, being in small bodies, are easily looked after by their officers, and they are generally more careful about their diet. Again, married men, who are not in a mess, are always more exempt from both dysentery and hepatitis than single men. If this is not attributable to their food being better cooked, the circumstance is inexplicable. It is an extraordinary thing, that out of one hundred and fifty married men in the 84th regiment, only two died during a tropical service of thirty months, while in the same period the mortality among the single men was above nine per cent. The two deaths referred to were from phthisis and from delirium tremens. Some influence may be given to the habits of married men being more regular than those of single men, but in a small station, where little debauchery goes on, the influence cannot be great.

"A supervision of the whole system of diet among European troops,—not as regards commissariat supplies, which are generally excellent, but as respects the cooking of these, and the time of meals, the encouragement of teetotal societies by every allowable means, and the formation of day and night guards, differently clothed to prevent the effects of the great daily thermometrical range of some Indian stations,—are measures which would, I am convinced, at once reduce the list of duodenal hepatitis, and would probably even diminish the number of cases of dysenteric, febrile, and primary hepatitis."—P. 228-9.

are allowed to have more by paying for it: "the canteen is kept open for the sale of liquors to the men; and it has been ascertained, that the average annual expenditure of arrack and European spirits, wine or beer, as sold in that place, amounted to 1440 gallons of arrack, 56 dozen of brandy, or gin, 18 dozen of wine, and 60 dozen of beer, during the period referred to in the above Table. The average number of individuals present with the regiment, throughout this interval, was about 500. The quantity of intoxicating liquors, thus used by the men, would be considered great, if equally divided amongst them, and taken at divided intervals; but, in point of fact, while some soldiers observe great regularity in never exceeding their ration allowance, and a few restrict themselves to a less quantity, or even abstain from the use of arrack altogether for limited periods, generally under the sanctity of an oath; others, again, lose no opportunity of becoming intoxicated, and often retain themselves, more or less, in this condition, for several days."

The smoking of tobacco, in the form of cigars, was very general; and some men, it is believed, were addicted to the use of Opium, or to that of *bang*—the highly narcotic leaf and flower of the *Cannabis Sativa*.

After detailing a variety of minute particulars respecting the amount, and so forth, of sickness during each year that he was attached to the corps, Dr. Geddes gives us the following summary of the general results:

"From the foregoing details, it appears that an average strength of 433 European soldiers, afforded an annual average of 1,332 admissions to hospitals for a space of five years; or a monthly average of 111 cases of disease. This exhibits a degree of sickness equal to 2¼ admissions to each soldier within the year; and their stay in hospital appearing, on an average of the whole, to have been 12⅓ days, the mean loss to the service from confinement there, throughout the above period, was at the annual rate of about 31 days, or, in other words, one month in twelve. Again, on an examination of the observations from which these averages are taken, it appears, with regard to the number of times each individual was in hospital during the five years alluded to, that these varied from once to 45 times. Of those men who were present at head-quarters during the whole of this period, four persons only had not been in hospital within this time; and of these, also, two had been unwell, but from their situation as staff-sergeants, they had been placed on the sick-list."—P. 75.

With respect to the mortality during the period of service alluded to, we are told that—

"The number of deaths included in the above tables of disease, as having occurred in hospital from 1st July, 1828, to 1st July, 1833, amounts to 133; being one in 57 of the sick under treatment, or somewhat less than two per cent. To these, however, ought properly to be added seven individuals, who either died a natural death before being brought to hospital, or whose diseases—contracted while at head-quarters—proved fatal, when the persons affected had been sent to a distance for the benefit of their health. The whole, being 140, forms the mortality from natural causes, in the course of five years, in a body of Europeans in India, averaging 533 individuals; this approximates to 26 per cent. in this period, or an average of five per cent. of the numerical strength in each year. Deaths from Cholera, however, being removed, the rate of mortality is reduced to 20 per cent.: or an annual average of four per cent."—P. 81.

The prevailing diseases were Fever, Hepatitis and its consequences, Abdominal and Cephalic Inflammation, Rheumatism, Dysentery, Diarrhœa, Cholera, Indigestion, and Syphilis. One-fourth of the entire number of

cases under treatment consisted of Fever; and there was a like proportion of "local complaints." Dysenteric disorders range next in point of frequency, being about one in every eleven. Then follow Rheumatism and Indigestion. To these succeed (in the order mentioned) Syphilis, Cephalic Inflammation, Diarrhœa, Hepatic Disease, Abdominal Inflammation, and lastly Cholera: the cases of this last disease did not exceed $\frac{1}{14}$th of the whole number. We may, *en passant*, remark that there is no such thing, however frequently it may be named in common parlance, as what is usually called a "seasoning fever." The disease, that the recruit may first experience after his arrival in the East Indies, depends on a variety of circumstances not immediately connected with the mere change of climate; such as the nature of the locality to which he is sent, his habits, exposure to weather, and so forth.

We commence with the consideration of

FEVERS.

This order of diseases is at once the most frequent and most important of all that are met with in our East Indian Hospitals, as well of the native as of the European troops. During the space of five years, nearly 1800 cases are reported to have occurred in the Madras European regiment. They are arranged by Dr. G. under the heads of Continued, Ephemeral, Intermittent, and Remittent Fevers. The two first form but a small proportion of the entire number; it is with the two last that we shall have chiefly to do in the following observations: we shall therefore begin with them.

Intermittent and Remittent Fevers.

With respect to the complete identity in nature of these two most frequent forms of pyrexial disease, our author remarks:

"It is observed that, on the arrival of a body of men at an unhealthy station the hospital becomes gradually filled by patients who are found to have attacks of fever daily, or every second day. In some of them, the frequency of pulse and other symptoms descend to a natural state between each paroxysm of fever; and in others, a certain degree of pyrexia remains at the accession of the following paroxysm. Patients are daily received under each of these circumstances; and in cases of relapse, the intermittent of one admission has occasionally become a remittent in the next, and *vice versâ*. The succession of symptoms and progress of each paroxysm, correspond in either variety; and at the same time it is found that the remedy which is specific in one form, possesses the same powers in checking the other. In short, apparently the produce of the same cause, showing similar symptoms, and removed by the same means, there seems no difference between these two forms of disease, but that of severity. It is true that, in certain stages of some instances of remittent, symptoms become superadded which have no corresponding appearance in cases of intermittent; but they are the result of the violence, or, along with this, the duration of the disease; and their frequent occurrence, or otherwise, indicates the degree of severity in an epidemic attack, or, occasionally, the method of treatment employed. Believing, therefore, all these cases of a fever in paroxysms to be of the same nature, whether having a remission, or an intermission between them, it is intended to

apply the term *Paroxysmal* to these diseases in general; in contradistinction to that of *Continued*, as given to Synocha, and others."—P. 90.

These fevers were comparatively rare in the regiment while it remained at Masulipatam on the sea-coast, compared with what was the case at Kamptee, commencing with the first rainy season there. They prevailed more in the months of August, September, and October (during and immediately after the rainy season) than in any other part of the year; gradually diminishing, in point of frequency, during the cold months, and again increasing in March and April with the returning heat.* The prevalence of the fevers was uniformly observed to be proportionate to the amount of rain that fell during the wet season, coupled with the degree of heat of the weather before and after the fall. Much too, doubtless, depended, in respect of morbific influence, on the range or variation of temperature experienced in the course of the twenty-four hours, during the sickly season. Indeed, as Dr. Geddes observes, "an alternation of temperature appears necessary to excite the phenomena of Paroxysmal Fever; and the course of this change is observed to be the occurrence of heat *after* that of cold, combined with moisture."

The age of the individual seems also to have a good deal to do with the degree of his liability to be affected with the febrific miasm. Those men at or beyond the age of twenty-five were observed to be decidedly less liable to fever than those a few years younger. "Of 193 persons who in 1830 were under the age of twenty-five, 100 were affected in the first year of their sojourn at Kamptee, and only 38 escaped altogether; while of 276 individuals, at and beyond the age of twenty-five, 99 were affected in their first season at this station, and 81 had no fever previous to the period of the author leaving the regiment."

The same thing was observed in respect of the tendency to a recurrence, as of that to the first attack, of the disease; for there was a larger number of relapses in those below, than in those at and above, 25 years of age. Among the soldiers, the Scotch suffered less than the English and still less than the Irish. Is this to be attributed to their greater sobriety?

With respect to the influence of longer or shorter residence in India on the liability to attacks of Fever, it is by no means easy, as will appear by the following extract, to arrive at any definite conclusions.

"It appears, that those having been the shortest period in the country—who were generally, however, the youngest soldiers—were most liable to be attacked with the disease; that, beyond a short sojourn there, an interval seemed to occur where individuals appeared less liable to be affected: but that the tendency rather increased afterwards, until the age of the soldier, perhaps, became an auxiliary in enabling the constitution to resist the cause of the disease. Thus of 444 individuals, whose residence in India was ascertained on the 1st of July, 1830, in 132, whose stay there had not extended beyond two years at that date, 108 had Fever in the three following years, instead of 95; which should have been the number attacked, according to the general average. In 54, whose residence had been from two to three years, this average was observed; those from three to four

* September was, on the whole, the most sickly, and February the least sickly, months in the year. Such was the case at Kamptee; but this statement does not hold good as respects other parts of India.

years, who amounted to 160, had two less than the general average; 37, again, whose residence extended to different periods from four to eight years, rather exceeded the average; while of the remaining 61 persons, whose stay in India had been more than eight years, 35 only had Fever, instead of 44, which should have been the number according to the proportion at which the Fever generally prevailed. Length of residence has, perhaps, little effect in the prevention of an early attack; a fully equal proportion of the older residents being affected in the first season of their sojourn at Kamptee as of those who had been a shorter period in India. Of 22 individuals, however, who suffered from eight to twelve attacks of the disease, four had been only one year in India at the period of the first accession of Fever; five had been two years; seven had been three; and three had been four years in India. The remaining three had been seven and nine years in that country. It would appear, therefore, that the greater proportion of those having many relapses had been but a short period in India; but the effect here may be divided between the influence of youth and that of the short sojourn in a warm climate."—P. 112.

Before alluding to some of the more remarkable phenomena of Intermittent and Remittent Fevers, Dr. Geddes makes the general observation that the descriptions, that have been given of these fevers by many preceding writers, would have been much truer to nature, and therefore more useful to the young practitioner, if they had been considered and described, not as distinct maladies, but merely as different degrees of one and the same disease, exhibiting the same general features, and requiring the same general principles of treatment.

With a view of establishing a distinction between the Remittent and the Intermittent forms of Fever, it has too often been the practice of authors to select only the most strongly marked cases for description, omitting the less decided, but perhaps more important, forms of the disease; and, all the while, but little information is given as to the relative frequency with which it makes its appearance in these decided forms.

Whenever the pulse fell down, in the intervals between the pyrexial paroxysms, to 72, the case was considered by Dr. Geddes as one of Intermittent fever; and when it kept above this mark, it was set down as one of Remittent.

After a variety of minute statistical details respecting the frequency of each of the usual symptoms of Fever, we come to the following notice of the Spleen, as one of the viscera that is very often affected:

"In 35 cases, some degree of pain in the site of the spleen is reported to have been felt, at certain periods of their progress. This pain has generally been increased by a long inspiration; and, sometimes, is stated to have been so by coughing, vomiting, walking quickly, or by lying on either the right or left side. It has, occasionally, extended towards the breast and left shoulder, or towards the pubes or umbilicus. The period of the case at which this symptom has been noticed, has most frequently been about the second or third day after admission; but in some instances it has preceded this event, and in others, been of later occurrence. It has generally been of speedy removal; but sometimes has continued for a short while after the tendency to Fever has become checked. In only one of these cases was there appearance of enlargement of the spleen; and, from the effect of remedies, it seems most likely that the pain reported in the site of this organ, or near to it, has, in the greatest number of instances, depended upon obstruction of the bowels near to this situation. The spleen was found enlarged in another case of Fever, after the patient had suffered twelve

paroxysms of Tertian Fever, before coming to Hospital; but in no other instance was any affection of this viscus to be observed."—P. 130.

In all cases, without exception, the abdomen should be most attentively examined, more especially in both hypochondria, and along the head and tract of the colon. It is uniformly of first-rate importance to relieve local irritation, at the same time that we seek to arrest the constitutional disturbance. In some cases, the Headache and Vertigo are the most annoying symptoms present. Under such circumstances, the effects of the quinine require to be narrowly watched; as this medicine is unquestionably liable in some constitutions to aggravate, if not to excite, the head-distress.

As to the hour of the day at which the attacks of paroxysmal fevers usually take place, we read:—

"It has been remarked that where the paroxysms of Fever are disposed to recur at certain intervals, their accessions more frequently take place at one period of the day than at another. This fact is exhibited in the Table given in the next page, where the usual hours of attack in the cases of each type of Fever are arranged. It will be there observed, that the general tendency of the Tertian interval—as shown in the one paroxysm of the single, and in the corresponding or more perfect fit of the double cases—was to have the accession of Fever at an earlier period of the day than in the Quotidians. Thus of 421 of the latter, the attacks occurred before noon in 161, and in 260, after that hour: while of 776 single and double Tertians, the Paroxysms took place in 659 in the earlier half of the day, and in only 117 in the latter portion. Another circumstance which has been remarked, is, that where there have been more than one or two admissions with Fever in the same person, the after attacks observe a considerable degree of similarity in their hours of accession as well as in their type: that, in short, where the system has become accustomed to Fever, a certain regularity of type and hour of accession is assumed: the exceptions to this rule being produced, it is believed, by accidental causes, at or previous to the commencement of the attack. It is difficult to exhibit, in a tabular form, the facts which tend to prove the truth of this observation; but it may be remarked, that they consist of a general view of the respective hours at which the several relapses of each patient took place; and the types of Fever which they presented. The result of this enquiry is, that, in cases where there were more than two attacks, there was generally a great predominance of the relapses of the same type, and occurring at the same period of the day; and that this was particularly remarkable among the Tertians, and their usual hours of accession; namely, the earlier portion of the day."—P. 137.

With very few exceptions, the febrile paroxysms occurred at some period of the *day*-time.

The Intermittents were, upon the whole, one-fifth more numerous than the Remittents; but the relative frequency of the two forms was found to vary a good deal in different years and months. The Remittents were most numerous from July to November, and especially in the month of September. It was observed that the proportion of Remittents gradually diminished, as the residence of the men was prolonged at Kamptee. As a general result, it would seem that, when Fevers are most prevalent, they are usually the most severe, and most disposed to assume a continued type; and that they diminish in violence according to the length of time of a person's exposure to the febrific cause, and also according to the number of relapses which he has experienced. Hence, Intermittents pre-

dominate during the less sickly months, while Remittents are most frequent during those that are most unhealthy; and hence also the fact that by far the greatest number of relapses exhibit a simple tertian character. There were only thirteen instances of Quartan observed out of the whole number of the cases that were treated; these occurred, with two exceptions, in the period when least fever prevailed, viz. in the seven months commencing from October. In two individuals no other type of fever was observed; but in the remaining cases, the patients had all suffered previously either from Quotidian or Tertian fever. In four cases, the disease was checked before the third paroxysm occurred; in six cases, the paroxysms reported to have taken place were three, four, or five; in the remaining three cases, the disease had recurred at each quartan period for the space of one or two months. The cases of Quartan were observed chiefly in men about 29 or 30 years of age, and who had been from five to ten years in India.

The *diagnosis* of Paroxysmal Fever is usually abundantly obvious, if the symptoms be accurately recorded at two distant periods of the day, and due attention be paid to the nature and history of any attendant local affection. Cases, however, are every now and then occurring which will somewhat perplex the less experienced practitioner.

"There are three classes of disease, for which it is liable to be mistaken; one, where a febrile state is the predominating complaint; another, where any of the common topical accompaniments of Paroxysmal Fever exist, and are attended by pyrexia; and the third, where a Fever is present, putting on the paroxysmal form, but having its origin in some organic visceral disorder. In the first class are comprehended those ephemeral atttacks arising from excitement by heat, drinking, and the like, or from disorders of the chylopoietic organs, or exposure to cold and moisture; in the second, are comprised certain cases of cephalic or splenitic Inflammation or Rheumatism, and Cholera, all of which, when Paroxysmal Fevers are numerous, may put on so much the appearance of these Fevers as to be confounded with them; and, in the third, are included cases of Hectic Fever arising from Hepatic abscess, Phthisis, or other extensive organic lesions producing constitutional irritation. In distinguishing such disorders from Remittent or Intermittent Fevers, the medical attendant will be chiefly guided by the prominence or presence of peculiar symptoms, or their more or less continued nature and duration; and it may be here remarked, as has been already adverted to, that nothing will be found to assist him more in arriving at a correct knowledge of the nature of the disease under treatment, than accurate and minute records of the symptoms of the case, combined with the information which a series of such reports will afford, respecting the previous history of the patient." P. 150.

The results of the treatment pursued in the 1210* cases which occurred in the quadrennial period of Dr. Geddes' service must indeed have been highly gratifying to him; only one proved fatal. This great success was doubtless owing partly to the promptitude with which the disease was encountered, partly to the general mildness of its character, and in part also to the skill which our author, from his long and multiplied experience

* It would seem that nearly 200 cases more had occurred during the period referred to; but, as these cases were attended by other medical officers doing duty at the time with the corps, they are not taken into consideration at present.

had acquired in treating the fevers of India. He tells us how much less successful he had been on some former occasions, when he was in medical charge of native battalions at Seringapatam in 1823, and at Cuddapah in 1826. In the latter instance, out of 955 cases, 20 proved fatal. We must not forget to mention that, at the period alluded to, Quinine had only been lately introduced from Europe, and even the supplies of Cinchona powder were often insufficient to meet the demands. Thus, in one-half of these 955 cases, no bark or quinine was used at all. There is so much varied and curious information in the following table*—one of several dozens which our author has given in the course of these "Illustrations"—that we have much pleasure in submitting it to our readers as an example of the great pains which Dr. Geddes has expended upon the composition of his elaborate work.

Of the 20 deaths, 17 occurred among the southern Hindoos, and 3 among the Bengalees; while in none of the Mahometans did the disease prove fatal. This was most probably owing in part to the difference of food and greater strength of constitution among the latter, and in part also to the circumstance that they reported themselves more readily sick, and gave a more intelligible statement of their complaints than the poor superstitious Hindoos. The men who suffered were chiefly young recruits; there being but three whose ages amounted to 24 and 25, while thirteen were only 18 and 20.

In fourteen of the fatal cases, the patients died in a state of delirium or insensibility, supervening upon violent and repeated paroxysms of the fever. In three other individuals, death seemed to be caused by the utter prostration that succeeded to the severe febrile paroxysms, but without the occurrence of any marked cerebral symptoms. In one of the remaining three cases, the patient, who had been affected with vomiting, diarrhœa and slight delirium, sunk just before the expected accession of the cold fit, apparently from a sudden failure of the vital powers. In the remaining two cases death was occasioned, in one by dysentery, and in the other by hectic fever, induced by abscess of the chest. So much for the fatal cases at Cuddapah. With respect to those that occurred at Seringapatam in a battalion of native infantry, 1119 strong, from 15th April 1823, to the 10th March 1824, Dr. Geddes is not able to give a very minute report; but the following are the leading facts. Out of 1503 cases admitted with fever into the hospital, 22 proved fatal.

"In these cases affection of the head was not so prominent as in those above stated. Three individuals only died in delirium or stupor; while another who had suffered from the former affection, eventually sunk by the occurrence of a sloughing ulcer on the loins. In two persons, death took place from the violence of the Fever—one of the cases being attended with cough and much expectoration; five patients died by the supervention of severe Dysentery while in hospital with Fever; and one by the accompaniment of a peculiar disease (*Beri-beri*) characterised by the lower limbs becoming paralytic, and this ending in dropsy, particularly affecting the cavities of the chest and pericardium. In ten cases the death was occasioned by the result of a chronic state of Fever, or of frequent relapses, in which either a tendency to œdema, or a loose state of the bowels occurred; and frequently both of these symptoms became combined together. By the increase of these disorders, accompanied occasionally by returns of Fever, the patient sunk in an extremely debilitated and emaciated condition."—P. 161.

* TABLE exhibiting the Prevalence of FEVER, its Types, Chief Means of Cure, &c., in a Native Corps in 1826–7.

Months.	Strength of corps.	Of whom were attacked.					Total.	Quotidian.		Tertian.		Double Tertian.		Ephemeral or one paroxysm.		Total.		Of which relapses.	Cured chiefly by								Average number of days in hospital.	How many had relapses till 1st May.	Average number of days till relapse.	Died.
		Hindoos.	Musulmans.	Bengalees.	Portuguese.	Indo-Britons.		Remittent.	Intermittent.	Remittent.	Intermittent.	Remittent.	Intermittent.	Remittent.	Intermittent.	Remittent.	Intermittent.		Emetics.	Antimonials.	Antimonials and opium.	Pulv. cinch.	Pulv. cinch. and opium.	Sulph. of quinine.	Pulv. cortic. mel. assafœtidæ.	Mil. asaf. and opium.	Mouth sore.				
1826, June	583	..	..	..	..	..	9																							
July	683	..	..	..	..	..	29																							
August . . .	909	..	..	..	..	..	21																							
September .	1080	..	..	..	..	..	59																							
October . .	1087	47	22	9	..	2	80	21	14	14	11	10	5	3	2	48	32	1	6	22	4	2	12	13	8	11	4	10½	27	47	2
November .	948	80	44	33	1	..	158	44	14	40	22	21	5	5	7	110	48	23	3	54	6	45	46	..	..	..	17	10	84	33½	3
December .	926	98	45	25	2	2	172	50	17	29	34	20	8	7	7	106	66	50	4	49	10	37	69	1	..	..	22	11½	81	32	2
1827, January . .	896	96	35	12	2	3	148	41	12	27	25	20	13	6	4	94	54	70	1	40	10	7	32	11	8	34	22	14	62	30	3
February . .	885	96	37	14	1	3	151	53	21	16	20	19	13	6	3	94	57	57	..	60	16	29	36	2	1	1	5	10½	48	28½	6
March . . .	875	66	33	19	..	7	125	50	16	16	10	19	10	3	1	88	37	76	1	31	5	23	40	20	..	..	1	10½	48	14½	2
April	879	78	20	20	..	3	121	39	18	19	15	14	11	3	2	75	46	87	..	54	14	11	21	..	..	8	16	8½	5	8	2
		561	236	132	6	20	955	208	112	161	137	123	65	33	26	615	340	363	15	310	65	154	256	47	17	54	87	10½	365	28	20

This Battalion consisted on the 1st May, 1827, of, Musulmans, 244; Bengalees, 153; Southern-Hindoos, Gentoos, 274, and Malabars, 182; Native Portuguese, 6; Indo-Britons, 6; Total, 865.

In the solitary fatal case, which occurred in the Madras European regiment, the patient died in a state of delirium and stupor after some severe accessions of fever; there had been diarrhœa and occasional vomiting. On dissection the liver was found large and friable; *the stomach was filled with a liquid like coffee-grounds;* and a few superficial ulcers were observed on the mucous surface of the bowels.

An occasional very troublesome consequence of protracted fever in India is the formation of a peculiar form of *Ulcer*, that is thus described by our author.

"They commenced from a trivial sore, such as that of a blister, the friction of a shoe, or the like; and assuming a round form, with raised, thickened, abrupt, or callous edges, and an irregular, unhealthy, and dirty surface, they increased in size with considerable rapidity and much pain. The pus adhered to the surface, and the discharge was a watery or bloody ichor. After extending, occasionally by ulceration, and at other times by sphacelation, the surface began to assume a more equal and clean appearance; the edges became reduced; and granulations forming from the bottom of the ulcer the sore gradually healed. These ulcers occurred at different periods from November to April; and chiefly in men whose health had been broken by frequent returns of Fever, or by long confinement to hospital, from these or other diseases, or by a severe affection of the mouth from mercury.*"—P. 156.

With respect to the subject of *treatment* of Remittent and Intermittent Fevers, all we propose to do at present is, to select a few simple memoranda from our author's narrative that may be useful in refreshing the memory of the reader.

Dr. Geddes remarks that, when he first went out to India, the usual practice in Remittent Fever consisted in the exhibition of Calomel pushed to salivation. "Bloodletting and other remedies were also employed; but without reference to the stage of the disease at which they were applied." The results of such practice were often most injurious. It is not often that this much abused medicine, Mercury, is carried to such an extent in the present day.

In the cold stage, a dose of Opium—one grain combined with eight grains of the *pulvis antimonialis*—was usually given. When vomiting, purging, or other alvine irregularity was present, a dose of Calomel—from 5 to 20 grains—was associated with the opium. In the hot stage, the great object should be to moderate the violence and shorten the duration of the febrile action. Bloodletting was not often required for this purpose; it was practised in 16 cases only. Local bleeding, by leeches—applied most frequently to the temples, in other cases to the nape of the neck, to the chest or abdomen—was extensively used, and, for the most part, during this stage of the disease. Antimonials were given in frequently-repeated doses.

"Opium, if not previously exhibited, was given, at this period of the disease, under the same circumstances as in the cold stage; and was combined with calomel, or the antimonial powder, according to the state of the stomach or

* This subject was considered at some length in our last Number, in the review of Dr. Wilson's Medical Notes upon China.

bowels at the time of its being used. This remedy was occasionally contra-indicated by severe affection of the head or a plethoric, robust habit of body of the patient, and severe Fever; but, in general—as soothing the patient's feelings, checking various symptoms, and tending to lessen, and thereby shorten, the cold as well as the hot stage of the disease, while it rendered the succeeding portion of it more complete—opium was considered a valuable adjunct in the treatment of many of these cases of Fever; and was generally given where the patient was seen in either of the two first stages of the disease."—P. 173.

The period at which the exhibition of a purgative, when necessary, was found most serviceable, is upon the accession of the sweating stage, so that the operation of the medicine might go along with the decline of the febrile symptoms.* It is to be remembered that purgatives are not required in all cases, and are, therefore, not to be given indiscriminately.† We cannot quite agree with our author in the propriety of the following suggestion; would not the remedy recommended be much more advantageous in the first or cold stage?

"When the patient was first seen at this (the sweating) stage, or decline, of the disease, and the stomach or bowels were not prominently affected, an emetic of Pulvis Ipecacuanhæ frequently took the place of a purgative at this period; particularly when the admission took place in the evening. The effect of this medicine was to remove indigestible or irritating matter from the stomach, and occasionally to act slightly on the bowels; while its operation also assisted the process of diaphoresis going on at the time. An emetic was thus administered in 495 cases."—P. 175.

As we have already mentioned, the treatment of the Paroxysmal fever of the East Indies is infinitely more easy and more successful since Quinine has been introduced into practice. Its use should, as an almost invariable rule, be commenced whenever the febrile paroxysm has fairly begun to decline, and the skin has become moist. We are not to wait until the pulse falls, nor till any inflammatory and dysenteric, or other affections

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* Dr. Sutherland, whose little work on "Mercury in Fevers, &c.," we were obliged, from want of space, to notice so briefly in our number for last April, remarks upon this subject:—

"There is, perhaps, no form of fever wherein it is of greater importance to be particular in the time chosen for the administration of calomel or any other remedy. The dose of calomel that would prove highly beneficial, along with a purgative extract, in cleansing the bowels of mal-secretions, if given when half the period of remission is elapsed, would have no purgative effect at all if given in the early stage of an exacerbation; and the same dose given half an hour previous to the expected exacerbation would most probably induce it, and render it more severe, that otherwise the patient might have escaped. Untimely purgatives are often the cause of renewed paroxysms."

† On the subject of those black tarry evacuations so frequently observed in cases of protracted Remittent Fever, Dr. Parkes says that, judging from the results of necroscopic examinations, the liver will generally be found much enlarged and congested, while the mucous surface of the bowels is but little affected. "The black stools probably came from the minute structure of the liver, and passed at once into the common duct without entering the gall-bladder. The black, coffee-grounds-like matter, which I have seen vomited in dysentery, comes evidently from the dark striated vessels occupying the bottom of sloughing ulcers in the colon."

that may happen to be present, be removed; for its use is by no means incompatible with that of depletory and other such remedies as may be required to relieve these complaints. By preventing the tendency to the recurrence of the febrile paroxysms, the quinine serves to facilitate and promote very materially the removal of any accidental morbid phenomena that are complicated with the primary disease.* The usual dose of the quinine was three grains, given every two or three hours. From a scruple to a drachm and a half was required to effect a cure.† The average stay in the hospital was about six days. Nearly three-fourths of the patients, 883, were discharged by the sixth day; 226 by the eighth; and the remaining 88 were there for a longer time. The quinine seemed to produce Vertigo and Tinnitus Aurium in some cases; in two or three instances these symptoms were followed by a certain degree of deafness. Occasionally its use in the fluid form was followed by diarrhœa and intestinal uneasiness.

Continued and Ephemeral Fevers.—We have not much to say upon this head.

"Continued Fever," Dr. Geddes observes, "in its form of a contagious disorder, as usually seen in Great Britain, has not been met with in the course of the author's experience in the East. It is true that, in certain instances of violent remittent, the symptoms, as have been above described, occasionally put on a typhoid appearance; and, in some cases afterwards to be alluded to, the disease has occasionally assumed the chief features of a Continued Fever; but in neither case was there any cause to believe in the presence of infection; while the history of the attack in the one instance, and the presence of certain symptoms in the other, showed that the febrile disorder was not originally of a continued nature, or if so, it was not a purely idiopathic disease."—P. 195.

Not a few of the cases of ephemeral or apparently Continued Fever were owing to the effects of intoxication, or of direct exposure to the sun's rays. In some instances, erysipelas or a furuncular eruption appeared upon the subsidence of the febrile symptoms. What is usually called a *stroke of the sun* (coup-de-soleil) is an attack of ephemeral or continued fever, accompanied with symptoms of cerebral congestion, which, it is well known, may terminate in stupor and death. Three fatal cases of this description occurred in our author's practice. In the more fortunate cases, a paralytic weakness of one side or of one extremity was sometimes left behind. Venæsection and purgatives were generally required. The abuse of spirituous liquors will occasion the same sort of apoplectiform attacks, which exposure to the intense rays of the sun is apt to produce.

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* Dr. Parkes remarks that "the combination of Calomel and Quinine appears decidedly useful in Remittent Fever, in reducing the bulk of the liver and spleen." He says that the combination of the quinine with the mercurial has appeared, on several occasions, to accelerate the occurrence of ptyalism; and that, when this effect had ceased, it was re-induced by the use of quinine.

† Nine pounds, four ounces and a half in all were used in the treatment of 1200 cases. It would appear, however, that the Cinchona in powder was given at the same time; for we read, in one part, that this in powder "was generally employed, from motives of economy, in the last two days of the patient's stay in hospital, when the force of the disease had been fully checked by the Quinine."

The Second Chapter is devoted to the subject of *Diseases of the Head.*

Cephalic Inflammation.—During the four years of our author's service, 38 cases, to which this appellation might fairly be applied, were admitted into hospital. It is unnecessary to describe the symptoms of the affection, nor need we enlarge upon the appropriate treatment. Topical bleeding, blisters and mercury pushed to slight salivation, are the chief remedies. If the febrile symptoms and head-affection assume anything of a paroxysmal character, Quinine must be given; but we need scarcely say that its effects require to be watched with attention, as any inflammatory action about the head is exceedingly apt to be aggravated by bark in any form.

Four cases out of the 38 proved fatal; in two, by serous effusion; in another, the serum was mixed with purulent matter in the cavity of the cranium; and in the last, there was simple inflammation or congestion of the encephalon, but without any traces of effusion.

Cephalic Inflammation sometimes terminates in Paralysis,—generally in the form of Hemiplegia—at other times in Epilepsy. Of the latter disease, 59 cases occurred in the regiment to which our author was attached during four years. These 59 cases occurred in 12 individuals. In two or three instances, the epileptic seizure was followed by a delirium, which had all the characters of that consequent upon hard drinking—a too frequent cause, by the bye, of the original malady. The remarks of our author on the use of Opium,—usually associated with Calomel or Antimonials, and after the employment of due depletory remedies—in the treatment of Epilepsy deserve notice.

"Opium was in some cases given as an anodyne, and in others as an antispasmodic. Its powers in the former capacity were chiefly required when the disease had originated in hard drinking, or was attended by symptoms resulting from that cause; as an antispasmodic it was employed in conjunction with depletory or other measures, according to the state of the patient, and duration of the disease at the time. It was thus exhibited in twenty-one cases of those having Epileptic fits; and in eleven of these no convulsions took place after a dose of Opium had been administered. In four instances, no fit occurred after a second dose had been given; in one, they ceased to recur after the fourth dose; and in another after the eighth; while in three cases, where one dose, and in another where two had been given, the convulsions eventually ceased, without reference to the exhibition of this medicine."—P. 257.

The number of cases of genuine *Delirium Tremens*—the "horrors" as it is called by the common soldiers—is distressingly frequent among our East India troops. But can we wonder at this, when we think of the disgraceful facility with which our soldiers there can obtain the very poison, which is the "*fons et origo*" of all the mischief. It is high time for the medical officers of the army to remonstrate with those in power on this most important subject of hygiene, until a salutary change is introduced. Dr. Parkes is certainly mistaken when he asserts that too much importance has been attached to the abuse of spirituous liquors, as a producing cause of gastric and duodenal disease among our East India troops. He has himself recommended the establishment of tee-total societies among the soldiers.

Of all the chapters in Dr. Geddes' work, that on *Thoracic Inflammation* is the least praiseworthy or satisfactory. The great blemish of the whole is the entire omission of so much as a passing allusion to the use of Percussion and Auscultation in the diagnosis of thoracic diseases, more especially of that very disease whose history occupies almost the whole of the present chapter—Pleuritis, acute and chronic. In the three fatal cases, it does not seem that the existence of the pleural effusion had been clearly made out during life, except in one instance; and in that, Nature herself made an effort to evacuate the fluid, by inducing a prominent elastic swelling between the sixth and seventh ribs on the left side. Upon an incision being made into it, nearly a quart of semi-purulent matter flowed out. The patient died ten days afterwards. No dissection took place; but our author concludes that, "from a survey of the symptoms attending the disease, the case seems to present more features of a chronic inflammation of the pleura, terminating in the formation of purulent matter, which made its way to the surface, than of any other disorder likely to have occurred.

There cannot be a reasonable doubt upon the subject. In the other two cases, it seems probable that, if a correct diagnosis had been formed, recourse might have been had with advantage to *paracentesis thoracis*. In one of the cases, the heart was found pushed somewhat to the right side, by the large quantity of the effusion in the left pleura.

The Chapter on *Abdominal Inflammation* is chiefly taken up with the subject of Splenitis and inflammatory affections of the bowels;—the different forms of Hepatitis and their consequences being considered in a separate chapter. Out of 81 cases, reported under the official appellation of abdominal inflammation, there were 28 of simple uncomplicated Splenitis. Most of these cases occurred in persons, whose health had suffered from previous attacks of Fever, Rheumatism, Dysentery or Hepatic Disease. Perhaps the second named is the most frequent precursory affection; and it deserves to be remarked that, in the progress of several of the splenitic cases, symptoms of a rheumatic character were more or less decidedly developed.

The only enteric affection, that we shall notice, is that wherein a subacute inflammation of the *Caput Coli* constitutes the predominant feature. The narrative of one of the cases reminds us of several instances which have occurred in our own practice; and, as we have reason to believe that the exact seat and kind of the existing disease is apt to be mistaken, it may be well to give our author's description, which is very true to nature:

"There appeared a combination of weakness and irritability of the large intestines generally, along with a peculiar tendency to inflammatory action about the head of the colon, or in the right iliac region. There were, accordingly, at irregular intervals, attacks of looseness of the bowels, with a feeling of soreness in the abdomen, or a pain from the movement of flatus, which was often of a severe tearing nature. The affection of the right side of the abdomen, however, was the most conspicuous part of the disease. At first this consisted of a slight pain or uneasiness about the head of the colon, which was shortly afterwards reported as being most severe; and situated to the right of and below the umbilicus, stretching up towards the spleen on pressure. This soon became diminished; but in two days afterwards, an acute pain, in fits, was reported to extend from the right groin towards the stomach, attended with vomiting; though

this was relieved in some measure by a full operation on the bowels. A soreness continued next day, increased on pressure, about the upper end of the inguinal canal; and under the operation of small doses of Sulphas Magnesiæ, occasional sharp pains were felt, and described as if two gushes of water were coming to the spot in different directions, and thus exciting pain when they met. Pain was likewise excited on drinking. There had also been some irregularity in the pulse, with sharpness in its beat, for several days. Chiefly by the operation of saline purges, the local affection became alleviated; but a lax state of the bowels, with occasional griping and flatulence, continued; and although the patient was quite easy in the horizontal posture, he felt on rising up, or on a long inspiration, as if the intestines adhered together at the former site of pain. By blistering the part, however, and remedies adapted to give tone to the bowels, recovery gradually took place; but the patient's stay in hospital extended to twenty-nine days."—P. 430.

On the whole, we prefer oleaginous to saline aperients in such cases. Leeches, followed by the application of hot poultices, should always precede the use of blisters.

Hepatic Inflammation and Abscess.—There were 268 cases registered under this head. On a general average, about one in every 23 admissions to hospital belonged to this section; and rather less than one in every 10 individuals was annually affected with inflammation of the liver, or, its result, the formation of an hepatic abscess.

Among these 268 cases of Hepatic inflammation, there occurred 21 fatal cases of Hepatic abscess. In one-half of the cases, the patients do not appear to have had any serious disease before the formation of the abscess. Between a third and a fourth of them had suffered from Dysentery. Others had suffered from Rheumatism.

"Upon the whole," says Dr. G., "with the exception of Dysentery, and perhaps of Rheumatism, it does not appear that abscess of the liver is often the consequence of any other disorder. Dysentery, too, is a very common symptom of Hepatic abscess; and it is not improbable that, in some of the cases registered thus, the Hepatic affection may have already occurred, and proved, in such instances, a cause, and not a consequence, of the dysenteric disorder. The connection, in short, between Dysentery and Hepatic abscess, is such, that it is difficult, in certain cases, to say which is the original disease; and, although due weight has been given to the attendant circumstances in forming a diagnosis, there has been a doubt—in arranging some of the Dysenteric cases of patients, where Hepatic affection has afterwards become more decided—whether they should have been considered as idiopathic diseases, or as symptomatic of the liver disorder."—P. 311.

In some cases there was but a single abscess, in others there were several. Of 29 cases, the abscess was single in 23, and multiple in 6. When the abscess was single, it was much more frequently in the right than in the left lobe. The small abscesses were generally diffused through the entire substance of the liver. With respect to the situation of the solitary abscesses, it appears that, of the 23 cases, " 12 were seated in the upper part of the lobe, near to the diaphragm; and in three of these, the disease had communicated with the lungs, and been partly brought up, by expectoration. In four, the site of the abscess was more deep seated, or in the posterior part of the liver. In one, it was placed near to its convex surface, and had been evacuated by an artificial opening in the right

hypochondriac region; in another it was large and solitary, in the right lobe, but extending partly to the left; and in three, the abscess was situated near the margin of the right lobe, which adhered, in two of these cases, to the colon. Of the solitary abscesses in the left lobe, one was situated near its concave surface, where it had burst into the cavity of the abdomen, and the other was placed in the upper part of the lobe."

The *symptoms* of Hepatic abscess are often exceedingly indistinct and obscure. There is no single symptom which is present in all cases, however similar these may be in the seat and size of the existing abscess: or which may not attend other cases of hepatic disorder, when no suppuration has taken place. Hence the not unfrequent errors that are committed in the diagnosis of the disease in question. Many cases are set down as examples of Dysentery, or chronic Diarrhœa; while in others, the symptoms have been attributed to Fever, either paroxysmal or continued, Abdominal Inflammation, &c. With respect to *pain* in the right hypochondrium, as one of the symptoms, our author remarks that—

"Although frequently indicative of the locality of the disease, it has in some instances been wanting altogether, or during certain periods of the complaint; in others it has been felt at a distance from the situation of the abscess; and it has occasionally shifted its chief site during the progress of the disorder. This symptom has also varied greatly in degree as well as in its extent; and this independently of the size of the abscess."—P. 344.

Dr. Parkes makes very similar remarks. According to Dr. Geddes' experience, pain in the right shoulder was present in almost every case where the abscess was seated in the convex part of the liver; but, when it was near the under surface, this symptom was generally absent. Sometimes it continued when the hypochondrial pain ceased; at other times it vanished, and again returned with or without the uneasiness in the side. The shoulder-pain may be felt in or about the joint, over the clavicle or scapula, or even in the upper part of the arm. One patient described it as if his arm had been dislocated.

In very many cases, there is no outward swelling or fulness to denote the existence of any purulent collection. In one case only did the abscess point externally. In four, it burst inwardly; in three of these into the substance of the lungs, and once into the cavity of the abdomen.

The character of the attendant Hectic Fever differs considerably in different cases. Sometimes it takes on all the violence of Synocha or Inflammatory fever; and at other times it is apt to be mistaken for a genuine Remittent or Intermittent. In a third set of cases, the febrile symptoms are obscure and indistinctly marked; perhaps there is nothing more than recurring rigors, followed, it may be, by slight flushes of heat. The sweatings are often very profuse; occasionally, however, they are scarcely noticed. There is, as might be supposed, considerable diversity in the state of the pulse. Whenever it is uniformly above the standard of health during the remissions of the fever, the case must be regarded as suspicious.

From what we have now said, it is obvious that, in many instances, the physician must exercise very great circumspection, and that in all he should make repeated and most pains-taking examinations, before he makes up his opinion as to the diagnosis of the malady. On the one hand, he should

most carefully avoid attaching an exaggerated importance to any one symptom that may be present, and, on the other, he has to guard himself against his suspicions being lulled by the absence of those symptoms that he might naturally expect to find. Much harm, it is obvious, must be done by errors of diagnosis in this disease; for, assuredly, if a case of incipient or existing hepatic abscess be treated as one of uncomplicated Dysentery or of Fever, positive and very serious injury may result from the treatment that will almost inevitably be pursued. It is well known that a relaxed state of the bowels is very generally present in cases of hepatic abscess, and this symptom may be so prominent as almost completely to mask every other sign. Again, in reference to fever, it seems not improbable that the system may be under the influence of febrific malaria, at the very time that serious organic mischief is going on in the liver. Such a complication will necessarily serve to render the diagnosis obscure and perplexing; as it may then be difficult to distinguish the symptoms of Hectic fever from those of malarious Paroxysmal fever.

On the interesting pathological question as to the *Relation between Hepatic Abscess and Dysentery*, the following data—collected together by Dr. Geddes, be it remembered, without reference to the point under consideration—merit notice.

"In the large intestines, the diseased appearances were generally in proportion to the degree of dysenteric affection immediately preceding death. In 13 cases, where there was either no irregularity, or the bowels were inclined to constipation, or the evacuations, although loose, were more of a diarrhœal nature, and generally attended with blood, little or no disease, with the exception of some contraction of the colon, was met with. In the remaining cases, ulceration, or disease, was discovered in all degrees, in proportion to the previous dysentery, and varying from one to two superficial ulcers in the head of the colon to general ulceration, with thickening of the large intestines. It may be remarked, that of 28 dissections, the large intestines were found considerably diseased in ten instances. One half of these occurred in the cases where there were numerous abscesses in the liver; in two the abscess was seated in the upper and posterior part of the right lobe; in one, on the margin, and in another, in the upper part of the same lobe, and in the remaining case, in the upper part of the left lobe. The circumstance most usually observed in those individuals was, that where the intestines were healthy, the abscess was found either in the upper and outer part of the right lobe, or deep seated in the lobe."—P. 363.

Besides the 31 cases* of *ascertained* Hepatic Abscess, our author gives an account of 34 cases which he regarded to be "cases of Probable Abscess in the Liver," from the character of the symptoms present, and in which a recovery seemed to take place. These symptoms, we may remark, were "pain of side affected more or less by position, pressure, or respiration; protrusion of the liver; varieties of Dysentery; pain about the right shoulder; vomiting, under various circumstances; appearances indicating either vitiation, diminution, or increase of the bilious secretion; varieties of a fever considered hectic; peculiarities of the pulse and tongue, with a disordered state of the appetite for food or drink; and lastly, disturbed sleep."

* Dr. Geddes, in discussing this very important subject, blends the results of his earlier experience in India with those of his practice during the four years that he was surgeon of the Madras European regiment.

In many of the cases under this head, the patients had suffered from dysenteric and other disorders before the development of the symptoms of hepatic suppuration. As a matter of course, there must always be more or less uncertainty as to the correctness of the diagnosis, that has been formed under such circumstances. In some instances, the probability of the opinion being correct was much increased by certain netroscopic appearances discovered in the liver, the patients having died subsequently of another disease. A brief account is given of some such cases. We shall merely notice the *post-mortem* appearances in two or three of them.

In the case of a man who had been affected with all the usual symptoms of Hepatic abscess for several months before his death, the following is the report of the dissection.

"The surface of the liver presented, in the upper part of the right lobe, a scar of an ulcer, without any adhesions there; and the substance immediately below it was hard and white. The same appearance was presented on the edge of the right lobe, where there was a slight adhesion to the colon; and the whole of the liver was somewhat paler and softer than what is natural."—P. 369.

In another case, where the patient died of dropsy, two years after the cessation of the hepatic symptoms, we read in the report of the dissection —"the liver was found to be irregularly indurated, of a pale colour; and on the outer surface of the right lobe, there was a deep indentation, as of a scar, this penetrating the substance of the liver, for some distance, in a tendinous structure."

Similar appearances were observed in three or four other cases.*

To avoid unnecessary repetition, we may as well introduce here the general conclusions which Dr. Parkes has come to respecting the connexion between Dysentery and the formation of abscess in the liver.

"I have carefully," says he, "dissected ten cases of secondary hepatic abscess, and several others, in which the dysentery and hepatitis appeared coetaneous.

"1. As ulcers exist in every case of common dysentery, and abscess only follows in some cases, the absorption of pus cannot be the true cause of production of abscess, as stated by some writers: besides, in certain cases, the ulcers in intestines, in secondary hepatic abscess, although very numerous, are small, and are in their earliest stage.

"2. I have carefully looked out for venous inflammation, and am certain that in many cases there is no process of this kind going on.

"3. If the abscess be owing to spreading of inflammation by contact, it ought always to be situated at the point nearest to the inflamed colon; but this is by no means the case.

"The mesenteric glands are enlarged and inflamed in all cases of dysentery; but,

"4. I have never seen any suppuration of them in secondary hepatic abscess.

"5. In all the cases I have examined the duodenum was free from disease, if we except an enlargement of the solitary glands generally, and of the orifices of Brunner's glands.

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* Allusion is made by M. Catteloup to the occasional occurrence of the cicatrices of former abscesses of the liver, in the bodies of some of the French soldiers who died in Algeria.—Vide *Medico-Chirurgical Review* for January of the present year, p. 61.

"6. So far from there being an immense secretion of vitiated bile, in many of my cases the hepatic secretion was suppressed; the gall-bladder was generally empty, or contained merely a thin red or brown fluid. Whatever influence these causes may have in certain cases, they are certainly not the general agents in the production of abscess."*—P. 113.

According to this gentleman's observations, the tendency to the supervention of secondary hepatic abscess is greater when the ulcers of the colon are numerous, small, and widely disseminated, than when they are large, less frequent, and are surrounded with the signs of active inflammation.

It is a curious circumstance, noted by Dr. P., that jaundice very seldom accompanies a case of hepatic abscess; for either there remains a free exit for the bile, or else its secretion is wholly arrested. Most medical writers allege that, when the last-named occurrence takes place—the complete suspension of the biliary secretion—the patient inevitably becomes jaundiced. Not so says Dr. Parkes: "I have never seen any reason to believe that the non-secretion or the non-separation of the bile from the blood will produce jaundice. I have never seen jaundice in any case of hepatic abscess, and, in fact, in no case in which secretion has been totally arrested. I therefore, with all respect, differ from those pathologists who suppose that the liver merely separates the biliary principles from the blood, as the kidneys do urea."

It is the opinion of certain writers that Hepatic Abscesses sometimes become speedily absorbed, the purulent matter being supposed to be eliminated and carried off either by the bowels or the kidneys.

"The author's experience does not tend to strengthen these inferences. In several instances, a discharge of a purulent appearance has been observed from the bowels; but this has not been peculiar to cases wherein abscess in the liver, or in any other organ, has been suspected. In some, it has been apparently the discharge from an ulcerated state of the bowels, as the consequence of a dysenteric affection. In others, it seems to have been the result of a change in the mucous secretion of the intestines; and being occasionally attended with a state of constipation, has been quickly removed by the aid of purgatives. An appearance of purulent matter in the urine, again, has been only recorded in one case of all those included under the head of Hepatic Inflammation."—P. 399.

Dr. Parkes will not admit that the purulent matter is ever carried off by the kidneys. "I have seen thick, apparently purulent, deposits in the urine, and have heard them called 'decidedly purulent;' but these are mere collections of vesical mucus, of a particular kind, and exactly similar appearances† are seen in pyelitis and catarrhal inflammation of the bladder,

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* These observations are more especially applicable to cases of *chronic* Dysentery. With respect to the *acute* stage of the disease, Dr. P. says that, according to his experience, Hepatic abscess will be found present in every fifth or sixth patient dying at this stage of the malady.

† "I am aware that this opinion is opposed to that of many Indian surgeons who attach great importance to this so-called appearance of pus, and believe they can distinguish it by the eye from the vesical mucus, resulting perhaps from reaction of acrid or altered urine. (Conwell and others). One thing I must protest against, viz. the opinion that pain and prominence of the right side, subsiding after such an appearance in the urine, is a proof that abscess existed and has been cured. Such an opinion is a complete begging of the question."

where there is no suspicion of pus being formed any where. These deposits are soluble with effervescence in acetic and nitric acids. No coagulation was ever observed from heat or nitric acid."

This gentleman considers that "the question whether abscess, once formed in the liver, ever becomes cured by the absorption of the pus, must be considered still undecided." He evidently inclines to the negative side of the question.

Hepatic Abscess is comparatively rare among the natives of India. On this point, Mr. Parkes makes the following remarks :—

"An interesting question arises, as to the infrequency of hepatic abscess among the dark nations of India, compared to the common occurrence of this condition in Europeans resident in the same parts. In Asiatics, equally important changes take place in the large intestines in dysentery, and in the liver and spleen in malignant and remittent fever, as in Europeans. I have been fortunate enough to have had opportunities of observing this both in Bengalees of different castes and nations, and in Hindoos, from the south and west of the Peninsula, and in Burmans. Why, in hepatic diseases, so wide a difference should occur, is a circumstance that may hereafter throw much light on the pathology of liver diseases. There are many peculiarities in the diseases of Asiatics which will some time or other amply repay research. I have noticed that in phthisis the process of softening seems delayed. Phthisis is not uncommon among Hindoos; and I have seen several cases in which it was not accompanied by cough and expectoration, but simply by debility and diarrhœa. In such instances the tubercle in the lung assumes the form of hard grey masses or nodules of various sizes, without yellow matter or appearance of softening."—P. 249.

We have no intention to enter upon the subject of the *treatment* of Hepatitis and Hepatic Abscess. The only point, that we wish to draw the reader's attention to at present, is the very important—and, we much fear, not well understood—one as to the exhibition of Mercury, when there is reason to believe that an abscess is forming, or has already formed, in the substance of the liver.

Dr. Geddes says of the cases, where the presence of hepatic suppuration was actually ascertained after death, that "the system was attempted to be brought under the action of mercury ; but, although in one or two of the earlier stages of the more chronic cases, some degree of salivation was excited, in general no effect was produced by the calomel, beyond a slight ulceration of the gums."

This statement is in accordance with what has been said by Annesley, and other experienced writers : vide *Medico-Chirurgical Review*, for Oct. 1845, p. 503. Dr. Parkes, however, entirely denies its truth, and says that "mercury, when properly administered, produces salivation as rapidly in Hepatic Abscess, as in any other inflammatory disease." The exemption from salivation in some cases, he attributes to constitutional peculiarities in the individuals. There seems very little doubt but that the system may become mercurialized, when suppuration is going on; but it is equally true that the existence of this morbid process exerts a strong counteracting influence to the full action of the mineral. But, be this as it may, the important point for the practical physician is to determine if this powerful remedy should be ever given with the intention of affecting the system, when there are good grounds to believe that suppuration of the liver has actually taken place. We regret to say that neither of our

authors gives any very satisfactory information upon this question. In our review of Dr. Budd on the Liver, already alluded to, we expressed our opinion that mercury should never be administered under such circumstances, except, indeed, for the relief of an occasional symptom; adding that, "it will then do no possible good, and will only serve to exhaust the patient's strength, and exasperate, by its depressing influence, the local malady." We regret to find that Dr. Geddes has not arrived at the same conclusion: at least, his practice has not been in accordance with it. On more than one occasion, he talks of exciting the absorption of the purulent matter by exhibiting small doses of mercury; but although we read in the history of some of the cases of "probable abscess of the liver," that there was but little appearance of general improvement until the mouth had been affected with mercury, we do not feel in the slightest degree disposed to retract or in any way modify the opinion which we have expressed—feeling strongly assured that a vast deal of mischief is annually done, among our East India population, by the excessive and often indiscriminate use of mercury in large and repeated doses, not only in Hepatic suppuration, but also in many other diseases. Dr. Parkes has, we are glad to find, adopted nearly the same views; for he says:—

"Although salivation can be thus established, it appears to me to have a very bad effect on the suppurating liver. It often increases the purging; or if, as sometimes happens, the blood in the stools disappears after its supervention, the rapidity of increase of the abscess appears augmented. It is generally a difficult thing to discover the real effect, beneficial or otherwise, of a medicine; but I think no difference of opinion can exist on this point, that mercurial action, during hepatic suppuration, does no good, and appears to do harm." P. 246.

It must be confessed, however, that Dr. P., in other passages of his work, seems to sanction the use of mercury in cases of hepatic abscess. And then what says Dr. Sutherland?

"In the medical treatment of hepatic abscess, the use of the mercurials is indicated until such period of the disease as the abscess shows a tendency to point, or in which rupture of it appears to be close at hand, or actually to have taken place. The employment of mercurials, when this state exists, must only hasten the fatal termination of the case, and preclude the very distant chance of life that the subject of hepatic abscess possesses through the processes of nature. In the earlier conditions of abscess, it is through the action of mercurials chiefly that the process of absorption necessary to effect a cure is to be set up; but after continuing the attempt by mercurials, and no appearance of the effect being produced, a prolonged persistence in mercurials would materially lessen the patient's chance of recovery. The case must then be left to nature, and assisted by nutritive diet." P. 92.

We are the more surprised at this opinion, as Dr. S. is anything but friendly to the free use of Mercury in some tropical diseases, to which it has been considered by most writers as especially applicable. One of these is that which we now proceed to examine, viz.

Dysentery.—As this disease is not treated of by Dr. Geddes, it is chiefly to Dr. Parkes' work that we shall refer in the following remarks. The

pathological characters of the disease are thus very faithfully given by this gentleman :—

"Admitting the inflammatory nature of dysentery, the peculiarity about it seems to be, that ulceration of the large intestines occurs with great rapidity, and except in one rare form, a case never presents true dysenteric symptoms without ulceration being present. It is evidently not from the severity of the inflammation that ulceration is so rapidly and so constantly produced, for it occurs in the comparatively slight cases, as is proved by the opportunities we sometimes have of examining these, after sudden death by cholera or coup-de-soleil: the proofs of inflammation, apart from ulceration, are often only just visible on post-mortem examination. Moreover, the same amount of inflammation exists every day in the stomach and duodenum without being followed by ulceration; and, to view it in another aspect, it is hardly conceivable that inflammatory action, in cases of dysentery, so severe as to produce almost universal ulceration of from one to four feet of large intestine, could exist without co-ordinate constitutional disturbance. If the same amount (in point of space) of inflammation and ulceration occurred in the skin (the analogue and prototype of the mucous membrane), the pyrexia would be extreme. And yet cases of very severe but nonfebrile dysentery are constantly witnessed."—P. 3.

The cause of the ulcerative process supervening so rapidly upon the inflammatory one is ascribed "to the glands of the mucous membrane being particularly implicated:" these glands are most numerous in the cæcum, the ascending colon, and the sigmoid flexure, and less so in the transverse and descending colon. Our author describes, with great minuteness, the anatomical changes discoverable in these glands in the different stages of the disease. We can notice but one passage :—

"Two important anatomical varieties of dysentery, are those in which ulcerations are found chiefly in the cæcum, and those in which they are principally confined to the rectum. These varieties are also generally the result of local irritants, and produce peculiar symptoms.

"Thus when the cæcum is the part affected the tenesmus is often absent or slight, the stools sometimes are partially fæculent, but there is great pain on pressure over the cæcum, and a very perceptible fulness in that region, arising either from arrested fæces or secretions, or from spreading of inflammation to the other coats and surrounding parts, producing inflammatory swelling and œdema, or in a later stage from absolute thickening of the coats of the cæcum from effusion of lymph. If the inflammation runs high, certain effects ensue, viz. ulceration of the ilio-colic valve, and the intussusception and strangulation of some parts of ilium in the cæcum. I have seen the commencement of this state of things, but have never witnessed that extreme ulceration which is mentioned by Mr. Twining.

"The anatomical variety of ulceration solely or principally in the rectum also has its peculiar symptoms; here there is generally intense tenesmus, and the stools are often nearly pure dark blood, and if the case is neglected some portion of the mucous membrane speedily sloughs, and protrudes from the anus.

"The mesenteric glands are always enlarged in dysentery, and are sometimes acutely inflamed. I have never seen them suppurated."—P. 14.

That the Liver suffers more or less seriously in every case of Dysentery is proved by the well-attested fact, that the bile is invariably altered in its qualities. In many cases, this secretion is all but suspended; no bile passes by the stools, and yet none is absorbed into the system. Sometimes it is found to be thin and not viscid, transparent, of a brownish red colour,

and having numerous crystalline-like particles suspended in it. At other times, the gall-bladder is full of a dark green, thick, and very viscid bile —so stringy, that it may be drawn out in filaments three or four feet in length.

With respect to the gastro-enteric membrane, it is declared to be, in most cases of Dysentery, "healthy; in all cases of simple dysentery the alterations in the canal are circumscribed by the ilio-colic valve. In scorbutic dysentery the ilium is affected, as is mentioned under the head of that complication. Gastro-enteritis is at certain times an accompaniment of dysentery, and is a dangerous complication, on account of the obstinate exhausting vomiting which may attend it. There are various shades of vivid, dark, or hæmorrhagic redness in small intestines, particularly ilium; enlargement and occasional ulceration in a small degree of Peyer's and of the solitary glands, most usually of the latter.*"

Dr. Parkes says that, in dysentery, the urine in general very rapidly decomposes, and acquires an ammoniacal smell. As we have already alluded to the relation between Dysentery and Hepatitis, it is unnecessary to say more upon the subject now. Our author agrees with Annesley and other writers of note in admitting that the two diseases may stand, the one to the other, either as cause or effect; for there is one form of "dysentery supervening to disease of the liver," and there is another, "when the affection of the liver supervenes to dysentery." It is often exceedingly difficult to determine which is the primary, and which is the secondary, affection. Some writers have been too much in the habit of asserting that Dysentery is generally, or at least often, caused by a vitiated and acrid state of the bile. Dr. Parkes objects to this doctrine for the following very satisfactory reasons:—

"First.—Because, when this deranged secretion does occur, we have bilious diarrhœa, and not true dysentery.

"Second.—Because there is often no irritation of the mucous membrane of the small intestines, to which we should suppose the so-called irritating hepatic secretion to be at least as hurtful.

"Third.—Because, under this supposition, the dysentery should be present during the whole course of the disease, and in every case.

"Fourth.—Because dysentery, in like manner, complicates some diseases of the spleen which pour out no irritating secretion.

"Fifth.—Moreover, the secretion has not been proved by chemical analysis, or any other test, to be possessed of irritating qualities; its irritating properties have been supposed, and the supposition has been received as if it were an established fact.

"Sixth.—In some cases, in which the secretion really does appear to be irritating, viz. by producing excoriations round the anus, and scalding the patient when he goes to stool, the mucous membrane of the colon, previously ulcerated in antecedent dysentery has been found by me to be healing rapidly.

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* "I have not mentioned the adynamic variety, because this is almost always a case complicated with remittent fever or with typhus. As I have observed in a subsequent section, among the dark races the ulcers are atonic, and may require stimulants; but by the term 'adynamic dysentery' is meant more than this, viz., a failure of the powers of life with dysentery superadded. I have never seen this apart from fever or cholera."

"So far from irritating secretion producing the consecutive dysentery, I have been led to think that the absence of all secretion has been the cause of this disease; in other words, when hepatitis has terminated in partial suppuration, and bile is still secreted, although altered in appearance, then there is no dysentery; whereas, when from extent or peculiar situation of abscess no bile is secreted, dysentery appears to supervene. This opinion is founded on a few observations only, and I mention it here simply that its correctness may be tested by the observations of others."—P. 59.

The reader will probably perceive in these remarks a certain coincidence with the views so earnestly insisted upon by Dr. Macgregor, in his late work on the diseases of the North-Western Provinces of India—vide the number of this Journal for last April.

Besides Hepatic Disease, Dysentery is often associated with Remittent or Intermittent Fever. We have so recently dwelt upon this point, in our description of the prevailing diseases among our soldiers and sailors in China, that it is not necessary to do more than merely to refer to it. We pass on to a brief notice of

Scorbutic Dysentery.—This is a most serious and unmanageable form of the disease. It proved very fatal to our troops at Rangoon in 1824; and is always apt to occur on board transports and other vessels, when the ventilation and food are faulty. Every now and then, it will be found in some soldiers at particular stations, although the rest of the men remain quite exempt. Probably, much depends upon the original vigour and strength of constitution, as well as upon the condition of the general health of the patient for some time previous to the attack.

"A soldier will often have a certain amount of scurvy for a short time, for which he never thinks of coming into the hospital; he is annoyed with various symptoms of dyspepsia, with rheumatic pains in the legs, perhaps with an occasional eruption in the same parts of a few purpuric spots or slight ecchymoses; the rheumatic pains are chiefly in the calves, hams, or ankles, and sometimes there is burning of the feet; there is occasional bleeding from the gums, and when these are examined they are found to be slightly swollen and of a dark red colour. The whole amount of the disease, however, is trifling, and a man will generally do his duty, and gradually recover without medical aid. If, however, from any cause an attack of remittent fever or of dysentery supervenes, then this constitutional taint at once appears in the way in which it modifies the course of these complaints."—P. 123.

In the Scorbutic form of Dysentery, the ilium is much more frequently the seat of ulceration and hæmorrhage from the bowels, and perforations of their coats are of more frequent occurrence, than in the ordinary form of the disease. It deserves to be particularly noticed that the mouth is always very readily affected with mercury, whenever there is any tendency to the scorbutic diathesis.

Treatment of Dysentery.—Is Mr. Parkes warranted in asserting that, "in the treatment of common acute dysentery, we have an infallible guide in the appearance of the evacuations? Many other symptoms, as general pyrexia, tenderness of abdomen, heat in course of colon, tormina and tenesmus, when present, are valuable as accessory phenomena; but the ab-

sence of any or all of these is never an indication, when the stools point out an opposite course of treatment."

We much doubt whether any judicious practitioner would determine his line of treatment from the characters of the stools alone, irrespective of other symptoms; and indeed Mr. Parkes, in the very next sentence after that now quoted, admits the importance of such aid in a therapeutic point of view: for he says:—

"As long as the stools are numerous (the attack being recent and uncomplicated), bloody, sanious, dark, and copious or scanty, lymphy, shreddy, or like meat-washings; or a mixture of blood and slime with or without partial fæculence or nearly pure blood, florid, or dark, mixed with a peculiar red mucus; or fæculent, yellow, copious liquid, and stained with blood; and more particularly when with these symptoms there is pain on pressure, and great tenderness, as is the case in most instances, or heat in course of colon, depletion must be actively employed."—P. 140.

In the early stage of the disease, our author strongly recommends, after venesection has been employed, the repeated and free application of leeches over the cæcum and sigmoid flexure—"three times in 24 hours, till the stools become fæculent." This practice may require to be persevered in for three or four days; after which, the daily number of leeches may generally be lessened. If there be much tenesmus, leeches to the anus give great relief. After bleeding, general and local, mild oleaginous purgatives are the most useful remedies in the early stage of dysentery.* Opium, alone or in conjunction with calomel (3-5 grains), or with blue-pill and ipecac., will generally require to be administered at the same time. Dr. Parkes is not friendly, upon the whole, to the exhibition of large doses of calomel with the view of rapidly affecting the system. That the practice often succeeds, he does not deny; but then it is to be remembered that—not to mention its very frequent failure—the subsequent effects upon the health of the patient are often injurious. It is invariably contra-indicated in the adynamic and the scorbutic forms of the disease; nor should its use ever be continued when there is reason to believe that hepatic suppuration has ensued. The therapeutic action of this potent drug appears mainly to consist in relieving congestion of the capillaries, and in causing absorption of effused lymph. Hence, in our author's opinion, "the real indication for salivation is the effusion of lymph; and consequently in chronic dysentery, and in the after stages of very severe acute dysentery, in which convalescence is so protracted as to approach chronic dysentery, ptyalism slowly produced, and carried to the point of a very gentle action on the mouth, is invaluable."†

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* Sulphur, given in drachm doses at bed-time, has been found by Twining and other practitioners to suit better, in some cases of chronic Dysentery, than any other aperient.

† "The preparation," says Dr. P., "which I have found most useful, is the Bichloride of Mercury—commencing with doses of one-eighth to one-sixth of a grain—in combination with the preparation of cinchona. * * * The blue-pill, or small doses of calomel, with ipecacuan., gentian and taraxacum may be substituted. Blisters, and frictions over the abdomen with a mixture of Iodine and mercurial ointment, are to be used according to circumstances."

And subsequently he remarks :—

"For my own part I have ceased to use mercury in dysentery, in any other way than as an alterative, except in chronic and long protracted and recurrent acute cases. I never aim at ptyalism, and can confidently assert that my recoveries have been greater in number, and more complete, since I in a great measure abandoned the use of mercury, than when I gave it in large quantities." P. 145.

Dr. Parkes does not seem to have used Emetics (we need scarcely say of Ipecacuan), in the early stage of acute dysentery; although they are certainly among the safest and most effectual of all remedies. He reports favourably of the nitric and nitro-muriatic acids in mild cases.*

"Injections of opium, ipecacuanha, acetate of lead, cold water, suppositories of opium and ipecacuanha, give great relief to the tenesmus. Injections of the acetate of lead in large doses, such as one drachm every four hours, are sometimes very useful.

"The warm bath relieves tenesmus, and the irritability of the bladder, which so commonly produces frequent painful and difficult micturition. Cold water injections sometimes relieve this also."—P. 147.

In the *adynamic* form of Dysentery, Mr. P. says that "alum combined with catechu and camphor is the best treatment, with small and frequently-repeated doses of Dover's powder between each dose of alum. Injections of alum are very useful in that variety where the intestine is so disorganised as to tear like wet paper after death." In the *scorbutic* variety, the results of treatment are often most unsatisfactory. All depletory means must be avoided altogether, or only used with the greatest circumspection. We much doubt whether mercury in any form should be administered: Dr. Parkes advises small doses of pil. hydr. ipecacuan and nitric acid, opium in enemata, a farinaceous diet with vegetables and lemonade.†

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* In a recent number, we suggested to East India practitioners to ascertain the state, whether it be alkaline or acid, of the alvine evacuations in such a disease as dysentery. It seems not improbable that we might thus obtain a guide, in certain cases at least, for the selection of some of our remedies.

† The following practical remarks of Dr. Fergusson (in his "Notes and Recollections," reviewed in our last number) on the allowance of fruits and vegetables in acute Dysentery deserve notice, as a contrary opinion too generally prevails.

"Because the acid and subacid fruits sometimes occasion griping when in health, these, and vegetables of every kind, are strictly prohibited. They are, however, amongst the best remedies. Nearly a hundred years ago, Sir John Pringle, one of the best physicians our armies ever possessed, proclaimed that ripe grapes were a cure for dysentery. The Portuguese and Spanish physicians, when I was in the Peninsula, went farther, and to ripe subacid fruits of every kind, added lemon-juice, with the best effects. Our own faculty, in different parts of the world, have highly lauded the mineral acids, more especially the nitric, and, in an epidemic dysentery which not very long ago afflicted Ireland, after one of our best summers, cream of tartar in large doses was found to be nearly as beneficial as mercury; in short, the acids, in every shape, but more especially in that of ripe fruits, will be found excellent remedies by all who can overcome their prejudices so far as to give them a fair trial." * * * *

"I believe, therefore, that the free, but not immoderate, use of fruits, by assist-

Whenever there is any reason to believe that Dysentery is at all complicated with co-existing Remittent Fever, we must never forget to combine quinine with our anti-dysenteric remedies. In closing these remarks on the treatment of Dysentery, it may not be amiss to remind the young practitioner that the same line of practice is not suitable in all seasons, or to all individuals alike. One year, the disease will bear depletion much better than another; and what may suit the robust and well-nourished European will be all but life-destroying to the feeble vegetable-feeding Hindoo.

RHEUMATISM.

This term is applied by Dr. Geddes to those cases, "in which pain in periosteal, ligamentous, or muscular structure, has formed the chief symptom of disease; attended generally, in the two first, with swelling and other symptoms of inflammation." During the five years from 1828 to 1833, there were between 570 and 580 admissions into the hospital from this disease. One season of the year does not seem to have been more favourable for its development than another. On the whole, fewer cases occurred during the cold than during the hot months. Out of 160 patients, the muscles and ligaments were the parts chiefly affected in 47; these structures along with the periosteum in 43; the muscles only in 23, and the ligaments only in 28; in 5, the disease was confined to the periosteum; in 8, this structure was affected along with the joints, and in 3 with that of the muscles. As might be expected, the amount of complication was usually much greater in the relapse, than in the primary, cases. The rheumatic affection was not unfrequently associated with Neuralgia in some part or another. One-half of the cases appear to have been attended with febrile re-action. This was however seldom very severe. In one-half of the febrile cases, the fever was regularly or irregularly paroxysmal —either remittent or intermittent. The number of cases of relapse was very considerable. In two-thirds of the cases, the duration of treatment was from 4 to 20 days; in the remaining third, it extended from 20 to 60 days and upwards. With respect to the causes, predisposing and exciting, of the disease, the operation of previous Syphilis appears to have been the most frequent; and this, whether mercury had been used for its cure or not. The general results of our author's observations on this head are, "that from constitutional peculiarities, the syphilitic virus gives a tendency to the occurrence of Rheumatic Inflammation; and it is probable that this disposition is increased by the exhibition of mercury, when the effects of this medicine are not so severe as to induce the patient to avoid

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ing to keep the bowels soluble, is at all times a preservative against dysentery; and that, when the disease is present, the same use is not only harmless, but, as in the case of the dysentery in Trinidad, may furnish a most important remedy towards a cure. For, if we examine the dismal records of this scourge to our fleets and armies, we shall find that its worst ravages have been seen amongst the famished garrisons of besieged towns, or in ships remote from land, while navigating the tropical seas, or in barren encampments, where fruits could not be found."

exposure during the period of its being given to him. In several instances too long confinement to hospital, or to one position, by the ulceration of a suppurated bubo, has been considered to assist greatly in bringing into action the rheumatic diathesis."

Dr. Geddes expressly says that, while recognising the frequent influence of previous syphilitic disease in the production of Rheumatism, it is not to be supposed that the cases, so considered by him, were instances of merely secondary syphilis. "The absence of any other symptom of the venereal disease, either occurring between it and the rheumatic affection, or at the same time as the latter, or subsequent to it; and the final cessation, in many instances, of the rheumatic attack without the aid of mercury," serve to show that such was not the case. The only peculiarities of Syphilitic Rheumatism seem to be its great tendency to affect single joints, and the small ligaments, as those of the hands, feet, or of the os sacrum, and its less frequent implication of muscular parts, and of the corresponding joints on the two sides of the body. This form of rheumatism is apt to be tedious, and is perhaps more liable to relapse than any other kind of the disease.

Not a few rheumatic cases occur after attacks of Fever, Dysentery, Cholera, and Hepatic Disease, aided, it may or may not be, by the exhibition of mercury. The constitution of such patients was usually in a debilitated, and perhaps also a depraved, state. In another set of cases, the rheumatic affection commenced without any appreciable antecedent disease. The number of cases, in which the disease exhibited the train of symptoms characteristic of simple Acute Rheumatism, as the disease is seen in this country, was very small.

As another evidence that the majority of cases of rheumatic disease in the East Indies cannot be viewed as entirely similar to what we are in the habit of meeting with at home, we may point to the consequences that so frequently follow upon its attacks.

"In all classes, the frequent recurrence of the disease, or its lengthened duration upon any one occasion, renders the patient pale, emaciated, and crippled; and other disorders, in some individuals, become generated, which eventually, in a great measure, eclipse the original disease. The most common and formidable of these are, ulcers on the skin, and certain symptoms referred to hepatic disease. By either, or, as is usual, by both of these disorders combined, or succeeding each other, Rheumatism has occasionally proved fatal; and when such symptoms are in a less degree, or when the disease has been confined to frequent attacks of, or long continued rheumatic disorder, it very often is a cause of men becoming invalided, or discharged from the service, or of officers being obliged to proceed to Europe for the benefit of their health. In the course of five years six soldiers were invalided, or discharged the service, in consequence of Rheumatism; and four of these were young men, two of whom, discharged in 1829, appear to have owed the origin of their disease to syphilis."—P. 473.

The entire system falls into a cachectic condition, which is often manifested by the formation of Ulcers or various cutaneous eruptions, affections of different bones, and general debility and emaciation. In such cases, when they prove fatal, the liver is usually found more or less decidedly and extensively diseased. The characters of the *Rheumatic Ulcer* are described to be these. It is round and irritable; the edges are raised,

thickened, abrupt, or everted; there is more or less redness around it; and its surface is irregular and dirty, with an adhesive matter or a sloughy substance adhering to it. The sore is usually painful, and extends by ulceration, occasionally by sloughing. The parts most frequently affected are the lower and upper extremities; occasionally, the cheek is the seat of it. The ulcer is sometimes single; at other times there are three or four in different parts of the body.

There is another form of cutaneous ulceration observed in rheumatic patients in the East; it is thus described by Dr. Geddes.

"In the remaining cases the ulcers were more numerous, and diffused over the skin, than in those just mentioned; and, at the same time, the characteristic features of all the sores were alike. The commencement of these has been observed under two forms; first, that of a small scab, from below which purulent matter has exuded, and on the separation of which, a superficial round sore has presented itself; and, secondly, as a phlegmonous, or furunculous tumour, containing more or less of a sloughy matter, on the separation of which a sore becomes formed. This ulcer, in its progress afterwards, when disposed to increase, becomes painful, and assumes an irregular bloody surface, with raised and abrupt edges; and its margin is either of a circular shape, or irregular, from any peculiar activity in certain parts of the ulceration. This goes on rapidly for a short period, assisted, occasionally, by a little sloughing; and there is, at times, a slight degree of hæmorrhage. The edge of the ulcer then begins to be less prominent and abrupt; the appearance of the sore is somewhat cleaner; and fungus granulations rise in its centre, the pain becoming less. From this stage, the chief characteristic features of these ulcers present themselves. These are owing to the irregularities which take place in the healing process. Granulation, cicatrization, and ulceration, are seen going on in the same sore; and a very common appearance presents itself in consequence: namely, a mass of granulation, generally spongy, and raised above the level of the surrounding skin; or a cicatrized spot in the centre, with a ring around of ulceration, deep, and with edges more or less raised and abrupt, in proportion to the violence with which the ulcerating process is going on. Sometimes, again, the sore is cicatrized at one side, and is extending by ulceration on the other. While some ulcers present this irregular appearance, in other situations they are observed, at the same time, to be altogether in an ulcerating state. It is, also, no unusual occurrence to witness places which had been entirely cicatrized, becoming painful: a small round sore is formed, and this soon occupies the whole of the new skin. Ulcers are accordingly seen upon the patient in all stages; and he often becomes detained in hospital for a long period: partly from the difficulty with which the healing process is thoroughly established, and partly from the tendency there is to a recurrence of the ulceration when the sore had become, in a great measure, or entirely cicatrized."—P. 482.

These ulcers were most frequently observed about the legs or thighs; sometimes on the arm or wrist, the back, or even the forehead. In some of the cases there was a lienteric diarrhœa present. A syphilitic taint existed in a few of the patients.

The *Treatment of Rheumatic disease* varies, as a matter of course, according to not only the severity of the symptoms, but also the nature of the constitutional complication. It would seem that, out of upwards of 300 cases treated by Dr. Geddes, in 17 only was it deemed necessary to have recourse to general bloodletting. He omits to say anything respecting the state of the blood drawn. In one-third of the cases, mercury,

carried to the extent of affecting the mouth, was the remedy chiefly trusted to. It was usually associated either with antiphlogistic remedies, or with opium, antimonials, colchicum or quinine. The severity of the rheumatic affection very generally subsided as soon as the symptoms of mercurial action became evident. The average stay in the hospital of those cases, which gave way to this treatment, was about 18 days. The usual dose of Calomel was from one to five grains, two, three, or four times a day; combined with antimony, when the pyrexia was high; and with quinine, where this showed a paroxysmal character, or where a degree of frequency of pulse existed after the other symptoms of fever had disappeared.

"In those cases where tedious ulcers occurred, and did not give way to local applications, a change was observed quickly to take place on the exhibition of mercury; but this was frequently of a temporary nature only, and, even under its continued use, the sores occasionally reverted to their original appearance; while in some instances, on its being discontinued, they began to heal. In cases of single ulcers, the curative process was frequently excited on the mouth becoming affected."—P. 485.

Colchicum does not appear to have been found very successful in the practice of Dr. Geddes: it has been suspected that some of its preparations become inactive in a tropical country. Neither is Sarsaparilla mentioned with much praise. It is curious that no notice is made of the Hydriodate of potash having ever been used. This is the more strange as so large a proportion of the cases seems to have been of that very class in which the remedy would have been prescribed in this country. Change of climate is, in many instances, absolutely necessary for the restoration of the health.

In quitting the subject of Rheumatism as described by our author, we may remark that it is abundantly obvious that this disease in the East Indies is much more frequently connected with an unhealthy or cachectic state of the system, than it is with us at home. It is not nearly so inflammatory in its character, and will not bear the same active depletory practice. It would have been more satisfactory, had Dr. Geddes informed us of the state of the blood when that was drawn, and of that of the urine in all cases. We are strongly impressed with the idea that the use of alkaline salts, in union with colchicum and hyosciamus or small doses of opium, would be well suited to a large proportion of the cases described by our author. Quinine might, or might not, be advantageously given at the same time, according to the character of the existing fever, the strength of the patient's constitution, and so forth.

After the ample analysis which we have given of the works before us, it is scarcely necessary to say a word in praise of their merits. The present volume,—for we are promised a second—of Dr. Geddes reflects the highest credit upon him for indefatigable industry and professional zeal. We strongly recommend every medical man going to the East Indies to have a copy of it at his side, as affording an excellent pattern for him to follow in the accumulation and arrangement of his observations, when engaged in practice. Dr. Geddes has done for the symptoms of the diseases which he describes, what Lewis has done for the necroscopic phenomena of fever, phthisis, &c. With what success we may leave our readers to determine. The work of Dr. Parkes is one of narrower scope,

but it is equally meritorious as far as it goes. It is obvious that the author has turned all his opportunities of observation to the very best account, and that he has neglected no means of illustrating the history of Dysentery and Hepatitis, two of the most important diseases which engage the attention of the East India practitioner.

A PRACTICAL TREATISE ON THE DISEASES OF CHILDREN. By *James Milman Coley*, M.D. 8vo. pp. 460. London, 1846.

DR. COLEY states that he has been induced to publish his book for the purpose of presenting "to the Medical Profession and the Public a comprehensive work on the Diseases of Infants and Children, which the physician, the surgeon, and the general practitioner may consult as a work of reference." We are much pleased in being able to state that the words "the Public" in the above sentence form a mere superfluity, introduced we suppose, to round a period, for the misgivings we had that they were intended, to imply an attempt at the baneful practice of popularizing medical science, now so much in vogue, have not been confirmed by a perusal of the work, which is in fact solely addressed, as it ought to be, to the medical profession itself. We wish we were enabled also to state that the anticipations of its becoming a work of reference, entertained by its author, were likely to be realised. To constitute it such a much greater degree of completeness than at present characterises it would be necessary. We do not mean this as regards the number of subjects treated of, the whole of these being apparently noticed: but the symptoms, diagnosis, and pathology of many, and indeed of most, of them are far too superficially dwelt upon to allow of the work superseding those now in use. Not to mention foreign ones, we consider Evanson and Maunsel's Treatise a very superior production to the present. Dr. Coley states that Treatises on Diseases of Children are defective in not embodying accounts of maladies of a surgical nature, and he attributes this to most of the works in question having been written by physicians. He, having been engaged in general practice in the country prior to taking his degree (as indeed have many of our most esteemed physicians), and seen much surgery in a populous district, has "enjoyed singular opportunities of observing the origin and progress of surgical as well as medical cases, and acquiring that discrimination and manual dexterity, which are necessary qualifications in any one who undertakes to instruct others on subjects requiring a practical knowledge of both branches of the profession." We were not aware before, that "manual dexterity" was one of the necessary qualifications of a writer, even upon surgery, and certainly not for the description of the very limited amount of operative surgery which the diseases of children call for. However, since the author placed such stress upon this characteristic of his treatise, we turned to those portions of it which especially relate to surgery, and can only say that, if preceding writers upon Diseases of Children have not bestowed equal attention upon this branch

of the subject, they might have done so with very little trouble to themselves, inasmuch as these consist chiefly of quotations from the works of well-known writers, as *e. g.* Little upon Club Foot, Lawrence, Middlemore and Walker on Diseases of the Eyes, Liston and Velpeau upon the operation for Strabismus, &c. &c.

But not only do we think Dr. Coley's work is a superficial one, and in no-wise entitled to rank as one of reference, seeing that it brings forward no large amount of original facts and observations, or contains anything like a complete summary of those already known; but we find it defective in another point of view, namely, in neglecting to specify the relative frequency of the occurrence of the diseases treated of in children, as compared with adults, and in the not sufficiently distinguishing the modifications of treatment some of these should receive in these young subjects. In fact, the whole catalogue of ills that flesh is heir to is studiously gone through, but the special adaptation of much of the information adduced to the complaints of children is not easily perceived. In proof of what we say, we will take an example or two at random. Thus, treating of *Mercurial Salivation*, the author, never remarking upon its rarity in children, gives us a description of the symptoms as they are observed in adults, and, among these, adverts to the fatal erethismus described by Pearson. He then tells us that salivation is best relieved by eight or ten leeches applied beneath the maxilla, followed by a soft poultice. This is quoted from Dr. Watson, but suitable as it may be for adults it would often prove death to a child. Then we are recommended to use a lead gargle—that is, we suppose, when we can get the child to employ it. In *Peneumonia*, too, we have all the symptoms of the disease as it occurs in adults (among these, as an essential one, the rusty expectoration, as if expectoration of any kind were common in a child) given as a matter of course, but none of those especially observed in a child. In regard to the pathology of the disease the author is more successful in establishing the requisite distinctions, and then only because he furnishes long extracts from Rilliet and Barthez's masterly work: but when he speaks of treatment he certainly describes such only as is fitted for adults. Thus we are told the child is to be freely bled from the arm or jugular vein in the sitting posture to syncope, and that, if the treatment has been delayed, blood will have to be abstracted several times (!) to secure relief. Calomel and antimony are afterwards to be put into requisition, suspending the form, however, "when the gums are seriously affected." It is true, the author observes, that some young children as well as adults, will not bear this treatment, and many require leeching only; but he evidently believes these are mere exceptions. Indeed, he cautions us not to be deceived by apparent debility consequent on pulmonary and cerebral congestion, found in some cases, in which the heart's action becomes almost suspended, and the patient apparently dying. This condition is to be managed by first giving four or five grains of sesquicarbonate of ammonia until some regularity of circulation is produced, and then bleeding; but the detection of the patient's true state in his apparent deliquium requires a greater nicety of touch than many we fear will possess, when the pulse of a child is in question. By its careful examination the attendant will perceive "that the calibre of the artery and the firmness or density of its muscular coat are undiminished,

and that the obscure and undulating impulse affords really the perception of a struggle in the heart to carry on the circulation, rather than any deficiency in the quantity of blood in the circulation." Those who know how difficult it is to decide in some of these cases in an adult, can best appreciate its frequent impossibility in the child. However, none can doubt the propriety of the temporary exhibition of stimuli, whether in apparent or real deliquium. Throughout his work the author far too indiscriminately recommends free bleeding, seeming to be unaware of the conditions of children which contra-indicate this. We some time since ridiculed the *hematophobia* displayed by Mr. Hood, in his recent book on the diseases of Children; but when we find such rash procedures countenanced in a work purporting to be one of reference on the subject, we must allow that his is the least injurious of the two extremes.

Other examples of these imperfections might be cited, but we prefer placing before our readers some of the remarks which Dr. Coley considers as of an original character; and, indeed, while we deny that his work possesses the complete and practically useful character which he seems to claim for it, we are free to confess it contains several interesting observations, evidently the production of a reflecting mind. He regards as a mere vulgar prejudice the influence which is so generally attributed to the process of *Dentition* in disordering various portions of the economy. "It is lamentable to notice the ignorance displayed by the profession as well as the public on this subject; every concomitant disease, the exact nature of which is not obvious to their apprehension, being attributed to the teeth." Treating of diarrhœa he thus speaks regarding the effect of teething.

"I may, however, observe that purging, which happens to concur with dentition, has no necessary connection with that process. I have already explained, under the head of 'Dentition,' the effect produced upon the alimentary canal by the growth and production of the primary teeth, which is the very opposite to that of excitement. When dentition happens to be proceeding with any remarkable activity, particularly in delicate children, the processes of digestion, chylification, and even the peristaltic action of the bowels are interrupted in the same ratio, and the whole chylopoietic system rendered torpid. Hence, instead of purging, we shall always find a state of constipation prevailing, together with inaction of the liver, until the deciduous teeth are unfolded, and the delicate animo-chemical process of depositing the enamel, which requires so much organic influence, has been completed. When, therefore, mucous, muco-purulent, or purulent diarrhœa occurs during dentition, it may always be traced to chronic inflammation in the mucous follicles of the villi, produced by cold, as will be explained in the next chapter. Another striking proof that diarrhœa, and other inflammatory diseases in the bowels of infants and children, under two years of age, do not proceed from the excitement of dentition, is the fact, that whenever such diseases do occur, the process of dentition is interrupted as long as such diseases continue; as may be observed by the defective construction of the primary teeth, which happen to be forming at the time, and particularly the deposit of enamel, which, after remittent fever, severe diarrhœa, or marasmus, will be found as soon as the teeth have completely emerged from the gums by the subsequent growth of the fangs, disfigured with defects in the enamel, consisting of its total absence in transverse patches corresponding in extent with the duration and severity of the contemporaneous intestinal disease. Notwithstanding these obvious facts, writers on the diseases of children, both British and Foreign, concur in labouring to prove the correctness of their mistaken views and inverted pathology, by contending that the mucous follicles during infancy undergo rapid

development in the intestines, and that they supply the sudden and immense secretion of serous fluid occurring in diarrhœa, and thus act as a salutary check to the excitement of dentition. These pathologists, in their desire to blame the teeth for every disease appearing during the earlier periods of life, quite forget that inflammatory diarrhœa and dysentery attack individuals at all ages, even those who have shed their secondary as well as their primary teeth; and that in all the same disorganizations are discovered after death as those, which are met with in children, who happen to die before primary dentition is completed." P. 200.

With the opinions here expressed, and repeated on various occasions throughout the work, we cannot agree. The coincidence of profuse diarrhœa with difficult dentition, and of its arrest upon the teeth coming through, is of too frequent occurrence to allow of our denying their relation of cause and effect. That the diarrhœa too is not an inflammatory affection seems to us proved by its rapid induction and cessation, its frequent alternation with the non-inflammatory secretion of the salivary glands, and the measures which most effectually relieve it. Of the dependence upon, or at all events the aggravation of many, and indeed most, of the chronic, and some of the acute diseases of childhood by difficult dentition, and of the great utility of freely lancing the gums in such cases we entertain no doubt whatever, and can only wonder at any being raised by an experienced practitioner.

Erysipelas of Infants.—Dr. Coley observes that, not only his own observation, but the descriptions of writers upon the diseases of children, prove that this disease, which usually attacks the abdomen, thighs or nates of infants, is identical in its nature with the phlegmonous erysipelas of adults. By the action of the cold the vessels of the cellular adipose membrane have become obstructed, and suppuration and gangrene, if relief be not obtained, are the consequences. This disease has no analogy with superficial erratic erysipelas, or with the blush upon the abdomen which is sometimes symptomatic of erythematous inflammation of the intestinal mucous membrane.

Treatment.—Instead of trifling with the application of starch powder, or flour, as recommended by some writers on this subject, until disorganization of the cellular membrane, or gangrene, has taken place, we should make free incisions through the nodules, and indurated crimson-coloured parts, deep into the cellular and adipose membrane, which decided practice will instantly arrest the local disease, and prevent the typhoid fever, and destruction of parts, which would inevitably follow. This practice is applicable, in every situation, in which the disease may appear; and will always prevent that dreadful destruction of parts, which authors describe as resulting from the disease. The amazing enlargement of the scrotum, produced by the induration and infiltration of the loose cellular membrane of this part, added to its dark purple colour, are calculated to alarm and deter an inexperienced practitioner from adopting the practice I have recommended, and found invariably successful. The incisions, however, must be deep and unsparing; otherwise the patient will be lost by impending gangrene, and its accompanying typhoid fever. Every hard crimson nodule, in particular, should be freely divided, as that is otherwise destined to certain destruction, and contains the elements of spreading mischief; as I have already explained, in unfolding the pathology of the disease in the article, 'Phlegmonous Erysipelas.' The only applications required after the incisions have been made,

will be evaporating poultices, and afterwards folds of linen rag, moistened with warm water. The physician must not place confidence in the antiseptic properties of quina, or any other medicine, but should immediately avail himself of the surgical assistance which alone can save the patient. When proper local treatment is adopted in due time, little medicine will be required; but the vessels should not be permitted to remain unrelieved by the knife, until their contents have been effused and converted into pus; for this timid and dilatory practice will only assist nature in completing her work of destruction."—P. 375.

The author makes no mention of the danger of and means of arresting hæmorrhage consequent upon these free incisions. We have seen more than one *adult* sink under its influence.

Furunculus or Boil.—"I consider this disease nothing more than an aggravated and severe form of erythema nodosum. It consists of a phlegmonous inflammation in the cellular and adipose membrane, in which the same phenomena occur, on a more limited scale, as in phlegmonous erysipelas, to which the reader is referred. The obstruction in the small vessels circulating in the cellular membrane, occasioned by the application of cold, produces the death of that structure, as far as the disease extends, and the blood extravasated from the over-distended and ruptured capillaries becomes coagulated, and undergoes a conversion into pus-globules. The mode by which nature expels the dead mass is by external progressive absorption, on the same principle that she contrives an outlet for any other extraneous matter.

"*Treatment.*—Liquor potassæ, sarsaparilla, and various other remedies, have been mentioned by writers and lecturers for the treatment of this troublesome disease; but all who speak of it confess its obstinacy and the inefficiency of remedies. The only medicine which exerts a specific action on the disease is *bichloride* of *mercury*, which should be given in small doses. For instance, one-eighteenth of a grain in a mixture twice a day, to a child four or five years old, and one-twelfth in a mixture or pill, to a child from six to twelve years. This medicine will not only shorten the progress of the disease, when suppuration is inevitable, but will, when used sufficiently early, prevent that termination; and put a stop to the disposition to generate successive crops of boils, which, as I have before stated, are often found to torment the patient during many months. When evident derangement in the stomach and bowels exists, a dose of chloride of mercury and jalap may be given with advantage every third morning. The best local treatment, in the first instance, will be the frequent application of warm water, and when suppuration has commenced, a common poultice, which should be continued until the slough has exfoliated; after which nothing more will be required than a fold of linen, moistened with warm water, until cicatrisation takes place."—P. 119.

Treatment of Porrigo Scutulata, or Ringworm of the Scalp.—"The astonishing number of specifics recommended for this eruption, prove the intractable nature of the disease. Very few of the acrid applications mentioned in books are advisable or useful. The hair should be cut off, not shaved; and during the day time folds of linen rag, moistened in cold water, should be applied all over the head, and moistened again with cold water as often as they become dry. At bed time the head should be covered with the leaves of ivy (hedera helix.) The Irish or giant ivy, from the Canaries, is the best. The patient should take internally a grain or two of iodide of potash, twice a day, and be well purged with salts and senna every second or third morning. I have never found this treatment to fail, except in one case, which was afterwards cured by an ointment composed of one drachm of sulphate of iron, and one ounce of lard; the cold water being used at the same time. The manner in which the water dressing

acts, is by promoting evaporation, which removes the excessively redundant heats from the surface; and I suppose the ivy-leaves, possessing a kind of natural varnish, operate by exciting cutaneous perspiration, which of itself is a cooling process. This is the only mode of treatment from which I have found success in a reasonable time."—P. 88.

Dysentery in Children.—When this is in the acute inflammatory form leeches to the abdomen and a saline aperient are indicated. The sulphate of magnesia forms the best aperient. It may be given in doses of from ten grains for an infant to half-a-drachm for a child of three or four years, and repeated every four hours until the pain and purging subside. Opium and astringents are here hurtful; but tenesmus may be relieved by warm water enemata or immersion in the hip-bath. A few drops of tincture of gentian may in this case too be added to the saline aperient. The *congestive* form of the disease, in which the vital powers are eminently collapsed, is one of great danger. The heat of surface and the circulation must be restored by blankets, hot flannels, warm baths, and repeated small doses of calomel and opium. A generous diet and stimuli must also be resorted to, and, if the tenesmus is severe, an opiate injection should be given. As soon as re-action is restored, a few grains of rhubarb and magnesia may be given every few hours; and when the heat of skin becomes anormally high, and there is discharge of blood and pain, small local depletions may be resorted to. For *chronic* remittent and intermittent dysentery, the small doses of sulphate of magnesia given three times a day suffice to effect a cure.

Treatment of Pertussis.—Dr. Coley protests against the use of the numerous poisonous and powerful remedies recommended in the treatment of this disease, and believes it may, as a general rule, be cured by much milder measures. A *temperature* of 65°, maintained day and night, is that upon which he chiefly relies. It should be the same in the bed-room and sitting-room, these being on the same floor when possible. The respective rooms should be well ventilated during the patient's absence from either, but the windows of the room he is in must not be opened. The bowels are to be regulated, which, with a mixture of citrate of potash and squill, will constitute all the medical appliances during the first stage. During the second stage, the cough will be found much milder than it would have been had the child been exposed to the air; and in from six to eight weeks all symptoms of the disease will have disappeared. This regulation of the temperature may be commenced at any stage of the disease, while the cough is alarming, and the expectoration copious, nor will it interfere with any treatment the special exigencies of the case may demand. If the patient is already suffering from hectic and purulent expectoration, the regulated temperature and half or a grain of sulphate of zinc with half-a-grain of quinine dissolved in water, will relieve him speedily. Concurrent phlegmasiæ must be treated by depletion, but they will never occur when this temperature has been adopted from the beginning.

The importance of a regulated temperature in this disease is, we believe, generally acknowledged; but the difficulty, or indeed the impossibility, of

maintaining it in the houses of the great mass of patients is too obvious to need comment; and in these cases it is a fortunate thing when the progress of the disease can be curtailed, as it often can, by prussic acid or some other of the substances stigmatized by the author.

Plugging the Nares in Epistaxis.—"Pressure may be effectually applied by means of a soft bougie, a ligature, and a small piece of sponge. It should first be ascertained whether the bleeding proceeds from one or both of the nostrils. When both nostrils bleed, two pieces of sponge will be required. The patient being seated with his head held backwards, the surgeon should fasten one end of the ligature to the bougie near its smaller extremity. The bougie must then be introduced along the floor of the nostril in a horizontal direction, carrying one end of the ligature with it, till its point is visible in the fauces behind the soft palate. The bougie being gently pushed towards the back of the fauces, the ligature may be seized by a pair of common forceps and brought out through the mouth, when it should be cut away from the bougie, which should be withdrawn. The end of the ligature which has been brought through the mouth must now be formed into a noose, into which the sponge should be fixed. Lastly, the other end of the ligature left hanging out at the nostril must be drawn tight, so as to bring the sponge in close contact with the posterior opening, and tied firmly to a roll of linen placed in front of the nostril. If the sponge is well adapted to the size of the posterior aperture in the fauces, the bleeding will immediately cease after the ligature is fastened. The same process must be adopted at the other nostril when the hæmorrhage arises from both. The sponge should be removed on the third or fourth day by quietly pushing it into view by means of a bougie, and taking hold of it with the forceps; care being taken to prevent its slipping into the pharynx by retaining the ligature in the other hand during its return through the mouth. This operation is much more easy of execution both to the patient and surgeon than the common and ineffectual practice of stuffing the nostrils with lint."—P. 244.

From the specimens we have exhibited, and which might be extended had we more space, our readers will perceive that the work, however defective as a whole, contains some interesting matter.

OBSERVATIONS ON THE PRINCIPAL HOSPITALS FOR THE INSANE IN GREAT BRITAIN, FRANCE, AND GERMANY. By *J. Ray*, M.D. [American Journal of Insanity, 1846.]

THE criticisms and opinions of enlightened foreigners who have viewed our institutions with an impartial and appreciating eye, form the most valuable and welcome returns for any attentions they may have received while among us. It is on this account we purpose laying the substance of Dr. Ray's essay before our readers, as a production written with great good feeling, and by one who, from his office of superintendent of an asylum in his own country, is fully qualified to offer an authorative opinion upon what he has observed in this. Perhaps we are scarcely justified in styling our Trans-Atlantic brethren foreigners; for, however great the political hostility and animosity which have at times prevailed among the mass of the inhabitants of either country, the medical profession has

always constituted itself a neutral ground upon which all may meet with that brotherly feeling fitting to be exhibited by the descendants of one common parent. With one exception, indeed, the reciprocal professional relations are as satisfactory as can be desired, and we need hardly state that the non-existence of an international copyright constitutes that exception. Its absence is far more injurious to the Americans than to ourselves. It is true that we are the pecuniary losers by it, and it can hardly be expected that a writer connected with a Journal, which, like ours, has been systematically pirated for years, can look at this element of the question with indifference; but the truly important aspect in which it should be regarded is the depressing effect which it exerts upon American medical literature. While numberless English works are reprinted on the other side of the Atlantic as soon as they appear, some two or three sterling productions are all our publishers, with equal facilities, have thought it worth their while to re-issue. And how can it be otherwise than that a nation which systematically relies for the supply of its literary food upon what it begs or filches from others, must be destitute of a stimulus and vigour necessary for producing it itself. Take away English re-prints and translations from French, and verily a small book-case will contain the productions of American medical (as indeed general) literature. Of the capabilities of American medical practitioners, none who have perused the original articles in their Journals, or the few good works they have published, can entertain any doubt. We there find the same practical sagacity which we are proud to say characterises the profession in our own country: but the results can never be exhibited in the form of good books, commanding an European reputation, until the vicious system of living upon the fruits of other person's labours has been discontinued. It is bad enough that no international copyright should exist between nations speaking different languages, but that it should be wanting amid people speaking the same tongue and boasting a common origin is indeed disgraceful.

To return to Dr. Ray's Essay. He availed himself of one of those opportunities which so rarely fall to the lot of medical men—a few months' leisure—for the investigation of the condition of the principal insane establishments in England and France and some of those in Germany. For the information of his countrymen he has published the results of his tour in the very interesting and useful "Journal of Insanity," conducted by Dr. Brigham, and institutes a comparison between the institutions he had just visited, and those of his own country, assigning the following reason for so doing:—

"Presuming that the results of my observations may not be entirely devoid of interest to my professional brethren, and perhaps to some others, I am induced to offer them to the public, hoping that they may be productive of good in a department of philanthropy where much yet remains to be learned. If we would have our course in it characterised by progress, it will be better to dismiss all notions of superior excellence, learn what others are doing, and be willing to receive with a teachable disposition the lessons they offer us. I fear we have been too prone to believe that our institutions for the insane are far beyond those of any other country, and in a spirit of self-complacency, have gone on year after year copying one another—too often our faults and merits alike—scarcely evincing a suspicion that anything could be learned from abroad. We do not seem

to have been aware that in Europe these institutions, for the last few years, have received a large share of public attention, and that intellectual effort and enlightened philanthropy have been devoted to their improvement, to a degree quite unparalleled in America. We should recollect that, there the greater control of the Government over all matters pertaining to the public good, and the greater wealth of the community of England, if no where else, give them an immense advantage over us in improving the condition of the public charities. Here the let-alone policy, which we have rejected in regard to trade, has been too much adopted in measures of philanthropy, which are thus abandoned to individuals whose zeal may not be according to knowledge, and whose pecuniary means are inadequate to carry out their undertakings in a generous and lofty style. Their steady, systematic, and intelligent mode of proceeding strongly contrasts with the fitfulness, irregularity, and lack of intelligence that characterise so many of our benevolent efforts, and produce an imperfect and disjointed result. It would not be strange then if it should appear that, in many respects, we have been outdone in a field of benevolent and scientific exertion where we have flattered ourselves that we shine without a rival."

We shall notice the various heads under which Dr. Ray has classified the results of his observations.

1. *The Visitation and Direction of Asylums.*—Under this head we find some well deserved censure upon the system which prevails among us. Who that has had an opportunity of witnessing the meddlesome inefficiency of visiting Justices of the Peace, and Governors of Subscription Asylums and Hospitals but must acknowledge the truth of this passage.

"The small share of the individual in the general interest, is swallowed up in the more direct and personal interest that springs from his official relations. Most of them can have but a vague idea of the duties of their office, yet they are naturally pleased with the power which it confers, and especially that kind of it most pleasing to a certain class of minds—the power of patronage. Delegating no power which they can possibly exercise themselves, and constantly hampering those whom they have entrusted with any, it has come to pass that the English asylums have probably been directed with less intelligence and disregard of unworthy considerations than any on the Continent, or in America. The visiting Justices have been described by one who has seen much of them as 'on the subject of insanity profoundly ignorant, individually irresponsible, and collectively despotic.' Indications of the spirit in which they exercise their functions, may be gathered from the rules they have made; and it is a spirit which can form no higher opinion of the duties and characters of those who are charged with the care of the insane than by regarding them like those of the keepers of jails, workhouses and prisons. * * * * * * * * * * * * In some establishments the visiting justices even participate in the executive management, upon the idea that none can be trusted but themselves. At Hanwell, they furnish themselves with keys and enter the wards, changing attendants and patients from one gallery to another, and dispensing their orders with the utmost freedom. It is not strange, that under such management, this institution should have been filled with confusion and disorder: nor can we wonder at the remedy such persons adopted, to counteract the effect of their own mischievous interference. Conceiving that a little military discipline would meet the exigency of the case, they installed into the office of Superintendent, a half-pay military officer. Of course, this arrangement could not last long, but I could not learn that any wisdom was learned from such experience."

When we consider the conduct of these meddling bodies we cannot but

admire the tact with which Dr. Conolly managed to carry on his great experiment at Hanwell, and still more to wonder at the success which attended it. And yet, after all, he was obliged to retire from the management of an establishment in which it would be difficult to say whether the prevalence of vexatious opposition or injudicious co-operation has proved most detrimental to true progress. It becomes indeed a serious question whether the well-being of the insane is much longer to depend upon the remote contingency of men of common sense being elected on the Visiting Boards, who, after assuring themselves of the capacity of their Medical Superintendent, will be content to leave the absolute management of the patients in his hands. That we are far from this point many parts of Dr. Ray's Report too clearly exhibit. Even so apparently an essential power as the dismissal of servants improperly conducting themselves is not placed in the hands of the medical superintendent, and in more than one instance they have been retained, although protested against as unfitted for their situations! Dr. Ray, too, well observes, that the whole tenour of the regulations of most asylums implies a necessity of a scrutinizing surveillance of the conduct of the superior officers of these establishments exceedingly derogatory to them. Hence, a servant not liable to dismissal by the superintendent is supposed to be more at liberty to report any misconduct of the latter that may come under his cognizance. "The suspicion is never to be dismissed that these gentlemen are ready to take advantage of any opportunity to go wrong, and need to be hedged in by a system of checks and balances. The superintendent is very much regarded in the light of an upper-servant of some cleverness and honesty, rather than of a gentleman whose talents, moral worth, and scientfic attainments have raised him to a highly responsible, arduous, and honorable position in his profession."

A rule is quoted from among those adopted at the Lincoln Asylum, and well characterised as mischievous in its tendency, as indicative of inconceivable meanness in those who made it. "Each patient discharged recovered shall be questioned by a deputation of the Board, not only with respect to the treatment of himself or herself, but also as to the treatment of the other patients." Those who know more of the medical institutions of the city of Lincoln than a casual visitor can, will feel surprised, not at this or any other absurdity being perpetrated, but that any improvement in the management of the insane should ever have been there first set on foot.

In France, the medical officers are selected from a body whose preliminary training has thoroughly qualified them for their occupation. They are appointed by the Minister of the Interior from a list of three nominated by the General Council; and the chief medical officer of the establishment exercises a paramount authority in everything regarding the interests of the patients.

2. *Officers.*—We quite agree with Dr. Ray that the distribution of duties is far more rational and efficient in America and Germany than in France and England. In the first-named country the Superintendent or *Resident Physician* is invested with paramount authority in everything which relates to the management of the patients, as also with the power of appointing and discharging the attendants. An assistant physician shares his

labours and seconds his views. The duties of the Matron are limited to the management of house-keeping, and the superintendence of the work and clothing of the female patients. How different from this simple common-sense plan is our own.

"There the officers are entrusted with less power, and that is more equally divided between them. Thus, responsibility is frittered away, and that unity of plan and purpose so necessary in maintaining the ordinary routine of service, not to speak of any higher end, is entirely wanting. In one of the largest establishments of the country I found that the officers themselves were not quite clear as to the exact limits of each other's responsibility. If the resident physician prescribed medicine, his prescription was liable to be countermanded by the visiting physician: if he moved a patient or attendant from one gallery to another, the matron might move them back again; and if the matron shut up a patient in her room, the visiting justices might, and probably would order her to be enlarged. Thus each one was constantly interfering with somebody else, and preparing for some fresh jealousy or heart-burning, disorder or dissatisfaction. * * * * * * * * * It was not perceived that there should be a radical difference between the management required for patients labouring under diseases of the body, and that most suitable for persons smitten with disorders of the mind.

"To this cause is chiefly to be attributed the slow progress which institutions for the insane have made in the way of improvement. The officer in the immediate charge of the patients had the least power to minister to their moral necessities, even if capable of perceiving and appreciating them, while the one who had the power could see nothing in his hasty and infrequent visits to call for its exercise. Even after the gross abuses that resulted from this management were unfolded, it was attempted to remedy them, not by cutting off the source of the evil, by putting the establishment into the charge of men possessing moral and scientific qualifications of a high order, and invested with the necessary power for carrying their views into effect, but by making stricter rules and a more searching espionage."

In nearly all the English establishments there is indeed a resident officer of some kind, but there is also one a mere visiting physician, and the respective duties of these officers are very loosely defined and very different in different institutions. "In some, the visiting physician seems intended for ornament rather than use, his visits being a mere matter of form, while the resident medical officer is the real chief. In others, the visiting physician is the fountain of all power the Justices have not kept to themselves, and directs the whole management, the duties of the resident medical officer being confined to the execution of his orders, which may be so general that the former reap all the credit of their success, and avoid any responsibility that may be incurred by their failure. In the rules of the Lincoln Asylum the House-Surgeon is expressly forbidden to enter the galleries of the female patients without the attendance of the matron or some responsible female!!" In a note, Dr. Ray furnishes from the pen of a friend a graphic account of the ludicrous farce performed at a *weekly* visitation of the physician of St. Luke's, during which patients were prescribed for at the rate of about fifteen per hour, the peculiarities, habits, &c., of those unfortunate individuals being enquired into in their presence, and in that of their comrades in misfortune.

The *Matron* is oftentimes a too important, and even a mischievous person, in the English asylum. "In some establishments she is the principal

member of the government on the female side of the house, completely controlling the same, and yielding to the physician only in the matter of medical treatment." No one can doubt the great utility of female influence in the management of the insane, but, for this end, it must act implicitly under the superintendence and control of the medical officer, and never in contravention to his commands. How seldom it is the case that a matron will consent to this, all who know much of our public asylums must be aware. Immense benefit would often accrue from appointing the wife of the medical superintendent to the office.

In France, the chief medical officer possesses paramount power, but is not resident. He passes a much longer period among the patients than the visiting physician does in England. His non-residence is however a very serious defect.

Generally speaking, the officers of European asylums are not so well remunerated as are those of the establishments in America.

3. *Site of European Asylums.*—Dr. Ray speaks with great and well-deserved admiration of the site of many of our county-asylums, and the tastefulness with which the grounds surrounding them are laid out. By far the most beautiful establishment, however, that he had the opportunity of visiting was, that of MM. Falret and Voison at Vanves, near Paris, situated in the midst of 100 acres, laid out in the finest style of landscape gardening. The Americans, notwithstanding their unlimited command of land, are far less liberal in their devotion of it to the purposes of asylums. While gladly acknowledging the admirable situations and external arrangements of our country asylums; we must not forget the pitiable state, in this respect, of various other establishments. We allude to private and public institutions situated in London and other towns, nearly, and in some cases totally, destitute of the means of affording the patient, not a fine prospect, for the circumstances of the locality may generally render that impossible, but even more room for exercise than a wild beast possesses in a menagerie. It is notorious that this is the case with several establishments, and it surely would be no injustice to compel those proprietors who are unable to furnish the requisite accommodation in this vitally important respect either to discontinue their occupation or to remove to more convenient localities.

4. *Construction.*—Dr. Ray admires the more recently constructed asylums in Britain, both for their convenience and appearance, especially those, such as the Surrey and Glasgow, which are erected in the Tudor-Gothic style. He considers that it is far better to extend the erections for the obtaining the requisite room than to raise them above two stories in height. The interior of the asylums he visited favourably contrasted with those in America in their greater amplitude, the width of the halls and corridors, the height of the ceilings, the spaciousness of the day-rooms, and in their light and cheerful aspect. The rooms are, with few exceptions, placed only on one side of the galleries, while in America the vicious plan of constructing them on both sides prevails.

5. *Sleeping-Rooms and Dormitories.*—The pauper sleeping-rooms in the

English asylums, Dr. Ray states, have a naked, cheerless, prison-like appearance; but he adds, "It would be a misplaced kindness to furnish poor patients with accommodations very much better than they ever knew before, and with which their own poverty-stricken abode, when they return to it, would present a painful contrast. Wisely, therefore, have cleanliness and suitable comfort been regarded as the only essential requisites in these rooms."

The directors of the different European establishments entertain very different opinions upon the expediency of large *associated dormitories*. In England, these seldom contain more than a dozen beds, and often a much less number; while, in France, they sometimes have 50 placed in them. A very much larger proportion of the patients sleep in dormitories in the latter country than in England. The dormitory system has been much disapproved of by some as calculated to allow of annoyance by noisy or filthy patients; but those who have most employed them maintain that one of their chief advantages consists in the greater quietude which many excited patients assume when placed in them. This testimony was afforded the author in France and Scotland, and at Siegburg. In America, where every patient has his room which he may call his own both by day and night, the general introduction of such dormitories would be impossible. Moreover, the much larger proportion of excited patients would prevent this. Still the author is convinced, that in certain cases they would prove very useful.

"There is a class of timid, nervous patients, who would be far more comfortable in an associated dormitory, especially when they first enter the institution. A poor, nervous female, startled by every sound, and apprehending every possible evil, who finds herself, perhaps for the first time for years, shut up in a room by herself, with no companion but her own agitated thoughts, unable to sleep, afraid to speak, is thrown into a state of disquietude, sometimes amounting to agony, not very favorable to recovery. Here the difficulty would be completely met by an associated dormitory. They are far better also for those suicidal cases which we now manage by having an attendant sleep in their rooms by the side of the bed. To be obliged to sleep on the floor in a room barely large enough for one, has no tendency to improve the humour of the attendant, and the consciousness of being specially watched is not likely to exert a salutary effect upon the mind of the patient. Nothing in the management of an asylum is a source of so much embarrassment as that class of patients in whom we suspect the existence of a suicidal disposition, but which is so feeble, or so successfully concealed, that we hesitate to place an attendant in their room at night, from the fear of unnecessarily exciting displeasure, and even suggesting suicidal thoughts where they did not previously exist. Thus, the patient goes unwatched, until he is found some morning ingeniously strangled by his pockethandkerchief, or a strip of his sheet. The liability of such deplorable events is much diminished by the use of dormitories. I do not mean to say that suicide would never occur in a dormitory, for the disposition is sometimes so strong that nothing short of unremitting vigilance can prevent it. What I do mean is, that it would be effectually prevented in that large class of the suicidal in which the propensity is not so strong as to lead to its gratification in the presence of others, whether asleep or awake. Many filthy patients, too, when tranquil, are, no doubt, better managed in dormitories, because the supervision there exercised is sufficient to improve their habits by exciting their self-control, and also secures that attention to their wants which they cannot receive so effectually when sleeping alone.

* * * * * * * * The dormitory itself, when tastefully

fitted up, presents a more cheerful and pleasing appearance than rows of small solitary rooms, which necessarily have a somewhat prison-like appearance. Few features in the foreign asylums left a more pleasing impression upon my mind than some of their dormitories. Those of the Northampton asylum were the very embodiment of good taste, neatness, and propriety—temples to the somniferous god in which it would seem he must be delighted to dwell. In walking over the Salpetrière I could not help stopping repeatedly to admire the *coup d'œil* which one obtained by standing in the door-way of one of its dormitories, and looking towards its opposite side."

6. *Day Rooms.*—Little or nothing has as yet been done, either in America or Europe, to relieve the nakedness of the walls of these by the suspension of maps, pictures, &c.; although they should be rendered as attractive as possible in order to induce the patients to resort to them, and prevent the baneful practice of remaining alone. The substitution of long, narrow, half-lighted galleries for cheerful day-rooms is a great error. The *windows* of the rooms in the British asylums too often have a very prison-like appearance. They are also superfluously guarded, and in a far greater degree than prevails in France. In Edinburgh and Glasgow a very convenient shutter is employed for the windows of excited patients, which, out of sight at other times, may be made very conveniently when required to cover any portion of the window desired. The *doors* of European asylums are also closed, and locked with far more clatter and noise than in America, owing to the ponderous nature of the instruments employed. The small tubular key described by Jacobi is in universal use in the States.

7. *Padded Rooms.*—These much excited the author's curiosity, as he felt convinced, from the description he had heard of their utility, either that insanity is a different thing in England from what it is in America, or that he had not been rightly informed of the nature of the cases in which they are employed. Subsequent enquiry confirmed these opinions. In England even, however, opinions are divided as to the utility of these chambers, but the author is disposed to believe they may be of much more service in this country than in America, where the violence in cases of excitement is so much greater.

8. *Warming and Ventilation.*—The author was much surprised not only at the want of system of ventilation in the English asylums, but at the little importance their directors seemed to attach to its absence. They, for the most part, seemed content to rely upon the simple expedient of opening the windows, an imperfect one at all times, and impracticable in cold and wet weather. The infected state of the large dormitories was generally complained of, and stated erroneously as one of their inherent defects. Dr. Ray is a great admirer of Dr. Reid's system of ventilation, but with our present experience of its inefficiency it is not very likely to be introduced into any new establishment in which the preservation of health is an object in view.

9. *Attendants.*—In America, owing to the ease with which more agreeable employment may be obtained elsewhere, attendants will not remain

more than a year or two in the establishments, while in England they are too often found therein after they have lost all their efficiency. In France, an excellent classification of the attendants prevails. One class performs the menial services, another acts as companions to the insane, and executes the orders of the remaining or superintending and responsible class, the organs of communication between the patients and the officers. It is obvious that much of the efficiency of the asylum depends upon the qualifications of the immediate attendants, and we fear the remuneration offered by most of our asylums is not sufficient to ensure these being of a high order.

10. "*Quiet of the European Asylums.*—With no feature of the foreign asylums was I so forcibly struck as the extraordinary quiet of the patients, as contrasted with the uneasiness and agitation of ours. Not a single instance of vociferation did I witness, and cries, and shrieks, and shouting, I rarely heard. That peculiar kind of vituperation joined with inexhaustible volubility, so familiar to those who have had charge of the insane on this side of the Atlantic, seemed to be unknown there; and even in the refractory wards, instead of the agitation and disorderly movements that characterise that class with us, there was a degree of stillness and quiet that would lead one to suspect, at first thought, that he was among the tranquil or convalescent. A visitor in passing through one of our asylums is besieged by persons who fill his ears with the bitterest complaints, representing themselves as the victims of the grossest injustice, and importuning him to procure their discharge. He is almost disposed to believe that there is really something wrong; that such feelings must have some other foundation than mere fancy; and he needs a hint or two on the subject, before he is made aware, that these persons believing themselves to be perfectly sane, cannot help regarding the deprivation of their liberty as an act of high-handed injustice, and the allegation of insanity as a bitter insult added to the injury. Twice only, once in England, and once in France, was I importuned for my assistance in 'getting them out.' I was allowed to pass along, seldom addressed, and exciting a look only of the faintest curiosity. The causes of such a singular phenomenon, I was naturally led to investigate, and, although I may have overlooked some of them, yet I believe, that the principal ones met my attention."

We wish we were able to extract Dr. Ray's exposition of these causes, which is a very interesting one, but our limits forbid us to do more than enumerate them. 1. The proportion of old, incurable, and therefore quieter, cases is far greater in the British and French Asylums than in the American. 2. In these countries, too, insanity seems to far less frequently assume the form of intense and uncontrollable excitement. Common as such cases are in America, Dr. R. did not meet with one in Europe. "The continuance of furious, maniacal excitement for months together, which is so common a circumstance with us, is seldom witnessed in England or France; and nothing which I communicated concerning the disease in this country, excited so much surprise and interest as this trait." Dr. Hutchinson of Glasgow, expressed his belief that cases of intense excitement were much more frequent some years since than at present. 3. The physical condition of the patients in the British Asylums is much less vigorous than is that of the Americans. Want of food is not unfrequently described among the causes of the disease in the Reports. 3. The peculiar moral and social condition of the mass of British patients, they being in a state of pauperism, in which they are but too contented with the, to

them, luxurious arrangements of an asylum. The American, on the contrary, has been cut short in the course of some hopeful career. 5. The manners existing in European society, and which inculcate so deferential a submission upon the part of the humbler orders, continue to exert their effect upon them with almost the force of an instinct, even when under the influence of their calamities. 6. There is a much smaller proportion of cases of moral insanity, (in which there is a complete perversion of some of the moral sentiments and affections,) in the European than in the American hospitals, where they exist in the proportion of 6 in 50, and become *foci* of discontent and confusion among those in whose company they may chance to be placed.

11. *Restraint and non-restraint.*—The subject of the disuse of restraint naturally excited much of the author's attention; but the information he was enabled to acquire concerning it was of a very conflicting and unsatisfactory character; and, as with regard to the padded rooms, he arrives at the conclusion that the non-restraint system has been regarded with undeserved favor in Britain, and that insanity is a very different thing here and in America. The directors of the various establishments furnished very discrepant opinions as to the possibility of entirely doing without restraint, especially in the violent description of cases seen in America, and for which none of the substitutes which he has here met with would prove effectual. We entirely agree with him that cases every now and then occur even in this country, for which restraint, not too long continued, affords the most humane as well as the most effectual mode of management. Dr. Ray is disposed to think a disproportionate share of attention has been given to this question.

"When we consider, on the one hand, how slight a thing restraint is in the European institutions, and how seldom it is applied at all; and, on the other, how many points there are in the management of the insane, involving their comfort and curability in a far greater degree, I cannot help concluding that this question has received a degree of attention altogether disproportionate to its intrinsic merits. I do not mean to sanction the idea that the imposition of restraint is an unimportant matter. On the contrary, I would have it regarded as, in most cases, a necessary evil, used only for the prevention of a greater. When insane manifestations are to be resisted, as they sometimes must, the precise form in which it is to be done, whether by mechanical restraint, by seclusion in a room, or by the hands, arms, or legs of attendants, is a question that must be determined by the circumstances of the individual case, not by theoretical considerations."

12. *Labour.*—This the author found actively pursued, and with unanimous testimony in its favour, in most of the European asylums he visited. The very different social position of the patients in America prior to the attack of insanity, already adverted to, prevents this engine of improvement being there so much resorted to. Their independent spirit quite rejects all idea of labouring gratuitously in an institution in which they are detained against their will and at their cost.

13. *Amusements and Recreations.*—The same amusements are observed in the European as in the American asylums. Reading, however, by reason of the frequent illiterate character of the patients, is much less

common; and even among the better classes of patients, books, and especially newspapers, are much seldomer seen. Walking and driving out into the country, so frequently practised in America, is seldom permitted in England, where paupers, as a general rule, never go outside the walls.

"There seems to be in the English community, an undue apprehension from the insane when at large. The safety of the lieges is thought to require their constant detention within the asylum walls, and the utmost jealousy is manifested of any attempt to relax the rigour of their confinement. One of the Superintendents informed me that on one occasion he permitted a few of his tranquil or convalescent patients, accompanied by attendants, to witness the exhibition of a circus or some dramatic entertainment. The next day to his surprise he was severely censured in the newspapers of the place, for unwarrantably trifling with the safety of the community, by letting loose a band of lunatics. The gratification experienced by these unfortunate fellow-men, and the safeguard provided against any harm were not thought worth taking into account."

The author also severely and justly comments upon the state of public opinion among the wealthier classes in England, which, regarding the occurrence of insanity among any of their members with morbid sensitiveness, as if it were a reflection upon themselves, induces the immuring of these unfortunate beings to a degree not only unrequired by their symptoms, but prejudicial to their recovery. He also observes upon the care with which such persons were removed from his observation when he visited asylums, however deliberately he might be allowed to examine into the condition of the pauper class. While protesting against any desire to exhibit these or any other unfortunate beings to the gaze of heartless curiosity, he is certain the almost monastic seclusion practised is no less injurious. Many of the Superintendents deplored this state of things, but stated that any attempt at its amendment would be followed by the removal of the patients from the establishment.

"It cannot be denied that in every asylum there are some patients who could be made happier and better by occasionally seeing and conversing with their more rational fellow-men. The only consideration that should influence our conduct in this matter should be the welfare of the patient, not the selfish feeling of his friends. Intercourse with the world should be regarded in the light of a moral remedy of no insignificant power, which it is as much our right and duty to use where it is evidently beneficial, as to withhold when there is reason to believe its effects would be pernicious. Its management should be left to the Superintendent, who is most able to form a correct opinion on the subject, and his decision ought not to be affected by the squeamishness of friends who would convert an asylum into a prison, nor by the vulgar curiosity of the public who would turn it into a show-house for the exhibition of the saddest infirmities of our nature."

13. *Schools.*—Dr. Ray did not see any schools in operation in the British Asylums, although some were said to exist. In France and America they seem to be in advance of us in this particular, especially in the former country. At the Surrey and Wakefield Asylums much pains have been taken in the instruction of *idiots*. Dr. Corsellis, of the latter institution has found that, although by perseverance many of their mischievous propensities may be cured, and some simple employment taught them, any attempt to excite intellectual exertion brings on paroxysms of ex-

citement or convulsions. This was not observed at the Bicêtre, where instruction of these unfortunate creatures is pursued on a large scale; and Dr. Ray attributes it to the fact of only the dangerous and diseased idiots being sent to the English asylums, while those of all kinds are sent to the Bicêtre.

14. *Medication.*—As the wonderful effect of enlightened moral treatment became exhibited in the English asylums, the active medical means, heretofore so much in vogue, lost their reputation. A great proportion of the cases in England too are chronic, and very often occur in persons whose supply of food has been defective and innutritious. In such, a supporting treatment is curative which would prove destructive in America; and a far more abundant use of malt liquors is allowable in the one country than in the other. In England, these are often found to subdue excitement, impart a healthy tone to the system, and prepare the way for convalescence beyond any other of the medicinal or dietetic substances more commonly used in America. Narcotics are given in Europe to some extent in order to procure sleep, but not, as in America, for the specific purpose of subduing mental and nervous excitement. For this purpose, the prolonged use of the warm bath or the cold douche is much relied upon in France; and, at Rouen, M. Parchappe employs the warm bath, with cold sponge lying on the forehead, in about sixty patients per diem. On account of its entire proscription in America, the author was much surprised to observe the frequency with which the use of *tobacco* is allowed to the patients in Europe; but he is disposed to believe it, under restrictions, a very proper indulgence.

15. *Religious Exercises.*—This is one of the most delicate of all the points in the moral management of the insane, and one in which well-intending officiousness may do an immensity of mischief.

"That part of the British public which takes any interest in asylums for the insane, is disposed to attach an undue importance to religious exercises in the moral treatment of the insane. They make the common mistake of supposing that, in mental as well as bodily disorder, the patient is equally able and willing to profit by the consolations of religion, while the truth is, that, generally speaking, the insane manifest far less docility than either those who are bound down with bodily infirmity, or those who are sound both in mind and body. *But the insane are sick, and the sick stand in need of religious advice and consolation*, and this logic has led, among other results, to the appointment of *resident chaplains*.

* * * * * * *

The practice of having a resident chaplain, however, was condemned by the medical officers, without any exception at all. It was thought that to render any essential service in their clerical character—to avoid doing harm indeed—there was required more practical knowledge of insanity, more knowledge of mankind in their sound as well as unsound condition, than the education and habits of clergymen enable them to obtain. * * * * * *

There is another objection to resident chaplains which ought to be conclusive, even were the others much lighter than they really are. It is so difficult to define their exact province in the work of restoring the disordered mind, that collision with the medical officers is not unlikely, nor has it in England been an unfrequent result. The latter may see all their efforts frustrated by what they deem the injudicious course pursued by the chaplain, who, however, cannot be

made to regard it in that light: and the consequence is that much ill-feeling is engendered, and a system of unpleasant relations established, productive of incalculable evil. When it so happens that the chaplain is a man of yielding nature, ready always to surrender his own views when conflicting with those of the medical officer, and always willing to make himself useful in any way that can be suggested, then he proves to him a valuable adjunct. But why risk the peace and harmony of an institution upon so uncertain a contingency as the chance of obtaining a person of this description—especially for an object of doubtful utility? It may be said that, if the Superintendent believes the course of the Chaplain to be injudicious, the latter can be replaced by a more suitable person. This is more easily said than done. Every one must be aware that the removal of an officer, however unsuitable, is, at best, a difficult and an unpleasant affair, and consequently, that much is usually suffered before it is undertaken. But leaving all speculative considerations, the success of the arrangement in Great Britain, where it has given rise to much ill-feeling and some scandalous scenes, has not been such as to recommend it very strongly to our favour."

The paper contains some interesting observations upon the disposal of *Criminal Lunatics*, and upon the *Size of Asylums* (Dr. Ray not approving of the very large ones in America), but our want of space prevents our adverting to them. As it is, we have noticed the Essay at greater length than we intended; but it is written in so fair and candid a spirit, that we thought it desirable to present our readers with a pretty full account of its contents as a *pendant* to the article on Insanity in our last Number.

THE SURGICAL, MECHANICAL, AND MEDICAL TREATMENT OF THE TEETH, &c. By *James Robinson*. Pp. xx. 320. London, 1846.

THERE is not, we fearlessly assert, in the whole range of medical practice, no, not even including all the absurdities, and still more, the wilful falsehoods, of Homœopathy, of Hydropathy, of quackery in all its forms, from the last edition of the advertising and puffing book of the regular physician, to the last advertisement of Morrison's pills, or Holloway's ointment, any one branch or element which offers more startling anomalies or more atrocious examples of lying empiricism than the present practice of Dental Surgery. Let any one who takes umbrage at this assertion, or who feels a consciousness, although even himself a Dentist, that he does not deserve to be included in this disgraceful category, and there are many such, look at the advertising colums of the Times or the Chronicle, the Satirist or the Age, or let him order from across the Atlantic the last or any other number of Stockton's Dental Intelligencer (we believe this is the title of the work), and we ask for no further apology for our unhesitating and contemptuous sentence. The present state of the profession (the trade we had better say) of the Dentist, is one of the foulest blots in the page of medical history. In a branch of medical and surgical practice, which ought to be associated with a good general knowledge of the principles of physiology and of pathology, which can only be successfully and

honorably followed by one whose professional education has been that of a physician and surgeon, whose information is only limited by the extent of the present improved teaching of our schools, and the general practice of our hospitals, we find that every Jew mechanic who, from want of character, or other not less disgraceful cause, fails in his trade—every charlatan who makes up for want of real knowledge of the profession by the most impudent pretension—every unhappy student who is plucked at the College or the Hall, considers himself fully competent to fleece the public in the character of a Dentist, and to practise, without knowledge sufficient to treat safely a whitlow or a cholic, a branch of the profession which includes as numerous and important a class of obscure sympathies and severe and even dangerous consequences, as those which are associated with any organ or system of organs in the human body.

Whence arises this shameful opprobrium? What is there essential to the practice of Dental Surgery that involves an anomaly so disgraceful, and so universal? Is there any one circumstance necessarily associated with this branch of the healing art, to which can in any way be traced this blight that hangs over it, and taints every one belonging to it with a portion of its evil? We believe that it is not difficult to discover its cause, in the universal association of the art of "*Dentistry*" (we rejoice in this vulgar Americanism as so appropriate to the case) with Dental Surgery. It is, we feel convinced, this and this alone, that has reduced Dental Surgery to the level of a mechanical trade, and never can it rise to take its legitimate rank with other branches of the profession of medicine and surgery, until this ill-sorted union is dissolved. It may perhaps be urged that it is impossible to sever the two. That there is not sufficient discrimination in the public mind to enable the mass of society to distinguish between the mere Dentist and the Dental Surgeon—that the association of the two has now become so universal that it would be a mere act of Quixotism in any individual to attempt the practice of one without the other. We have only to answer to this, that it has never yet been fairly tried, and, at all events, we feel that, until it is tried, no one has a right to complain of the position to which the professional dentist has reduced himself in the ranks of the profession.

We believe, however, that it may be done. An individual, perhaps, could do but little; and it would probably be ruinous to his practice, or at least temporarily injurious, for him to attempt it alone. But surely, if the educated members of the profession, those who have become recognized and authorized practitioners of surgery, by the acquisition of a diploma from either of the Colleges of Surgeons, or other similar body—the graduates, so to speak, in the profession—if these would but unite to throw off from themselves, as by one act, an opprobrium under which, we doubt not, many of them feel oppressed and degraded, the voice of the intelligent public would sanction the effort, and their confidence would reward an act of courage and honour so creditable to those who effected it, and so unspeakably beneficial to the public themselves.

This is not, however, the place, nor have we room, to enter at large into the details of any feasible plan for effecting so great an object. The hint may be of use—and heartily do we wish that any representation of ours could so rouse the respectable portion of this degraded branch of their

profession, to a sense of the degradation which they are themselves perpetuating by every act of tooth-making, as should make their disgrace too heavy for them to bear, and force them to cast it off as an unworthy appendage to the legitimate and honourable practice of their profession.

The above remarks will at once evince to our readers, that the book before us will find but little favour at our hands, considered as a professional work. Its whole plan and scope involves the very union which we have been deprecating; and the very title has the anomalous arrangement of the *mechanical* interposed between the surgical and the medical treatment of the teeth; and, as if this were not a sufficient indication of the prominence given to the manufacturing department, we find in addition the words, "including dental mechanics."

Our duty as honest reviewers will not allow of our taking a partial view of any professional work which comes before us in our official capacity. Private respect for persons may sometimes render that duty very painful; but in proportion to the difficulty of being impartial is its necessity increased. We proceed, therefore, to examine what is the situation which this work ought to occupy in the estimation of the profession, with the determination to "nothing extenuate nor set down aught in malice."

The first impression made on our mind on opening the book, is that the author, from some cause, not perhaps very difficult to be understood, is deeply enamoured of the American system of dental practice. As he identifies himself *in limine* with our excellent friends across the Atlantic, as he is an "Honorary Doctor (!) of Dental Surgery of the Baltimore College of Dental Surgeons," (see Title,) as he is, moreover, we believe, the accredited agent of a certain American periodical, before alluded to, we shall perhaps be excused if we consider this close association as some indication of the professional tendencies of our author. Now we are extremely willing to give all due credit to the American *Dentists*, as mere Dentists, as practitioners of what they so felicitously term "Dentistry"—a term which, by the way, Mr. Robinson has adopted and continually employs. We have been assured by those who know much of these matters, that they are extremely adroit at filling hollow teeth with gold (and, *par parenthèse*, it may be added that these are not the only hollows which they contrive to fill with the same precious metal), that they are clever at managing the arrangement of irregularities in the growth and position of teeth, and that the work which they exhibit in the construction of artificial teeth is something quite extraordinary: but what are their claims to be considered as SURGEONS—what their knowledge of the real principles of the treatment of disease—what their physiology, their pathology, their diagnosis of associated and sympathetic disorders? We intend, on some future occasion, to enter at length into the discussion of these questions, not only with respect to American "*Dentistry*," but with reference also to the existing state of this branch of the profession generally. At present we shall confine ourselves to such incidental examples as may come before us in our review of the present work; premising, however, that whatever may be our judgment as to the general state of the profession in America, we are willing to grant that there are some striking exceptions to the otherwise universal censure to which we sincerely believe them to be obnoxious, as mere mechanicians.

The title of the very first Chapter of the book before us, sufficiently exemplifies its real scope. It is entitled "The History of the Dental *Art*;" and the first words of this chapter are, "the origin of medicine, like that of many other *Arts*, is involved," &c. Now here is the fatal mistake which Dentists are very apt to commit. It is all very well for them to look upon their trade as a mere *art*—they have made it so, as a body, by the very mixture of practice which we have been deprecating:—but inasmuch as medicine is to be considered as something more than an art, so ought also dental surgery and dental medicine to be placed on the footing of a science, and its practice to be kept on a professional basis. We will, however, dwell no longer on this matter at present.

In the Chapter in question, we have an amusing digest of the ancient history of the treatment of the teeth, from the earliest ages; and the author has the following pertinent remark on the reasons which would naturally lead the Egyptians to take the lead in the supply of lost teeth by artificial means. "The significance of these organs,—to say nothing of their ornamental or useful functions,—was acknowledged in a remarkable manner by the ancient Egyptians, so that one of their most severe and infamous punishments consisted in the abstraction of a front tooth." "The loss of a front tooth, whether by disease or not, would naturally, under the circumstances of Egyptian law, give rise to unpleasant suspicion, and every exertion might be expected to be made to supply the deficiency. Accordingly, Belzoni and others have discovered artificial teeth in the Sarcophagi of the ancient Egyptians. These, it is true, are rudely made, and, from being of wood, are ill adapted for performing mastication, &c." (p. 8.)

This first Chapter closes with a comparison between the merits of English and American Dentists, a comparison no less invidious in its depreciation of the former than it is undeserved, if we exclude the numerous quacks and advertisers of our country from the list. Let us first see what he says of the English authors on this subject.

"About this period, the famous John Hunter turned his attention to the subject, and presented the world with his 'Natural History of the Teeth;' a production which, while it enlarged the sphere of dental knowledge, piqued the pride and roused the ambition of the English practitioners of the art.

"The inaugural Dissertation on the Structure of the Teeth of man and animals, published in 1798 by Robert Blake, gives evidence of the rapid strides that had been made in the anatomy and physiology of the teeth. This work was soon followed by others, and at the commencement of the nineteenth century, the surgeon-dentists of this country were fully entitled to rank with the practitioners of the other branches of surgery.

"The most important of the works of our own time are those of Fox, 1803, Bell, 1829, Nasmyth, 1839, Owen, 1840; also those of Snell, Waite, Robertson, Jobson, and Koecker; besides which, we might enumerate several smaller works by Saunders, Clendon, White, and others, and many valuable detached papers in transactions and periodical publications."—P. 13.

Now for the list of American authors whom Mr. Robinson arrays so triumphantly as evincing so great a superiority over those of his own country.

"Within the last century, dentistry has advanced far more rapidly in the United States than in any other country. Thus we have Gardette in 1821, Parmly

L. S. Parmly, and Flagg in 1822, Trenor, 1828, Fitch, 1829, Brown, 1833, Spooner, 1836, Goddard, 1843; and in 1845, Dr. Harris, one of the editors of the *American Journal and Library of Dental Science*, published a most able and comprehensive work, entitled the *Principles and Practice of Dental Surgery*. And many other productions on the subject have appeared in America, and especially in the periodical just alluded to."—P. 14.

Now really we could hardly help smiling at this extraordinary comparison; for we cannot but think that the list of English authors on "Dentistry," as here given by Mr. Robinson, from Hunter downwards, may possibly bear a comparison with his vaunted list of American writers, aye, even if we give them Mr. Robinson into the bargain.

The following farrago may be well quoted as the summing up of this comparative view of the merits of the practitioners of the two countries.

"Before concluding, we may be allowed a word respecting the present state of dental art and science. The conditions of success appear to be not different in this from what they are in other branches of knowledge and practice. They are all summed up in one phrase, UNITED LABOURS. Whatever of discrepancy there is in the works of our chief authorities, is greatly owing to the isolation in which they studied, and to the want of a general means of collating their ideas. Again, whatever of progress we find in that country which takes the lead in the dental art, appears to be due to an absence of prejudice and jealousy which allows free communication of ideas, and association of common interests, among the members of the profession. For the association of dentists in America has not only given its members generally a *status* in society unknown to dentists elsewhere,—has not only repressed those characters who intrude themselves upon the public here, and given merit its station and honesty its preëminence,—but has also contributed largely to the advanced state in which dental science stands in the United States. It is painful to think that we do not yet possess the same advantages in England. The names of Harris, Brown, Parmly, Maynard, Greenwood, Goddard and Haydon, shine high over our heads in these respects, and present us with bright examples of brotherly good feeling, scientific excellence, and practical success."—P. 16.

We fear that few of our readers have had the advantage of perusing the authorized medium of communicating information on "Dentism," (we use another of Mr. Robinson's Americanisms), and we must therefore ask them to take our word when we assure them that, as far as we can judge from that and other sources, we are patriotic enough to believe that we could select a body of practitioners of Dental Surgery in this metropolis, every one a member of the Royal College of Surgeons of England, who need not fear a comparison with those of any other country, no, not even of America, in knowledge of their profession, in the education, manners and feelings of gentlemen, in their "status in society," and in every other quality, which ought to distinguish the professional man or the gentleman.

Let us assure Mr. Robinson that we are sincerely pained at being forced upon this comparison; the attack is his own, and he must not feel hurt at our defence.

We proceed with our analysis. In the course of some very useful and judicious remarks on the diseases consequent on the irritation produced by the first dentition—and most of which, by the way, we seem to recollect to have met with in substance before—we have a very proper and rational denunciation of the common administration of opiates by nurses

and mothers, concluding with this undoubted and important truth. "A teaspoonful of castor-oil will commonly be a far more efficient *opiate* than all the Godfrey's Cordial and soothing Syrups that were ever invented."

In the observations on the "progress of the second or permanent teeth," we find nothing new worth transcribing: and the same may be said of the chapter on the second or permanent teeth, and that on the "Comparative View of the Teeth of Animals;" but the writer's views on the mechanical treatment of irregularity require some notice.* The general impression left by the perusal of this portion of the work is, that the author's prevailing tendency to the mere mechanical art of dental practice, has led him to depend more than is proper upon such means for remedying these cases. He appears to us to consider far too little the mischief which so often results from the application of mechanical force to growing teeth, and even to those which are already perfectly formed. At the same time we think that there are many practical suggestions which are likely, with proper caution, to be useful to the practitioner. He very properly condemns the common practice of extracting the temporary teeth "before the second are sufficiently developed to take their place." On this subject he relates a case, which does not appear essentially to differ from scores of almost daily occurrence; the only peculiarity as far as we can see, being that the patient was "the son of a nobleman." (By the bye, what on earth does Mr. Robinson mean by "the pale of civilized dentism?")

On the subject of remedying irregularity arising from crowding of the teeth in the jaw from the arch being too narrow for them, a somewhat complicate apparatus is recommended (pp. 64-65), which appears to us to deserve a trial. There is, however, an obvious objection to Mr. Robinson's plan, and that is its being applied at as early an age as from the 9th to the 12th year. Not only is there great danger of loosening the teeth from their being moved at so early a period, but we should fear that the proper formation of the dentine would be interfered with, by the action of any considerable pressure on the growing teeth. Did Mr. Robinson ever examine the pulp cavity of a permanent tooth at this age?

We next pass on to the immobility of the jaw, arising from contraction of the elevators of the lower jaw (we suppose this is what our author means); and we find this symptom, whether arising "from the cutting of the wisdom teeth, from the use of mercury, from hydrophobia, or *some* other cause (!)" all considered as "tetanus or lock-jaw!" The instrument recommended for forcing open the mouth in such cases is nothing more than the *speculum oris*, which is employed at every hospital, and sold by every instrument-maker.

On the subject of the "colour of the teeth as a test of consumption, &c.," our author seemes to consider that, if people's teeth were examined, they would present such appearances, as would often lead to a plan of treatment by which phthisis would be prevented or cured. He appears to take great credit for his discovery of these new opinions; but we do not hesitate to say, that the effect of a scrofulous constitution on

* The following juxta-position of names strikes us as being funny. "Some curious anomalies of the teeth are related by *Pliny* and *Dr. Pritchard*."—Note, p. 36.

the appearance of the teeth has been known to every writer of consequence on the teeth, from Hunter to the present time; and many of the details which Mr. Robinson adduces, have, we are convinced, no existence but in his own imagination.—See particularly the coloured engraving at p. 78.

The subject of "the teeth considered as a test of age," is one of great importance, or, rather we might say, it would be so, were such indications to be depended on to any extent. We have ourselves seen so many examples of abnormal periods of the second dentition, that we entirely agree with Mr. Robinson, in considering such a test as absolutely fallacious. We have cases within our recollection, in which the teeth were nearly in the same state of progress at 5 and at 8, at 9 and at 13.

We come however to the *vexata questio*, the cause of caries. Here our author acknowledges himself at fault; and, after giving what he calls the opinions of about a dozen of writers on the subject, gives his verdict, without stating for what reason, for that originated by Parmly, and "maintained by Dr. Harris in his last work," (p. 88.) Now we beg to say, that the statements of Hunter, of Fox, of Bell, and of Saunders, are identical; and that the others agree only in attributing this disease to external causes alone. In other words, that the disease is attributed by the first-named writers to changes in living structure, in the others, to changes effected in the inorganic substance by which that structure is covered and protected. Mr. Robinson says,—"And now, having cited the opinions of others, I shall perhaps be expected to register my own. The field of speculation, however, is well enough occupied without it; and moreover, any view would be practically worthless, unless it enabled us to foresee the disorganization of the teeth, which we cannot do at present. I will nevertheless commit myself so far as to observe, that the nearest approach to truth appears to me to be the chemical theory of Parmly, put forth in the year 1820, and maintained by Dr. C. A. Harris in his last work." We are therefore at once enabled to judge what are the opinions of the author, of his Magnus Apollo Dr. Harris, and of the originator of the theory Dr. Parmly; and the passage to which we are referred as containing the enumeration of the decisive theory is the following precious sentence:—

"*Parmly* (1820): "The premature decay of the teeth, is the consequence of uncleanliness, which acts upon them in the same manner as on other parts, by sapping and corroding the vital energy, and thereby causing them to moulder away."—P. 87.

Now we have no hesitation in saying that, if any one physiologist of the two hemispheres were asked the real simple meaning of this passage, he would acknowledge, and with a smile too, that he knows nothing at all about it. And this is the physiology on which American "Dentistry" is founded! Well may our author exclaim, "ne sutor ultra crepidam."

The class of persons for whom this work is written may be easily ascertained by the GLOSSARY which the author has thought it necessary to append to it. The work purports to treat on the *surgical* and *medical* treatment of the teeth. What sort of surgeons must they be who require an explanatory *glossary* of such terms as ABSORBED, ALKALINE, ELVEOLAR PROCESSES, CHRONIC, DIAGNOSIS, FEBRILE, MAXILLARY BONES (!) MUSCLE, &c., &c.?

But we feel that we are bestowing more space on this work than it deserves. It were easy to comment with painful severity on almost every page. The whole affair is obviously an attempt at making a book for an especial purpose. We had hoped that this was not the case when we commenced this review, but it is so painfully forced upon us that it is impossible any longer to conceal or to apologize for it.

The only portions of the book which are really free from the general censure, appear to be those which relate to purely mechanical matters, and with these, *we* have nothing to do. The Dentist may, and we doubt not will, find some useful hints on these subjects, but the result of our examination of the work, as far as it has respect to true dental surgery, is, that it is calculated only to perpetuate the unhallowed union which we have been deprecating, and to endeavour (it will, however, be fruitless) to exalt a mechanical art into the rank of a profession. Once more we call upon the educated *Surgeon*-Dentists of this country at least, to repudiate the mere mechanism of "Dentistry," to render themselves independent of the art of tooth-making, and to show that there is a distinction between the scientific surgeon and the mere mechanic who, by administering to the vanity of a faded coquette, or even by subserving the better object of assisting impaired mastication, and thus becoming the humble adjuvant to the surgeon and physician, ought to be, and *must* be placed in a lower and a distinct class of professional society.

COMMENTARY ON THE HINDU SYSTEM OF MEDICINE. By *T. A. Wise*, M.D. &c., Bengal Medical Service. 8vo. pp. 431. Calcutta, 1845.

FROM an early period of Dr. Wise's residence in Bengal, he has been in the habit of employing his leisure time in studying the history of medical science in different ages and nations of the earth. His attention was naturally directed, in an especial manner, to the examination of the Hindu Shastras or ancient records of medicine in the East. The present volume is the result of his researches upon this subject. It is only of late years that the European student has been even so much as aware that there were any writings, at all, on medicine among the Hindoos. Sir William Jones, the celebrated Oriental scholar, had asserted "that there is no evidence that, in any language of Asia, there exists one original treatise on medicine as a science;" and Mr. Mills, the learned historian of India, has made a similar statement. That such a belief is, however, far from being correct, has been abundantly shown by the recent writings of Professor Wilson, and of Drs. Heyne, Ainslie and Boyle; and now the work of our author presents us with an abridgement of an almost entire system of medical science, if science it may be called, extracted from the Shastras.

While the greater part of Europe was in a state of savage ignorance, there is unquestionable evidence to prove that the inhabitants of Hindostan and of the surrounding countries had attained to high civilization, and had

made great progress in most of the arts and sciences of life. Medicine was one of these, which seem to have been studied with much care and diligence. The most ancient medical work, which is extant (partially so, at least), is called the Ayur Veda, and is said to have been given by Brahma himself to the world, in compassion to man's ignorance and suffering. This sacred work contains, among other subjects, a description of the structure of the human body derived from anatomical examination; for there is abundant evidence to show that dissection of the human body was practised in these early times.

It is unnecessary for us even to give the names of the other medical books, as the information cannot be of interest to the majority of our readers; those who wish to know particulars, cannot have recourse to a better source than our author's work.

The medical caste, *par excellence*, among the Hindoos is known by the name of Vaidhyas, *i. e.* those who understand the "Ayur-ved" or medical shastre. The Vaidhyas are the proper teachers or professors of the art. Many of the precepts, which they inculcate, would do no discredit to our most learned doctors. For example, the importance to the physician of combining practical experience with attentive study of books is thus very emphatically enjoined:—

"Without a knowledge of books he will be confused, like a soldier afraid in the time of action, will be a great sinner, and should be capitally punished by the rajah. On the other hand, a want of practical knowledge will impede his advancement, and his senses will be bewildered, when called on to treat acute diseases. Such a physician will not be esteemed by the great, as he cannot practise with success when only instructed in half his duty. Such a person is the murderer of his species, and the medicine prescribed by him may be compared to poison, or lightning—such ignorance prevents all the good effect of remedies. As the two wheels of a chariot, or the two wings of a bird, assist in their progress, so will the knowledge of the shastres, and of practice, lead the physician to proceed with safety and success in the treatment of the diseased; but, should the physician want either of these essential qualifications, his progress will be impeded, as one wing or one wheel will impede the progress of the bird, or the chariot. It is the combination of both these qualifications which is required; when medicine becomes like the water of immortality (*Amrita*). Such a physician, if he is to acquire celebrity, must still daily endeavour to improve his mind by an attentive perusal of scientific books."—P. 18.

Again, is there not sound advice, alike to patient and doctor, in the following remarks?

"Some severe diseases are cured immediately, by a good physician; but simple diseases are increased much by the want of early assistance. At the commencement, like a young plant, it is readily rooted up; but, as it expands and grows in strength, the difficulties are much increased. Even for a slight disease, the assistance of a practitioner will be of much use; for, as a large man at the bottom of a pit may get out by long-continued exertion, his extrication will be much facilitated by the assistance of a friendly hand. As in war, a sword may defend many, so in the hand of an enemy it will destroy. In like manner, the shastres and water may become the cause of destruction instead of benefit to mankind." P. 19.

Mercury, we suppose, is regarded as the greatest of all remedies; for we are told that the physician, who knows the value of this metal, is like a god; he, who knows the qualities of herbs and roots, is like a man; he,

who knows the use of knife and of fire, resembles a demon; and he, who knows the proper prayers to be offered up in the time of sickness, is like a prophet. A bad or ignorant physician may indeed sometimes cure a patient, but he does not consider the thousands he has killed; such a one is like a boat in a storm without a pilot, or a blind man in the performance of any work, and is to be looked on as an angel of death.

Some of the duties inculcated upon a physician are certainly rather strange. For example, he is directed never to look at the rising of the setting sun; he should not eat or drink out of a broken vessel, nor sleep with his face to the earth! As a matter of course, too, great respect is to be paid to various omens. Thus, we read that, "before a physician visits a patient, he should first remark the position he is in when the messenger arrives to consult with him; and by the person's countenance and conversation, endeavour to ascertain whether or not the patient will survive. As he proceeds to visit the sick person, he must carefully note any good or bad omens that may occur regarding the messenger, the flight of birds, the relative position of animals, &c. Seeing cows, or Brahmans on the right-hand side are favourable; as also corpses, jackals, vessels of water, &c., when seen on the left side. It is unfavourable when lizards are heard upon leaving the house, when vultures or bad characters are seen, or when the Physician is called by another person, or is hit by anything behind, or when a person sneezes.

In the examination of patients, most minute attention is directed to be paid to the state of the various excretions, among other signs or indications of disease. "Unless the disease," it is sagaciously remarked, "is well explained, seen and known, the practitioner will not understand it, and will be made foolish by his ignorance; whereas the knowledge and judgment of the physician, like a lamp which illuminates a room, enable him to understand the nature of the disease of the body."

There is an odd mixture of truth and error, of sound morality and wicked folly, in the following description of the *Recompence of the Physician.*

"When a physician has cured a disease, he is entitled to the usual gifts for the performance of a good action. These will vary with the rank and condition of the patient. Money will be the recompense bestowed by the rich; friendship, reputation, increase of virtue, prayers, and gratitude will be that of the poor. When a Guru, a Brahman, or a Dandi, a relative, a humble and good friend, or one without relations consults a physician, he must not accept of any pecuniary recompense: his reward in such cases will be an increase of knowledge, and the gratification of his desires in having an opportunity of performing a good action. His cures will ensure the admiration, and the esteem of all men; he will be honored and respected as a master, and after death he will go to heaven. Should the patient prove ungrateful after being cured, his holiness and good fortune will pass to the physician. But the physician must avoid administering remedies, to hunters or great sinners. Such people do not deserve his assistance." P. 29.

This and the preceding extracts are taken from Book I, on the History of Medicine, the character and duties of physicians and their pupils, &c.

The Second Book professes to treat of Anatomy and Physiology, and to describe the elements of the body; its generation and growth; the struc-

ture of its various parts; its spiritual elements; as well as the different temperaments, death, &c.

The influence of *longing* on pregnant women is very particularly insisted upon:

"During Pregnancy, if the woman is not gratified with what she wishes to eat, and the air is deranged, the child will be crooked and cripple, or will be small in size, dumb, or cannot speak distinctly; will be blind, or have his eyes defective, or will be an unbeliever in the sacred shastres. In other such cases these defects are produced by acts of previous wickedness of his own, or of his parents in a former state of existence.

"Whatever is wanted by the pregnant woman should therefore be supplied, when a perfect child will be born. If the woman desires to see a Rajah, the child will be great and rich; should the mother wish to adorn herself, the child will be well formed and vain; should the mother wish to see a holy man, the child will be holy and just, and if she longs to see ferocious animals, the offspring will be of that description. In like manner, the desire for particular sorts of food indicates the disposition of the infant, and the form of his body. When the mother wishes to eat buffalo's flesh the child will have blood-shot eyes, much hair, and he will be warlike; and when hog's flesh, he will be sluggish and sleepy."—P. 35.

The Chapter on Death contains some really fine and very just reflections:

"Death is always near; and when it occurs, nothing but the sins and virtuous actions, which have been performed, accompany the soul.* 'When a person leaves his corpse, like a log or a lump of clay, on the ground, his kindred retire with averted faces; but his virtue accompanies his soul. Continually, therefore, let him collect virtue, for the sake of securing an inseparable companion with which he may traverse a gloom, how hard to be traversed! For, in his passage to the next world, neither his father nor his mother, nor his wife, nor his son, nor his kinsmen, will remain in his company: his virtue alone will adhere to him. Single is each man born; single he dies; single he receives the reward of his good, and single the punishment of his evil deeds.'"†—P. 81.

The immortality of the soul is frequently and emphatically dwelt upon; as in the following passage:

"After death, the body is like a house without a tenant; and the five elements slowly separate and join their like; the atoms of earth join the earth, the watery mix with water, &c. Death is therefore called in Sanscrit *Panchatwa*, or separation and passage to the five elements. To promote this separation of the elements after death, which would be defiled if buried, and to purify them in their passage from the body, so as to enable the earth, air, fire, water, and ether, of which the body is composed, to join the mass of the same elements which compose the world, the bodies of Hindus are burnt. 'What then dies? not the body, for it only changes its form; and certainly not the soul! Why then regret the death of relations and friends, if they have passed through life with pro-

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* "A mansion infested by age and by sorrow, the seat of maladies, harassed with pains, haunted with the qualities of darkness, and incapable of standing long; such a mansion of the vital soul let its occupier always cheerfully quit."—Menu, Ch. vi, § 77, p. 183.

† "Menu, Chap. IV, § 239, 240, 241, 243. The same idea is thus expressed in another Sanscrit work. "The wise man meditates on the acquisition of knowledge and riches, as if not subject to sickness or death; and cultivates virtue, as if death had already seized him by the hair." (Hitopadesha.)

priety! such grief is indeed natural, for it is universal, but it is the offspring of our ignorance and our selfishness."—P. 81.

The Pythagorean doctrine, however, of *metempsychosis* is taught in the subsequent part of this chapter.

The Third Book is devoted to the various subjects that come under the general appellation of Therapeutics, the first department of which is hygienic regimen. All Hindoos anoint their bodies daily with oil: a practice that is considered to be conducive not only to the preservation of health, but also to prosperity and good fortune. It promotes an agreeable perspiration over the whole surface, and guards the system against the injurious effects of vicissitudes of temperature. The Orientals are also much in the habit of using baths of various kinds.

"Independent persons, such as rich merchants, bankers, talukdars and others, generally bathe at 10 or 11 o'clock, and after performing these ceremonies they breakfast. Shop-keepers, day-labourers, &c. eat at 10 o'clock, a handful of rice, which has been moistened for half an hour in water or a handful of gram moistened, and do not generally bathe till after 12, 2, or even 3 o'clock. There are some who bathe twice or thrice a day, but they are few in number. This description applies to the male inhabitants of towns. The higher class of females seldom bathe in rivers, but do so in tanks, in their respective gardens; or in warm water, between 10 and 11 o'clock. Widows of the lower class are not strict, and do not observe the rules of the Shástras regarding bathing.*"—P. 96.

Vapour baths are a favourite remedy for a variety of painful complaints. The patient is first well rubbed with oil, and is then seated over a pot of boiling water, with a covering thrown over both. Sometimes medicinal plants are put into the water.

We cordially say Amen to the soundness of the following remarks upon a most useful, though too often neglected, part of therapeutic treatment:

"The Hindu medical writers usually commence the cure of a disease by arranging the diet that is to be followed by the sick person. So much do the Hindu Physicians rely upon diet that they declare that most diseases may be cured by following carefully dietetic rules;† and if a patient does not attend to his diet, a hundred good medicines will not remove the disease. The generality of diseases being supposed to be produced by derangement of the humours, if one or more are morbidly increased in quantity, their indications of cure are commenced by promoting the just balance of the elements and humours, by a judicious choice of aliments, and by such means as assist the vital principle on the completion of the assimilation. On this account they have not only been careful in describing the regimen, but also the food and drink for the different seasons, and even the vessels in which they should be kept."—P. 98.

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* "The Hindu men and women may be seen proceeding towards the sacred Ganges in the cool of the day, the one with his small copper lota, the other with her antique earthen pot perched upon her head, amidst the rustling of the beautiful palm trees, which almost hide the graceful cupolas of the neighbouring temples. After washing their heads with some of the mud and water of the Ganges, they proceed to clean their teeth with the branch of a tree which they have brought with them. They then wash their bodies, using mud for soap, fill their vessels with water, and return home."

† "Or as Baglivi expresses the same opinion, as the heading of one of his chapters 'de ciborum delectu, sive de methodo curandi morbos quamplures per opportunum ciborum genus, sine ope remediorum.—Op. omnia, T. 11, p. 530."

From the chapter on Surgery, we shall select the description given of the Rhinoplastic operation performed by the Hindoos.

"*When the nose is cut off, or destroyed by diseases.*—The former is a frequent punishment in the native courts. A fresh leaf is first cut of exactly the size of the nose, it is then to be placed upon the cheek, and the necessary quantity of skin and cellular membrane is to be dissected. The nose is then to be scarified, and after dissecting up the flap, it is to be placed upon the raw part of the nose to which it will adhere. Sutures and bandages are applied to keep the parts together. After the bandage has been applied, a couple of wooden canulæ are to be introduced into the nostril to allow breathing, and to support the new nose. A piece of linen cloth previously soaked in oil is to be applied over the bandage. An aperient is then to be given to the patient, and his general health is to be attended to. Should any other deficiency of the nose be present it may be supplied in the usual manner. If the nose should be deformed it may be reduced in size by the knife."—P. 189.

Book IV. treats of medical diseases generally and especially. The first subject that we shall notice is that of Small-pox, in order that we may have the opportunity of introducing the following remarks of our author respecting the primary seat or *habitat* of this destructive pestilence.

"It appears that the Chinese as well as the Hindus were familiar with *small-pox* many centuries before the Arabian physicians described it. It was probably conveyed westward by the Persian conquerors of Hindustan; which seems to be a further confirmation of the country from which it originally came, and the manner in which it gradually approached and eventually reached Europe. The distance and the hot deserts through which the only intercourse for so long a period was held, prevented for a time its progress westward; but, as navigation extended, ships from India would frequently touch at the Arabian ports of the Persian Gulph, and Red Sea, where it seems first to have appeared, A.D. 900.

"The description of the small-pox by Rhazes, the distinguished Arabian physician, first drew the attention of the European physicians to the disease.

"Some say it was introduced into Arabia in 572, the year that gave birth to Mohammed; other testimonies seem to accord with the statement that it was at the siege of Mecca (A.D. 569), by Abraham that the Arabians were first affected with the disease.

"The conquest of the followers of Mohammed conveyed the disease to Persia, Syria, and Egypt; and the successful stand made by the inhabitants of Constantinople, for some time prevented the spread of the disease beyond the Hellespont. So completely does this appear to have been the case, that Honus, a resident physician in that city in the tenth century, states that neither the small-pox nor measles were known in his time in Constantinople.

"The whole of the southern coast of the Mediterranean sea had been subdued by the Arabians; but, it was not till the commencement of the eighth century that the disease was introduced into Spain by the Moors. The victorious Saracens overran Spain, crossed the Pyrenese mountains, and inundated the southern provinces of France. They were driven back by Charles Martel; but they left the small-pox and measles with the conquerors. From this source the diseases quickly spread over Europe.

"The Spaniards in their invasion of Hispaniola and Mexico conveyed the same disease to these countries, where it committed the most extensive ravages. It would thus appear that the small-pox as well as the measles commenced in Asia, and extended to Africa, Europe, and the new World."—P. 239.

Leprosy.—This loathsome malady is evidently regarded by the Hindoos

in nearly the same light in which it was by the Jews of old, as a mark of special Divine chastisement, and as a disease to be cured rather by confession of sins, prayer, and sacrifices, than by any medicinal remedies. "Lepers in this life are supposed to be born again in the next with the disease upon them; and it is believed to be communicable by contact, by breathing the same air, by eating together, by wearing the clothes, or ornaments of a person labouring under the disease. Whoever speaks disrespectfully or does any other improper action, or commits sins against his *guru* or *bipra* (pandit brahman); such as committing adultery with a brahman's wife, killing a good man, and robbing a person of his estate, will be liable to be afflicted with this disease."—P. 259.

Seven varieties of the disease are described in the Hindoo medical writings. The eruption is either in the form of small, white, coppery or red spots, spreading over large surfaces, and covered with a thin mealy dust, (Lepra vulgaris, or L. alphos); or the blotches are livid, resembling a ripe fig, and are usually accompanied with much pain and burning on the surface; or there are patches of tubercles, which are hard and red round their edges, dark in the centre, and attended with severe pain; or the patches are purplish and black, irregular, hard, dry, pricking and painful; or they resemble the seed of the *kunch* (Abrus precatorius), with red and black spots in the centre, and often terminate in suppuration; or they are black, round, and spreading, often accompanied with much itching, burning and pain. It would seem that under the term Leprosy, a variety of squamous and tubercular diseases, attended with a cachectic state of the system, are comprehended. In the bad forms of the disease, many of the skin-patches become the seat of foul ulcerations, which discharge an offensive ichorous matter. Sometimes the hands and feet become stiff, immoveable, and drop off with severe pain; the nose falls in; worms breed in the sores; the voice becomes hoarse and unnatural; and the patient at length dies in a state of wretched emaciation and exhaustion. "Leprosy," we are told, "commences first in the skin, and gradually extends deeper and deeper affecting the different essential parts, as flesh, blood, fat, &c. Thus, like the small shoots of the Banian tree, which are at first confined to the surface, they advance deeper and deeper, until they extend over the whole body. In the first stage, when it is superficial, the use of proper diet and medicines may cure it; but when it has extended to a greater distance, the difficulty of curing it becomes much greater."

Demoniac Possession is recognised as a distinct disease by Hindoo physicians. It is, on the whole, interesting to read the accounts, however absurd many of the details are, of different kinds of evil spirits that are believed in the East to enter the bodies of men; as we are forcibly reminded, by the perusal, of the analogy between Hindoo belief and many of the statements in Holy Writ, respecting this source of suffering and disease.

The severe form of periodic *Gastrodynia*, described by Dr. Wise, as occurring among the natives of India, appears to us to be of the same nature (though in a much more aggravated degree) as what is not unfrequently met with in this country, in certain kinds of Dyspepsia connected with an atonic condition of the stomach. The pain usually comes on from one to three hours after taking food; it commences with a sense of dull gnawing uneasiness, and goes on increasing until it becomes most distressing and

severe. Firm pressure on the epigastric region generally relieves it partially. Eating too, especially solids, mitigates the suffering for a time; and hence the patient is induced—not indeed by the feeling of hunger, but from having found a previous relief in this way—to the very frequently having recourse to food. Persons who have exhausted their strength by excessive mental application, or who have been in the habit of going very long without food, are those who, according to our experience, have generally been the victims of this form of Gastrodynia. The use of warm stomachics and antispasmodics is required at first;* and subsequently chalybeates must be given, in conjunction with bitter infusions. The food should consist chiefly of meat and bread; the drink of brandy and water, or well-brewed porter or bitter ale. Tea must be eschewed; coffee is much better.

There is obviously a good deal of sound rational practice in the following account of the treatment to be pursued in cases of *Intestinal Worms*.

"The treatment is to commence with the administration of an emetic prepared with ghee and *Sarasádi*, which is a mixture formed of various stimulants and bitter remedies. Strong cathartics are then to be given with clysters. The oil of *Biranga*, made of vermifuge seeds, is to be given with salt, and with the decoction of *Biranga*. The patient should at the same time be careful to avoid such food, and other causes which promote the generation of worms. He should take of the juice or paste of the *Pálása* seeds (Butea frondosa), with the decoction of the *Balanga* seeds. The juice of the leaves of *Paribadraka* (Erythrina fulgens), with honey; or the juice of the leaves of *Patura* (Salincha B.) or the powder of *Biranga* with honey. *Biranga* powder should be mixed with the bread and used by the patient. Different preparations of iron, or the powdered root of long-pepper taken with goat's urine are of use. Tin is recommended to be rubbed upon a rough stone, and the small particles thus removed from the mass is taken in whey."—P. 350.

On the subject of *Syphilis*, we are informed that, soon after the appearance of the disease in Europe, its ravages extended over a considerable part of Asia and Africa, as well as Europe. In the East it is known by the name, among others, of the "disease of foreigners or strangers." Old Hindoo medical writers have described various maladies of the genital organs; but it is obvious that they were not acquainted with the venereal disease; for there is no word for it in the Sanscrit language. In the modern Sanscrit works, it is called "Faringi Roga," or Portuguese disease. The general belief among the native doctors is that mercury is the only remedy for it. From a variety of facts and considerations, our author comes to the conclusion that Syphilis "was introduced from America to Europe and Asia as the small-pox and measles seem to have first appeared in Asia, from whence they spread, and committed such ravages in Africa, Europe, and America."

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* Nothing is usually better than the following mixture:—

℞. Magnesiæ ustæ	ʒ ss.	
Aquæ Menth. Piperit . . .	℥ v.	
Tinct. Rhei Comp.	ʒ vj.	
—— Hyosciami	ʒ j.	
Æther. Sulphurici	ʒ j.	M.

Capt. Cochl. magna omni horâ, urgente dolore.

In taking leave of Dr. Wise, we cannot deprive ourselves of the pleasure of expressing our high admiration of the learning and indefatigable zeal which he has displayed in the composition of the present volume. By many it will be regarded rather as curious than useful; but all have not the same tastes. The medical historian and antiquary will be pleased to have an opportunity of comparing the state of their science in one country and in one age with that in another. We have been reminded, during the perusal of this commentary, of the writings of Paulus Ægineta in not a few respects. Dr. Wise, we are glad to find, promises us another volume, in which he proposes "to trace the decline of the medical profession in India, and the best means of removing the state of ignorance which now prevails over the whole of Hindostan."

I. A MANUAL OF OPERATIVE SURGERY, BASED ON NORMAL AND PATHOLOGICAL ANATOMY. By *J. F. Malgaigne*. Translated from the French by *F. Brittan*, M.D. Small 8vo, pp. 580. London, 1846.

II. PRACTICAL SURGERY. By *Robert Liston*. Fourth Edition. Octavo, pp. 580. London, 1846.

THE publication of the above works furnishes us with an opportunity of drawing our reader's attention to a most important subject, viz. the deficiency of the provisions for teaching Operative Surgery in this country. Not that we feel at all disposed to admit the justice of the qualification, intended by M. Malgaigne as complimentary, that English Surgery, once superior, is still *almost* equal to the French. It is quite so, at least as regards the acquirements and achievements of its chief professors, although we fear we must admit it is not so as regards the diffusion of the improvements brought to pass by these. The progress of operative surgery, as indeed of every other branch of medical science, must be mainly due in this, as in any other, country to the zeal, intelligence and numbers of a limited class of persons who have sufficient opportunities at command, and sufficient talent and industry to avail themselves of them. To the general mass of practitioners the meeting with operations cannot occur with sufficient frequency to enable them to improve upon the modes of conducting them, or even in regard to those of a capital character, to justify their undertaking them at all. But it is none the less important that they should be fully prepared to meet such as may fall to their lot, some of these indeed occurring under such emergencies as to render other aid unavailable, or being so beset with difficulties and danger as to make a serious call upon the knowledge, manual dexterity, and nerve of the attendant. Mr. Liston justly much insists upon the necessity and importance of young men duly qualifying themselves for this important part of their duties, and strongly animadverts upon its neglect by even those who have had good opportunities of cultivating it.

"The art of operating has, even by many of those in prominent situations, been too lightly considered and too little practised on the dead body. The foundation of the study of the art must be laid in the dissecting-room, and it is only when we have acquired dexterity on the dead subject, that we can be justified in interfering with the living. Many poor creatures have been sacrificed in consequence of the ignorance, carelessness, and self-sufficiency even of scientific professors, who have either despised or neglected the study of surgical anatomy, the consideration of the casualties which may occur during the various operations, and the due education of their fingers. The infliction of unnecessary pain through want of adroitness in the use of instruments, and consequent protraction of the operative procedure—the hazarding in the slightest degree the safety of any one who puts confidence in us, and entrusts to us his life, or of one who, as in public practice, is, by chance, and without the means of appeal, thrown upon our care—cannot by any means be palliated or defended—and is, in point of fact, highly criminal."—*Introd.*

Common humanity will respond to these sentiments, and the truth and judiciousness of the following expressions concerning the operation for Hernia, (with which, and the means of arresting hæmorrhage, it is essential that every practitioner should be acquainted), will be generally admitted.

"It is highly desirable that every practitioner of medicine should fully comprehend the nature of abdominal hernia, should be well aware of its diagnostic signs, and be competent to afford relief before the symptoms have become of so urgent a nature as to indicate great and impending danger to life. Cases of hernia are most likely to be treated safely for the patient, and with judgment and skill, by the practitioner who is fully prepared to proceed to the last remedy when circumstances demanding it arise. It has been very truly said, that skill in operating is of the utmost importance in giving the surgeon self-possession: a bad operator will hesitate in the most simple cases, whilst a good and dexterous surgeon, like a man skilful in the use of weapons, will not enter rashly into difficulties, but, being engaged from conviction, will bring himself through with courage. Every young man, then, should endeavour to acquire some degree of dexterity in operating, for that will go far to make him a judicious surgeon."—533.

It must not be supposed that because Mr. Liston is one of the most brilliant operators this or any other country ever produced, he attaches an undue importance to the art in which he has attained so great a celebrity. Throughout his work he seems impressed with the important maxim of dispensing with operations whenever possible, but performing them with dexterity when unavoidable. He even seems to us to go somewhat out of his way in a work intended as a Treatise on Practical Surgery, in detailing the measures that may render them unnecessary; and takes every possible means of simplifying rather than exaggerating their difficulties, or magnifying the amount of skill requisite to surmount these.

But while rendering homage to the spirit in which his work has been executed, and acknowledging the truth of the statements it contains, as to the importance of the due education of the student in the art of operating, we must not forget the position the author occupies in the Profession as one of the Council of the College of Surgeons; and we may fairly enquire whether he and his colleagues have sufficiently interested themselves in this vital and important subject, one too which seems so especially to fall within their province. The Council enjoy a virtual monopoly of the instruction of pupils in surgery in the metropolis, as well as of examina-

tion into their acquirements after the completion of their studies; and we wish to ask how those of them who agree with Mr. Liston, as we doubt not all do, in attaching so much importance to operative surgery, can reconcile with their ideas of duty to the public the conferring their diploma upon many men who have received no kind of instruction in this branch of the art, have never practised the operations upon the dead body, and in some instances have never even dissected at all? We acknowledge that proof of proficiency in it could be ill demanded by the examining Board, who, in their capacity of teachers, are well aware that those who come before them have not had the opportunities for duly qualifying themselves; but we have a right to demand why this body of accomplished surgeons, well acquainted with the wants of the profession, has not bestirred itself for the provision of such opportunity? Of course the main obstacle to the performance of operations upon the dead in London arises from the scarcity of subjects, which, indeed, during the last season, well nigh amounted to a dearth. But such scarcity is surely by no means of necessary existence in an immense capital like this, when so much smaller places as Paris and Dublin are efficiently supplied; and it can only arise from the faulty provisions of the Act which was passed under the pretence of securing a sufficiency of an essential commodity to the profession, at the same time that it furnished security from outrages heretofore perpetrated. It is not our purpose here to discuss how far and why this act has proved ineffective in the former of these respects, as the subject, owing to its increasing importance, must, ere long, come more directly under our notice; but we maintain that, when the Council of the College became long since aware that no provision for a thorough surgical education existed, and that numbers (and when we include those who have contented themselves with the Hall Certificate these are great) were entering the profession quite destitute of this, it was incumbent upon them to institute a rigorous enquiry into the cause of, and to apply to the Government for a remedy for, so serious a grievance. And would this body have not been more worthily and more successfully thus employed in labouring to secure to the great mass of their future members (and present ones, for how many would continue their anatomical studies were the means within their reach) the means of effectual education, than in their recent attempt at the unjust elevation of the few at the expense of the many? Who can doubt, had the Council made a strong representation to Government of the imperious necessity of additional facilities for dissection, backed as their course would then have been by the unanimous voice of an approving profession, that long ere this such facilities would have been accorded. At all events they should have made the attempt publicly and boldly, and not through the mere medium of a quiet half hour's chat in the reception-room of the Home Secretary; and, impressed with the importance of an acquaintance with operative surgery, they should have declared they would pass no candidate who did not manifest it; and thus remove from themselves a serious responsibility, and throw upon others the *onus* of providing additional facilities of study, or submitting to the inconveniences of a paucity of duly-qualified practitioners. For men who have avowed their conviction of the necessity of this acquisition, there

can be no excuse whatever for their continuing to admit persons into their body destitute of it.

While the provisions for the pursuit of surgical anatomy continue so defective in this country, we can feel nowise surprised that young men, whose pecuniary means enable them to do so, resort to Paris for the completion of that portion of their education for which their own country disgracefully denies them the facilities. So, too, we have reason to believe the great mass of the French surgeons are far better qualified as regards the operative portion of their art than are the English. Indeed, when we have observed the number of young men who hurry up to our schools for so limited a period—their neglect of the practical portion of their education, either from lack of material or from their being cramped by the dictation of the ill-contrived and impossibility-requiring programmes of authority—and the futile examinations by which their capabilities are tested, but upon the strength of which they are dispersed all over the country as fitted to meet the exigencies of practice—we have arrived at the conclusion, either that the importance of a knowledge of anatomy for the practice of surgery has been much exaggerated by teachers, or that a vast amount of needless suffering is inflicted by incompetent hands upon unfortunate patients, and that knowledge which might have been obtained from the dead is too often acquired at the expense of the living. Those who are aware of the pains-taking with which our best operators have ever continued to keep up and refresh their anatomical knowledge, cannot but wonder that more cases of mal-praxis do not result from the exploits of the ignorant and half-educated.

The vast improvements which have taken place in the science of surgery during the last half century, whereby so large a number of operations heretofore considered necessary are dispensed with, and others still retained are so much simplified, have doubtlessly disposed many to look upon the acquisition of skill in operative medicine as being of less importance than it was formerly considered to be of, and to bestow proportionally less pains upon it. This is, however, very wrong. Operations even of a dangerous character may yet fall to the lot of every practitioner; and a glance at the table of contents of either of the works we have now under notice forcibly exhibits under what a multitude of circumstances our possession of manual dexterity and anatomical knowledge is of the last consequence to those who consign themselves to our care. It is to be feared that not a few of our students, unable to obtain sufficient opportunities of dissection, or feeling from the nature of the examination they expect to undergo no compulsion to avail themselves of such as may fall to their lot, hope, by the aid of manuals and treatises like the present, to be enabled to meet the exigencies of practice. But we fear such works must be placed in the same category as anatomical plates, rendering like them great assistance to those who have once made themselves practically familiar with the subject, and speaking an unknown language to such as have not done so. There can be no doubt that one of the worst characteristics of this present epoch consists in the endeavour to impart a knowledge of the various branches of medical science and practice by means of manuals and condensations of various kinds, which, whatever may be their effect in enabling ill-educated students to pass imperfect examinations, can never form

accomplished practitioners. It is deeply to be regretted that any repulsive encyclopædiacal reading of this kind should have displaced, to the extent it has, the perusal of the instructive and captivating productions of the great writers on medicine and surgery of this and other countries. Well founded as we feel this criticism to be, we are not justified in introducing it in the present article without excepting Mr. Liston's work from its operation. Indeed, its chief fault seems to us, in its possessing scarcely enough of the characters of the manual when we consider it is intended as a guide for young practitioners in surgery. Thus its description both of the precise pathological conditions of parts requiring manual interference and of the modes in which this last is to be rendered, are by no means sufficiently minute and clear to render reference to another work unnecessary; while the book has been expanded and rendered expensive by unnecessarily dwelling upon the general features of the various diseases adverted to, and by the republication of cases which had already appeared long since in the Lancet, and a simple reference to which would have sufficed. Nor do we think the very cursory notices of the various applications of tenotomy, the cure of aneurism by compression, and other interesting topics which have of late excited the attention of the surgical world, by any means sufficient. However, the author of a work which, in so short a space of time has reached its fourth edition, can well afford to disregard more important criticisms than we feel ourselves in a position to offer; and, in fact, we think the popularity which the work has attained is, in the main, fully justified by its great and undoubted merits. As a manual, M. Malgaigne's Operative Surgery has been so long held in esteem on the Continent, that it is very surprising it has not before this been translated. Its descriptions are frequently admirable, and the author's appreciation of the various operative procedures discriminating and sagacious. Treating upon the same department of surgery, it is singular to observe the portions of the subject each has not thought proper to embrace in his plan. Thus, Mr. Liston presents us with no account of the operations upon the teeth or the eye: and M. Malgaigne has no description of the treatment required for fractures and dislocations.

Of works of so comprehensive a character, any detailed notice is out of the question. Moreover, with Mr. Liston's former editions most of our readers are familiar; and we have not been able to discover the "considerable additions" advertised as having been made to the present one. We may, however, advert to one or two topics.

Bleeding from the Foot.—We copy M. Malgaigne's account of this simple operation, because we think it is far too seldom had recourse to in this country, seeing the great relief it is capable of affording in many affections of the portal system and generative organs, and the ease with which blood may frequently be so obtained, when its abstraction from a vein in the arm is either difficult or impossible.

"The internal saphena vein may be opened in front of the internal malleolus, or the external saphena in front of the external malleolus: but the latter is seldom large enough to be opened when the internal is not.

"The patient, seated in a chair or at the edge of his bed, first places his feet in hot water, until the veins are very apparent: then the surgeon selects the foot,

wipes it, rests it on his knee, protected by a napkin, and places the ligature two fingers' breadth above the ankle, moderately tightening it, and securing it with a bow on the opposite side. He then explores the vein, puts the foot again in the hot water, prepares his lancet, retakes the foot, and opens the vessel. Care must be taken not to prick the bone and break off the point of the lancet. If the blood flows in a jet it is caught in a basin; if it only dribbles slowly the foot should again be put in the water. We can then only judge of the quantity by the time or redness of water. When the bleeding is supposed to have been enough, the foot is taken out of the water and wiped, and a compress and figure of eight bandage applied. Care must be taken lest the water be too hot, or the foot plunged in too deeply. It is said that the weight of the column of the water tends to coagulate the blood, which stops the mouth of the opening. On this account the foot should be kept only just covered, and the wound wiped from time to time. It is well also to make the patient move his toes."—P. 53.

Opening Abscesses by Incision.—M. Malgaigne lays down the following general rules:—

"1. In order to lessen the suffering of the patient, the abscess should, if possible, be incised with one stroke of the knife. 2. It is proper that one extremity of the incision should terminate in the lowest part of the abscess. It has also been recommended to make the incision in the direction of its greatest diameter, and, if possible, parallel to the axis of the body, or well-marked folds of the skin; these two rules are subject to too many exceptions to be strictly preserved. 3. One opening is usually sufficient; it should be large enough to afford a free exit to the pus, if not, it should be enlarged with the bistoury and director. 4. When the abscess is very large, it is better to make several incisions than one of too great extent. 5. When a first incision does not reach the lowest part of the abscess, and when compression will not bring the sides of the sinuses together, recourse must be had to a counter-opening, either by cutting down upon the skin raised upon a director introduced through the first opening, or by retaining the pus at the point where we wish to open it, and incising as for common abscess. 6. When the abscess is prominent and superficial, incision from within outwards is preferred: but when the thickness of the parts prevents this, the incision should be made from without inwards with a straight bistoury. 7. When the abscess is situated under a muscle, it is better to separate its fibres than to cut them; but if this is not compatible with the direction of the incision, they may be cut across."—P. 79.

Mr. Liston has some interesting remarks likewise upon this important point in minor surgery. He considers a broad-bladed, sharp-pointed bistoury in a folding handle, and fastened when opened by a spring-catch, as a far more workman-like (a vulgar but favourite mode of expression of his) tool than the abscess-lancet in general use. The knife is to be entered steadily, with its blade perpendicular to the surface, and pushed onwards until the lessened degree of resistance allows its point to move freely. The superimposed parts are to be divided by a rapid sawing motion. Sometimes it has to be carried very deeply before the matter can be reached, and which, with due care, may be done safely even in the vicinity of important organs, the vessels and nerves being removed by the morbid accumulations to a much greater distance from the surface than they are wont to be normally. A free and clean opening should at once be made, so as to render squeezing the tender parts, extension of the aperture, or counter-openings unnecessary.

"It ought to be recollected, that an opening of an inch in length, quickly and

smoothly made, is attended with no more pain than a course and hazardous plunge of an abscess lancet, which, though the inflamed surface may be partially lacerated to some considerable extent, will be found, perhaps, to have barely penetrated the cavity by an aperture of not more than two lines,—the object being, after all, inefficiently fulfilled. The opening must be uniformly made at that part of the abscess which is most likely to be generally dependent; the state of the patient, and his probable position for some time after his little operation, must, with that view, be carefully considered beforehand."—P. 11.

Occasionally it is better to make more than one opening even at first. Abscesses extending over a large surface can seldom be completely discharged without. This is generally the case with abscess over the ligament of the patella resulting from bruise or inflammation of the bursa. An opening in this case should be at once made upon each side of the limb at the most dependent parts of the abscess. This expedites the cure, and is productive of far less pain than would be produced by subsequent incisions, which the progress of the case might require.

Mr. Abernethy's plan of partially evacuating large chronic abscesses, and temporarily closing the valvular aperture, is now exploded, on account of the violent constitutional irritation it often gave rise to. The opening, on the contrary, should be free and direct, so as to allow of no lodgement of matter, even if a counter-opening be required to prevent this.

"The common and thoughtless practice of squeezing together the sides of suppurating cavities, whether chronic or acute, ought by all means to be discouraged. The patient, it is true, seems to be relieved at the time from a greater load of matter, but in reality much pain and positive injury are thus inflicted. The surfaces so treated are apt, from the mechanical injury, to inflame; the vacuum occasioned is filled by air or by rapid secretions of bloody serous fluid, if not by copious escape of blood from the vessels deprived of their customary support, and torn from their connections. The after discharge is profuse and most offensive, and accompanied by a dangerous excitement of the system, an intense irritative fever, and delirium. Those who have had the opportunity of seeing, under the microscope, the injected lining membrane of the cyst of an abscess, will be little inclined to squeeze together the sides of purulent cavities into which they have made openings. The infinite number of vessels that ramify on the surface, and the layer of lymph by which these are covered, are the means by which the filling up and obliteration of the cavity is effected by nature; by bruising and destroying these, very much pain will be caused to the patient, and the cure will not, in all likelihood, be so rapid."—*Liston*, p. 14.

Lithotrity and Lithotomy.—Mr. Liston is one of the few surgeons in this country who have taken much pains to acquaint themselves with the performance and advantages of Lithotrity, and his evidence in its favour is of the more importance, inasmuch as the nearly uniform success which has attended his lithotomy operations might have prejudiced him against other procedures. He says:—

"Until very lately, patients applying to surgeons were constantly recommended to submit to the knife, in order to get rid of stone in the bladder, whatever might be the size of the concretion, or the state of the urinary organs. On the other hand, if he fell into the hands of the professed grinder, no matter what the peculiarities of his case, he was as certain to be subjected to the boring or hammering processes. Now that the merits of both operations are better understood and appreciated—some few surgeons having thought proper to turn their attention to

the matter, and study and understand lithotrity as well as lithotomy—patients have a chance of being treated judiciously and conscientiously, and of having that proceeding resorted to which is adapted to their respective cases. I was not slow to adopt the operation of crushing, have always had a favourable impression of it, and have throughout used the same language regarding it, yet I have the credit of being an opponent to lithotrity. I have all along been, and am certainly still, opposed to the abuse of any one operation, by its indiscriminate employment in all cases, and by its being practised by those alone who know no other. It can be employed safely only by those who understand well the healthy anatomy of the urethra and bladder; who are acquainted with their sympathies, vital actions, and pathological changes; and who both understand, and are in the constant habit of treating, derangement of their functions. The operation of lithotrity is applicable to patients above the age of puberty, when the symptoms have not endured very long, when the foreign body is ascertained to be about the size of the one sketched two pages back—measuring six or seven lines, or even more perhaps, say as large as a chesnut;—when the bladder and urethra are in a tolerably healthy and normal condition—as indicated by the power to retain the urine comfortably for several hours, and to pass it in a tolerably free stream; and when the viscus admits of injection and a careful exploration. * * * * * * * *

"The operation of lithotomy must yet continue to be performed on children, and on those of mature age who are so ill-informed or foolish as to permit the stone to attain an inordinate bulk. The concretions in young subjects are generally composed of a very dense substance, the oxalate of lime, in whole or in part; and the urethra is often so narrow as to preclude the application of instruments strong enough to reduce them to fragments. The reasons for giving a preference to incision over crushing, when the stone is large, have been already given.

"This favorable opinion of lithotrity was published in 1840; since when it has been confirmed and much strengthened by ample experience. Of late years, in point of fact, I have scarcely been obliged to have recourse to lithotomy at all in private practice. At the hospital, patients yet present themselves with large stones and bad bladders. Then lithotomy is both a less painful and much more safe operation, as already propounded. During the above period twenty-four patients have been cut, and all have recovered without accident; these patients have been of all ages from two to eighty years of age, and some of them not over favourable subjects. So that, after all, there is not much to find fault with as regards this cruel and bloody 'operation' when carefully set about."—P. 500–5.

It is satisfactory to know that Mr. Liston's great success as a lithotomist has been obtained by the use of the simplest apparatus, his incisions being commenced and completed by the same straight knife. He makes his external incision very free, but the internal one is very limited. "It should certainly not extend beyond six or seven lines from the urethra outwards and downwards," which in all ordinary cases will allow the requisite dilatation to be safely made. "The object in following this method, is to avoid all interference with the reflexion of the ileo-vesical fascia from the sides of the pelvic cavity over the base of the prostate gland and side of the bladder. If this natural boundary betwixt the external and internal cellular tissue is broken up, there is scarcely a possibility of preventing infiltration of urine, which must almost certainly prove fatal." An opening of this extent, owing to the elastic and yielding character of the parts, will readily admit the finger, and allow of the extraction of a stone of considerable dimensions. In cases in which, from the corpulence or unmanageableness of the patient, or the size and rigidity of the pros-

tate, the inner opening is executed with difficulty, the blunt gorget may be employed for enlarging the opening into the bladder, but Mr. Liston has never had occasion to use it. The exact position of the stone must be carefully ascertained before using the forceps, and its favourable adjustment within these carefully adjusted by the finger. The use of the forceps is in fact the most difficult part of the operation; "and it is equally the duty of the pupil to practise the seizing and extraction of the stone, as it is for him to study the relative anatomy, so that he may be enabled to cut into the bladder with precision and safety." An important piece of advice this, which is much needed, as students invariably occupy themselves with the cutting part of the operation alone. If the surgeon finds upon examination with his finger that the stone is a very large one, he must convert his operation into a bilateral one before attempting to remove it. If his external incision has been sufficiently free and low enough in the perineum it will require no enlargement, and he has only to pass a narrow-bladed, blunt-pointed bistoury along the finger, and make a similar incision on the opposite side of the neck of the bladder, directing the edge of the knife towards the right tuberosity of the ischium. In nineteen cases out of twenty the single lateral incision suffices. When the incisions are placed low, and the deeper ones performed cautiously, there is no danger of hæmorrhage; and of considerably more than 100 operations performed by Mr. Liston, only one old man suffered from hæmorrhage, and that because the diseased condition of the branches of the hæmorrhoidals prevented their contracting when divided. Mr. Liston attributes much of the success of his operations to his practice of introducing through the track of the wound, and securing in the bladder, a gum-elastic tube, which, by facilitating the flow of urine, much lessens the danger of infiltration and its dreadful consequences, and affords an easy means of suppressing any excess of arterial or venous oozing. It need not be retained in children for more than 20 hours, but in old persons of lax fibre it should be kept in for 40 or 50 hours at least.

"The operation of lithotomy, about which so much dread has lately been excited and fomented by interested persons, and which certainly, according to the complicated methods, and more especially with the great apparatus delineated at p. 506, was formidable and not over-successful—if performed in the very simple and easy method recommended, is effected with much less pain than is supposed; it is completed with perfect safety, in a short space of time, and offers very favourable results. It is, however, an operation that never ought to be undertaken without due consideration of all the circumstances that may arise; and the surgeon who undertakes it must have resources within himself sufficient to encounter and overcome difficulties in all the various stages of the proceeding. Were the circumstances of all cases precisely similar as regards the depth and resistance of the parts, the size and consistence of the prostate and of the foreign body, the capacity of the bladder, and the width of the outlet of the pelves, then the operation might always be completed in a given time with certainty, and in the same manner. But it is not so: unforeseen obstacles occur, from first to last, and the operator must make up his mind to proceed in all cases with the greatest caution and deliberation: he must commence with a determination to finish his task safely and well: and he will also accomplish this quickly, when the state of parts is favourable, and nothing unusual intervenes."—*Liston, p.* 518.

The practice of making so small an internal incision is a controverted

point with some of the best operators. M. Velpeau, Sir B. Brodie, Mr. Fergusson, and M. Bresciani de Borsa* are advocates for as small incisions as possible into the prostate being made, while the testimony of Cheselden, Martineau, John Bell, Samuel Cooper, and M. Malgaigne is in favour of a free incision. The last-mentioned surgeon thus speaks of the operation, after having described its varieties invented by different operators :—

"After much study and comparison, I have come to the conclusion that but very little influence on the result of the operations, as regards the life or death of the patients, is to be attributed to the proceeding of operation. It is the pain and inflammation that kill the patients after lithotomy; and the most painful causes of these assuredly are the dragging, tearing, and bruising of the tissues; accidents inevitable in all the proceedings of perineal lithotomy, when the calculus surpasses the most moderate proportions, and which, when the patient does not sink under them, cause the most serious infirmities. I have seen several patients, who had been cut by the most skilful operators, the limits of the prostate being respected, who had lost all faculty of having erections and ejaculating. I saw one who, in addition to the absolute loss of his genital functions, could not retain his urine, and was obliged to keep his penis constantly squeezed by a constrictor. Double, triple, and quadruple incisions do not definitely augment the extent or elasticity of the external layers of the prostate (what does this mean ?); and, by dividing it still more, they seem to expose it more also to be bruised in the passage of the calculus.

"In my opinion, there is only one way of rendering perineal lithotomy less dangerous, at least as regards the operation itself; it consists in following a precept entirely opposed to that which is generally laid down, viz. in *dividing the prostate freely on one side beyond its limits;* cutting the neck of the bladder and the cellular tissue, if the size of the stone necessitates it—in a word, making so free a passage for the stone that the wound may remain an incision, and not be complicated by contusions and lacerations. As for the external wound, it seems to me that it might be enlarged advantageously if required, by encroaching more or less on the right side of the raphe, to obviate the necessity of approaching too near the sciatic tuberosity. An external incision, bilateral if necessary, and a unilateral incision of the prostate, but with all the necessary extent, is the proceeding to which I give the preference, and which has already been adopted with success by several skilful lithotomists."—*Malgaigne*, p. 529.

Inguinal Hernia.—M. Malgaigne, speaking of *Trusses*, says that much consideration has led him to conclude—1. That three-quarters of the hernias are badly supported by any truss applied in the ordinary manner. 2. The English spring is far preferable to the old French one. 3. That moveable pads, in a great number of cases, have some real advantages over the fixed. 4. That in oblique inguinal hernia the pad should press on the course of the canal, and on the internal orifice, without touching the pubis, unless in some exceptional cases. 5. That in direct hernia the pad should be more voluminous, fixed, and resting on the pubis. 6. That hard pads are most suitable for compressing the canal, soft ones for direct hernia. 7. That it is much to be wished that surgeons would pay especial attention to this very important branch of our art, which has been too long

* See his interesting description of his mode of performing lithotomy, by which very great dilatability of the prostate is implied, *Medico-Chir. Review*, April, 1846.

left to bandagists. M. M. believes that, if trusses were better applied in adults, a cure might be effected in them as well as in children. "I have seen an inguinal hernia cured in one month in an old man 68 years of age, by complete obliteration of the canal obtained by means of simple compression." Mr. Liston, in like manner, says, "I have met with repeated instances in which a person, well-fitted with a truss, has been enabled, after a few months, to discontinue its use."

The Taxis.—The following rules for employing this, are laid down by the first-named writer.

"1. Evacuate the bladder to augment as much as possible the capacity of the belly. 2. Recommend your patient to breathe freely, without crying out or raising his head—in fact, not to make the least effort. 3. Make at the commencement but slight pressure, so that you may afterwards augment it by degrees, and continue it longer without bruising the hernia. 4. Return first the parts that last protruded. 5. Return them in the same direction they came out in. 6. In certain exceptional cases these general rules fail, and the patients themselves are in the habit of using some special way or means, to which it is better to have recourse. There are three principal proceedings.

"(1). The surgeon seizes the hernia with one hand, in such a way that the palm of the hand presses on the base, and the fingers all around the neck of the tumour. He raises it in this way, and pushes it in the direction of the ring, compressing with his fingers the part next the ring, so as to diminish it to a suitable diameter. (2). He embraces the hernia with one hand or both (according to its volume), exactly applying his fingers on its entire surface, so that if possible no portion may remain uncovered, and presses all parts of the circumference of the tumour towards the centre. The efficacy of this pressure is augmented by, as it were, drawing the tumour out from the belly, and carrying it from one side to the other, compressing it with your fingers, so as to unfold the portion of the intestine contained in the ring, and cause the wind and fæces that engorge the hernia to re-enter the belly. (3). He leaves free the bulk of the tumour, but applies one or two fingers close to the ring on the sides of the hernial sac, and pushes thus across the ring the part immediately next to it. When you have in this way returned a small portion, keep it back with the fingers that pushed it in, and push up another, but with the fingers of the other hand, and so on. It often happens that when the strangulated portion, which formed a sort of plug, is returned, the rest follows easily. * * * * * * * * It remains for us to make known three other modifications which seem to us to deserve serious attention. M. *Ribes* takes a mattress, folded and doubled up, so that the edge of the superior fold passes a little beyond the edge of the inferior one, and the surface of the mattress describes a very oblique plane: one or two bolsters are put under the doubled end of the mattress, to increase this obliquity, and the whole is covered with a sheet. This done, the patient is placed on the bed, with his buttocks on the edge of the mattress, and his thighs stretched out and in a line with his belly, and with his pelvis much elevated, and his diaphragmatic region as low as possible. His head must be supported with a pillow, and he must keep in this position all the time required. Then the taxis is tried; and, between each attempt, a bladder of ice kept on the tumour. *Amussat* gives the patient a similar position, but, in addition, he flexes the thighs to relax the abdominal muscles, and inclines the entire body to the side opposite the hernia; and, lastly, while he is performing the taxis, an assistant keeps up a gentle traction on the belly, trying to draw it to the healthy side, and raises from time to time the parietes of the abdomen, slightly pinching up the skin. But, what is more important, M. Amussat continues the taxis during two, three, or four hours, or even more, and has, by these means, reduced hernias that seemed only

curable by operation. It is an admirable method, when you are sure the intestine is free from gangrene; but one to which recourse must not be had when the duration of the strangulation, or the rapid progress of unfavourable symptoms, gives a reason to dread this fatal termination. Lastly, M. *Koehler* applies a cupping-glass on the hernia itself, to draw outside a large quantity of the intestine, and especially disengage the strangulated portion. This proceeding has seemed to him to greatly facilitate the reduction.—*Malgaigne*, pp. 421-3.

The Operation.—After describing the procedures of other surgeons, M. Malgaigne adverts to his own.

"I make my incision, not on the sac and scrotum, but on the place where the strangulation seems to be situated, prolonging it upwards and downwards to the extent required by the *embonpoint* of the subject, and the size of the hernia. All the tissues are divided in the same way down to the peritoneum; and, as we thus expose and lay bare the vessels, we put them aside, and have nothing to fear from them. If you find the strangulation is caused by a fibrous opening, you need not open the sac, but reduce the hernia. If not, the neck is divided from without inwards, carefully and gently; or, if the stricture appears very strong, you make a small incision in the peritoneum, either above or below the neck, which you then raise upon a director and divide.

"In this proceeding I find the great advantage of being able to see everything as I go on. 2. I reach the strangulation by the shortest way, and with the least possible incision. 3. I leave untouched the scrotum and sac, and consequently am not troubled with the cicatrization and suppuration of a wound, at all events useless. I lately performed it for a large scrotal hernia; the neck of which was situated on a level with the abdominal ring, the sac, which I had respected, though I opened its neck, filled in the first few days with a certain quantity of liquid, which was reabsorbed as the inflammation of the superior wound went down, and the patient was cured without accidents."—P. 429.

Operation without opening the Sac.—Mr. Liston thus states his opinion upon this important point of practice.

"He will thus cut the resisting tissues to a sufficient extent, in a line parallel to the linea alba: the reduction of the contents of the sac may then be attempted; and, if it prove successful, one great danger of the operation, that arising from exposure of the peritoneal sac, the lowering of its temperature, and the consequent shock upon the system, is avoided. But, even in favourable cases of this variety of hernial protrusion, there are difficulties to be encountered in accomplishing this most desirable object; the constriction is caused by a condensed cellular and fibrous tissue immediately investing, and incorporated with, the serous cyst. The stricture may, in fact, be said to exist very often in the neck of the sac itself, and this must be cut before the contents can possibly be returned. In many cases, the reduction is impeded by adhesions, and by entanglement with omentum: again, when the strangulation has existed any considerable time, it is desirable to ascertain the precise condition of the protruded parts, and to consider, after careful and actual inspection, whether they should be returned or not."—P. 551.

When treating of *Femoral Hernia*, he thus expresses himself:—

"He may now try to reduce the contents of the sac by pressure: taking especial care not to push back sac and all—rather a serious accident. This plan was advocated strongly, long ago, by Petit and Monro secundus, and has lately been

revived and practised by Mr. Key, Mr. B. Cooper, Mr. Luke, and others. I have fortunately succeeded in effecting this object in a considerable number of instances, within these few years: and it is a proceeding which I should most strenuously advise the adoption of, when nothing contra-indicates it. The attempt can do no harm: it causes little or no delay, and, if it is not successful, the sac, after all, is opened, and the operation completed. If it does prove successful, the surgeon's mind is relieved of an uncommon load of anxiety."—P. 558.

Dilatability of the Vagina.—M. Malgaigne states, in the following passage, the varying degree of this according to age:—

"The dimensions of the vaginal orifice are very variable. It is important to know that in young women it is exceedingly dilatable; it is less yielding in adults; and after the cessation of the menses its rigidity goes on increasing, so that, at a very advanced age, instead of feeling at this orifice a supple ring, yielding under the fingers, we find it hard and splitting at the slightest effort made at overcoming its resistance. It then sometimes scarcely admits the little finger, and the vagina itself, instead of offering its usual rugæ, presents polished walls and a very constricted capacity. The result is that, in young women, however narrow the orifice may appear, we may confidently rely on its dilatability; that, in adults, we must not place so much reliance on it, and must use a speculum not much larger than the apparent capacity of the orifice; and, lastly, that at a later period, we should be more reserved in employing the speculum at all; proceed with gentleness and caution, in order to avoid lacerations, which cicatrize with difficulty; and use only a very small speculum. Lisfranc has found it necessary to prepare the parts, during eight or ten days, by dilating them with prepared sponge."—P. 543.

We must here conclude our extracts, and can only wish a continuance of success to two such excellent works.

DEONTOLOGIE MEDICALE, OU DES DEVOIRS ET DES DROITS DES MEDECINS, DANS L'ETAT ACTUEL DE LA CIVILISATION. Par le Docteur *Max. Simon.* Octave, pp. 567. Paris, 1845. Bailliere.

Medical Deontology, or the Duties and Rights of Medical Men in the present State of Civilization. By Dr. *Max. Simon.*

IT was only in our last number that the following passage occurs at the close of an article upon Military Punishments: "Surely there is no occupation, in which medical men can more profitably fulfill their beneficent *mission*, than in using all their professional influence and authority to plead the cause of the suffering and oppressed against the heartless neglect or tyrannical severity of irresponsible taskmasters. Of recent years, the fruits of enlightened medical philanthropy have been gloriously displayed in the changes already effected in our prisons and lunatic asylums, &c., and in the improved general œconomic treatment of our pauper population. May the same good spirit continue to animate every member of the profession, in whatever sphere he may be placed! It is thus only that

our calling may justly claim to itself the proud distinction, that has been assigned to it by the eloquent orator of antiquity, of being 'an art almost divine.'"

When we wrote these words, we did not expect to have so early an opportunity of directing the attention of our readers to the general question of Medical Deontology,* or, in other words, that of the Rights and Duties of our profession. Dr. Simon's work had indeed been published several months before; but it is only within the last few weeks that it has been brought under our notice. The perusal of it, we rejoice to say, has given us very great satisfaction. Everywhere it breathes a spirit of high-toned moral feeling, and dignified independence of thought. May its influence be felt and appreciated by his countrymen! To us it will be a source of very sincere pleasure if the extracts, which we propose to select from its pages, have the effect of introducing it to the notice of the British medical public, and of its merits being acknowledged by our press. Much honour is due to the literature of France that, amid the general apathy and indifference of medical men of all countries, in the present century, to the intimate connection between their very responsible calling and the exercise of the highest social virtues, one of her sons has not been ashamed to step forward with the bold avowal and manly vindication of principles, which, although they may be regarded as Utopian or Puritanical by many, cannot but actuate every truly great and good physician in his intercourse with the sick and afflicted of mankind. He will ever remember that he has other duties, besides those of studying the physical effects of disease, or of prescribing remedies for its relief; great and pressing duties which he owes to himself, to society, and to his God. It is the sin of the present age to view everything with a sort of materialising and mercantile spirit, to estimate the value of an object only by its immediate results, and by the effects which it sensibly and visibly produces. It is an age of facts rather than of principles, of experiment rather than of reflection. How strikingly is this the case in all that appertains to the science and art of Medicine. Every year, nay, every month or every week, brings forward its new remedy, its novel discovery. The journals teem with practical *facts*—so, at least, are they supposed to be, and so are they termed. It is almost surprising that anything now remains to be found out, considering the abundance of successful or infallible remedies or modes of treatment for almost every disease. Then again in Physiology, every thing is determined by positive experiments on living animals; and, when so determined, it is asked how can the conclusions, so derived, fail to be correct? It may be so; but we should like to be told, how comes it to pass that there is so much discrepancy and downright contrariety of statements and conclusions among different physiological observers? The question is puzzling; where is the solution to be found? What is to be done? If we might presume to answer, we should say; watch more, work less; interrogate nature by long and patient observation, rather than by rough force or cruel compulsion. Are we not told that not even a sparrow falls

* This term was first used by Bentham to denote the science of Ethics. It is derived from δεον *quod decet, æquum* and λογος *sermo.*

to the ground without the knowledge of Him who made it? What then shall we say of the shambles of the modern vivisector? But it will not do to digress too far from our more immediate subject.

We have said that the medical men of the present day have their attention directed almost exclusively to the sensible phenomena, the practical details of their profession, and thus overlook, and perhaps have never so much as thought of, any other duties that may be required of them. Medicine, it would seem, is not now regarded as a high and holy pursuit, that ought to elevate the thoughts and spiritualize the feelings; it is a mere vocation by which a man earns his bread. "Healing the sick" is no longer a sacred calling; it is a trade, a mercantile occupation. Hence the numerous and varied means by which its votaries seek for distinction and repute. One man succeeds by keeping an open shop and dealing in perfumery and physic; another attracts his neighbours by the brilliant-coloured light over his door; a third is active in parochial matters, and acquires the name of a clever, smart man of business; a fourth trusts chiefly to his carriage to drive him into practice; a fifth communicates the results of his extraordinary success in the treatment of certain diseases to the columns of a journal, or gets a puff into a newspaper; while a sixth makes a hit by a well-timed popular work, dressed out, it may be, in the most attractive style. All is done for effect, to catch applause, to build a fortune. We say this, not so much in the spirit of censure as in that of regret—regret at witnessing so noble a profession as ours degraded to the level of a common trade. This is assuredly not as it ought to be; but that such is really the case, will not be denied by many. And what is the cause of this state of things?—simply and solely this; forgetfulness or disregard of the real extent and degree of the *duties* which the pursuit of the medical profession involves. But then, it will be asked, what are these duties that are spoken of? Sydenham suggests them in these memorable words:—

"Whoever applies himself to Medicine should seriously consider the following things. In the *first* place, that he must one day render to the Supreme Judge an account of the lives of those who have been entrusted to his care. *Secondly*, that whatever skill or knowledge, under divine blessing, he has acquired, ought primarily to be directed to advance the glory of God and the welfare of mankind; it is a disgrace to make these heavenly gifts subservient merely to the ends of avarice or ambition. *Thirdly*, that he has undertaken the care of one who is neither ignoble nor to be contemned; in order that he may estimate the value of a human being aright, let him remember that the only begotten Son of God became man, and ennobled the nature which He assumed. *Lastly*, that he (the physician) himself is not exempt from the common lot of mankind, but is subject to the same laws of mortality, the same accidents and the same sorrows as others; therefore let him, fellow-sufferer (ομοιοπαθης) as he is, with greater diligence and love seek to relieve the sick and the afflicted."

Now the question comes to be—supposing that we are willing to take our own great countryman as a teacher of professional ethics—are these the motives that actuate medical men in the present day? Let us hope that they are so; but are we warranted in entertaining such a hope? Without prying into the recesses of each other's hearts, may we not fairly and in the spirit of perfect charity judge, to a certain degree at least, by the outward acts and manifestations of conduct? Do any medical books

or lectures, in the present day, ever breathe so hallowed and so solemn a spirit as the word which we have just quoted? Have not some of our recent works on Physiology been polluted with open indecencies, and has there not been an air of infidel presumption cast around not a few of the vaunted discoveries of modern times? Go to our very temples of medical science, and listen to the senators of the profession, clothed in their robes of authority, and seated, it may be, in their chairs of state. Has not the hall of one of our Colleges, within the last twenty years, been made the theatre where atheistic scepticism and bold impiety have been proclaimed under the garb of scientific exposition, and where the most injurious calumnies have been uttered against honourable men, who had no opportunity of reply?—and have we not heard of the learned fellows of another College now listening with a most edifying patience to the frippery of a semi-classical semi-fashionable gossip respecting the deaths of distinguished characters, and then sanctioning by their presence the advocacy of the juggleries of mesmeric artifice? Is it in such ways as these, that the established authorities of our profession seek to elevate its importance or enhance its utility? True; the sins or follies of one or two individuals are not to be charged against an entire body; but then, has either of the public bodies referred to disavowed all sympathy in such proceedings, or protested against the desecration of a noble science by such sillinesses on the one hand, and such profligacies on the other? Again, what has been done by these institutions—and here our remarks apply more especially to the College of Physicians—to which so much power and so many privileges have been granted, to promote the cause, we shall not say of the profession, but of medical philanthropy or beneficence throughout our land? Has it been to them that society is indebted for any of those ameliorations, or even for any suggestion touching those ameliorations, in the hygienic condition of our working-classes, that are now happily on foot? Has anything been done by them to enlighten and direct the government in enquiries respecting the state of our prisons, penitentiaries, or workhouses; the influence of trades and occupations on health; the effects of different sorts of food and of different geographical localities on the same important subject; the spread of contagious, and the diffusion of epidemic diseases; the intricate question of quarantine; the establishment of medical missions in countries where human beings are left to pine in wretchedness and suffering from want of professional relief?—not to mention a variety of other kindred subjects, all of which ought to be viewed as belonging to the domain of the science of medicine, and the investigation of which rightly appertains to one at least of our public medical establishments. Hitherto little or nothing has been achieved in this field of most useful labour, save by individual exertion and zeal. We must indeed except from this rebuke the conduct of our Army and Navy Medical Boards; for to them, under the admirable superintendence of their present most enlightened and conscientious directors, is the profession indebted for numerous and very valuable acquisitions to medical knowledge, which no private individual could have effected. Can we say as much for the College of Physicians? The question constantly recurs to one's mind, what has it, in its corporate capacity, done for science or humanity? Even when directly appealed to and consulted by government on matters imme-

diately appertaining to the health of the community, the conduct of its leading men, viewing it as a medical cabinet or institute, has seldom commanded the confidence either of the profession or of the public. Witness, for example, the doings of the Board of Health that was appointed on the approach and during the invasion of the Cholera. It is unnecessary to multiply instances of the utter inefficiency of this College; its very existence is more negative than positive. How shall we explain or account for such a state of things?—the answer is at hand; we have already spoken it. Those, who have had authority in its affairs, have either never duly considered, in its full and simple bearings, the solemn subject of Medical Deontology (to use our author's term) as it has been so emphatically laid by him, whose very name gives lustre to their college, or—painful as the assertion of the alternative is—they have wilfully and unworthily sacrificed principle and duty to the claims of selfish or corporate interest. Let us in charity believe that their error has proceeded rather from ignorance and want of due reflection, than from disregard of obvious moral requirements. The great and paramount question that we ought all to put to ourselves, whether as individuals or as members of a public body, is sternly and simply this; what is our *moral* duty? It is not what do we owe to such and such a one for having promoted our views, or coincided with our opinions? nor yet, what will best promote the secular interests or respectability of our party or our college? The only infallible and unerring guide of conduct is to "*do justly*, love mercy, and walk humbly before God." Hear what our author says in the admirable Introduction with which he prefaces his work. After alluding to the utter inefficacy of mere statutary enactments to teach medical men their duty (morally, not technically, be it remembered) under various trying emergencies, temptations, and harassing perplexities of their profession, he says:

"To the man who refers to God only, in his decisions and actions upon the subject of a thing so precious as life, the thought of God should ever be present; a philosophy, in which the sap of this fertilizing thought does not circulate, must be incapable of being the constant guide to the physician, amidst the numerous obstacles he encounters in the exercise of his profession. The study of medicine mounts up to God by the sympathy which the sight of suffering awakens in us; but science as it is of so lofty an origin, it does not completely do its work, save on the condition of requiring from Charity her love and her devotedness. The physician, who submits his conscience to the light of this elevated philosophy, may err; but his faults will only be imputable to the imperfections of his science. Comprehending the dignity of human nature and the deep signification of life, he devotes himself entirely to the study of a science, which may exercise so decided an influence over the individual destiny of men. Prudent, circumspect, he will not be found carelessly to adopt those premature theories, which sometimes pass over a generation like a fatal epidemic. In cases where theory and experience deny him positive instruction, he will confine himself within the line of a sage expectation. To whatever rank of society he belongs, the suffering man will be his brother by the double fraternity of pain and hope; through the most repulsive rags of misery, he will recognize in him the indelible character of his heavenly origin, and will proffer to him with a tender solicitude the most devoted attentions. Kind and considerate to all, he will not avail himself of amenity of language as a means of acquiring wealth or distinction. He will know that suffering spiritualizes man, if we may so speak, and imparts momentarily even to the most rugged natures a delicacy of sensibility, which a *brusquerie* of manner deeply offends. To the humble pallet

of the poor, as to the perfumed couch of the rich, he will bring the same gentleness, the same affability; and thus, while he discharges a hard duty, he also renders more sure the action of those curative means which science has prescribed. In a word, in whatever situation the physician is placed by the exigencies of his profession, he has imperative duties to fulfil, both towards himself and towards society, and it is from his conscience alone that he can acquire the light he needs to direct him with safety.

"But Conscience abandoned to its sole inspirations may stumble in those dark paths, in which she should guide us; she is accessible to all the passions, she has her caprices, like every power which is not attached to something fixed and immoveable. To find then a more sure guide, we must rise still higher, even to Christianity itself, which provides infallible instructions for all the circumstances of life, to Christianity, whose doctrine summed up in a single word, Charity, allies itself so wonderfully with a science, whose chief end is the relief of human suffering."

There is much sound philosophy as well as virtuous feeling in the following remarks on the powerful influence, either for good or for evil, which a medical man may have on the future welfare, no less than upon the present comfort, of the poor creatures that commit themselves to his care. It would serve to exalt the character of our profession not a little, if this subject were more frequently and more seriously considered than there is reason to believe that it is by those whom it concerns.

"In great cities, in manufacturing towns, where the enticement of higher wages or the seductions of pleasure tend more and more to concentrate population, the physician is called to exercise a powerful influence over the welfare of men. He may spread around him most fertile ideas; speaking by turns the language of science, of reason, of morality, knowing by a wise and prudent intimation how to render the sufferings of disease conducive to a salutary reform, he may have it in his power, at the same time, to bring back to virtue and restore to health a multitude of unfortunate beings who abide in ignorance, misery and crime. There is between these three elements, which a gloomy etiology so often shows us in the pathology of misfortune, a mysterious affinity which reveals itself to the least attentive observer. Disease, in the thousand forms it assumes, is in many cases the sinister expression of this degradation, or at least of this incomplete development of man. Suffering, by suspending in a moment the illusions of passion, causing the deceptive mirage of pleasure to cease, and restoring to reason the freedom of its judgment, admirably prepares the mind for lessons of virtue. A magnificent apostleship is here afforded to the physician, whose generous heart ought not to be a stranger to any of the miseries of mankind; and, while his hand places over the wound of the body the *dictamnus* of science, he may not unfrequently awaken in the deadened soul the feeling of the dignity of human nature. Not that we pretend that the physician should here usurp the office with which the priest is clothed in Christian societies; his work is pre-eminently a work of science, which he ought beyond every other to strive to accomplish; but if, in the accomplishment of this work, he ought constantly to be inspired with the sentiment of a tender sympathy, with a devoted charity, it is impossible for the man who has comprehended the importance of this mission not to rise up to the source of the evil, the effects of which he combats in the fatal maladies with which the body is affected. In how many instances is the prophylaxy nothing else than the destruction of vice, under whose influence the most robust constitutions are seen to bend? In using an instrument towards the instruction of morality, we not only do not go beyond the limits of science and art, understood in their real relations, but we should evidently fail in our object, if we did not reach the first cause of those organic or functional disorders which we have under our eyes. Many physicians before us have alluded to this relation between medi-

cine and morality, and have endeavoured to show how one might become a useful auxiliary to the other. Although a similar attempt, renewed in the present day, and by an author who has scarcely any other claim to the attention of those whom he addresses than the purity of his intentions, may be a little venturesome, we shall boldly enter this path, convinced as we are that the physician, who walks therein, resting upon sound doctrine, may serve one of the gravest interests of society."

M. Simon, evidently deeply impressed himself with the responsibility of the mission which a medical man voluntarily takes upon himself, takes every opportunity of urging upon his younger readers the necessity, nay the policy, of their being guided upon all occasions, not by love of fame, wealth, or distinction, but solely and entirely by high moral principle.

"In all professional careers, which suppose in those who have embraced them a high degree of intellectual development, the only rule of conduct worthy of the lofty nature of man is the abstract idea of duty; an idea that is practically embodied in a devotedness to the common weal. True, the man of intellect requires to be housed, fed, and clothed as well as the mechanic. Nay, the physical wants of life seem to increase with the degree of intellectual development; and the question therefore comes to be, how is this severe moral law, which we maintain is especially obligatory on the professional man, to be reconciled with the legitimate satisfaction of these wants? We answer—by another law, the truth of whose existence is daily exemplified in the movement of human affairs; viz. the almost constant coincidence of private advantage with public good. And even should it be the case that he, who devotes himself to the benefit of his fellow-creatures, finds himself deprived of that position which his services entitle him to expect, shall he not find some compensation for his trials in the secret joys of an approving conscience? In the noble inscription which Fabricius D'Aquapendente had placed over the door of his study—*lucri neglecti lucrum*—we may read the satisfaction of one who had found happiness in doing good."

These are noble sentiments, and do honour to our author's heart. Let them not be looked upon as the mere fancies of Utopian benevolence. There is more worldly wisdom, than most men of the world will admit, in the reflection that "there is that scattereth and yet increaseth; and there is that withholdeth more than is meet, but it tendeth to poverty."

The grand object of the Second Chapter, which treats of the "influence of medical studies and of the habitual spectacle of suffering upon the *morale* of medical men," is to guard them against the materialism of thought and feeling with which our profession has been so often reproached. From having so much to do with the mere machinery of life, the mind of the young enquirer is apt to identify the cause of a function with the organ that performs it; and thus, upon all occasions, to associate the existence of a vital principle with the presence of a certain mechanism and of certain chemical changes that are continually going on within the living body. The results too of the study of Pathological Anatomy on the one hand, by showing the frequent, although certainly not the constant, connection between abnormal structure and disturbed or perverted physiological development, and, on the other hand, of Comparative Anatomy, unfolding as this science does the relations between organization and the gradual manifestations of life, have tended not a little to this contracted state of mind, that would in all cases limit the investigations of truth to those facts only that are discoverable by the scalpel, microscope or test-tube. It is the

duty of the professor or public instructor to seize, every now and then, a favourable opportunity to spiritualise the facts and phenomena of physical research, and to divert the thoughts of the young enquirer from the mere materialism of the science which he is teaching, by lifting them to the contemplation of moral and religious truth.*

"Galen, at the close of his work on Anatomy, struck with the marvellous harmony of design that pervades the complicated mechanism of organization, exclaims that his book is a hymn to the Divinity; and it is told of M. Biot that, when relating the wonderful discoveries of Newton, he stopped short in the midst of his discourse, and wept tears of admiration. Unfortunately all medical men are not capable of such elevation of thought and sentiment; whether it be that the materialism of their pursuits has contracted to them the perspective of life, or that they do not feel any longing desire to pass the limits of cold analysis. It cannot be doubted but that, upon minds that are constituted so, the exclusive study of the material side of life, the observation of the phenomena of the suffer-

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* The last Hunterian Oration afforded a melancholy instance of the evil effects which a neglect of the principles here inculcated will almost inevitably produce, even in men of high literary and professional attainments. He, who thinks and talks lightly of those subjects that concern the glory and honour of his God, is the very person who will be most apt to forget the duties that he owes to his neighbour. The true Christian is characterised not more by his reverential humility than his gentle philanthropy. The latter grace of character however, to be thoroughly sincere and influential, must be grounded in the former. Sufficient condemnation has been passed upon the oration in question for the outrages it contained against the decencies of social life; we trust that such an exhibition will never be witnessed again in any of our public institutions. There was another blemish, which has not yet, we believe, been noticed, but which (if the above remarks be true) deserves no less a passing reprobation. Among other quotations, intended to illustrate and adorn his speech, Mr. Lawrence adduced those well-known lines from Virgil, as involving a physiological truth:

> Principio cœlum, ac terras, camposque liquentes,
> Lucentemque globum Lunæ, Titaniaque astra
> Spiritus intus alit; totamque infusa per artus
> Mens agitat molem, et magno se corpore miscet.
> Inde hominum pecudumque genus, vitæque volantum,
> Et quæ marmoreo fert monstra sub æquore pontus.

Such a sentiment may be worthy of a heathen poet, a believer in the doctrines of the Pythagorean philosophy that taught that the Deity was nothing more than the *anima* or *spiritus* of the material world, but cannot be received by any one who admits the truth of revelation,

With equal bad taste was the exquisite passage in Lycidas.

> So sinks the day-star in the ocean bed,
> And yet anon repairs his drooping head,
> And tricks his beams, and with new-spangled ore
> Flames in the forehead of the morning sky,

(designed, it is well known, to symbolise the resurrection of the redeemed "through the dear might of Him that walked the waves"), forced in at the climax of the peroration to denote or illustrate what?—the resuscitation of Hunter's fame! Were it not for the schoolboy absurdity of the exhibition,—for the speaker suited the action to the word, by waving his hand aloft in repeating the two last lines—we might characterise it as very profanation.

ing organism that re-act on the spiritual principle, this comedy of disease which lays bare all the weaknesses of man, just in the same manner as the 'comedy of death' in olden times sought to show the vanity of life and of its pomps—it cannot be doubted, we say, but that such instructions daily repeated, and this too not in vain fictions, but in the positive language of actual truths, must tend largely to extinguish that spirit of enthusiasm, that sentiment of the beautiful, which seizes the ideal under the changing forms of sensible reality. If the physician do not guard against such influences as these, by occasionally meditating upon the principles of a philosophy which teaches him to perceive the dignity of human nature, under the mists and clouds that conceal it, he will fail to feel for his brother that tender sympathy which so powerfully seconds the action of the plant of science, and assures its healing virtues against every suffering. The man, who will enter upon the study of medical pursuits without having this antidote, this precious preservative in his head and in his heart, may indeed be able to conquer all the difficulties of the science, but he will soon find the most noble instincts of his soul begin to decay; and, as all the faculties are bound together and give mutual support to each other, the intellect itself will in the long run be crippled in its powers. One of the most constant effects of these studies, pursued without precaution, is to engender a spirit of impudent scepticism; and, along with this vice, there is too often an indulgence in language that is marked not only by levity, but by coarse and disgusting indecency."

After such remarks as these, the reader will not be surprised to hear that Dr. Simon advocates strongly the necessity of a liberal and enlightened education to the young physician, before he enters upon the study of medical pursuits. The importance of this is apt, in the present day, to be too much merged and lost sight of in the unbounded attention that is paid to all the intricacies and minute details of physical and chemical science. The mind should be trained to habits of calm and comprehensive reflection, and taught to remember that the investigation of living and sentient bodies, acted upon too by the various emotions of mind, is not to be pursued upon exactly the same plan that is applicable to the study of brute matter. There is much sound truth in the following estimate of the inductive method of enquiry, applied to medical pursuits.

"The Baconian philosophy—which, by making observation, the observation of phenomena, the only legitimate method applicable to the study of science, has tended so much to foster among medical men the education of the special sensibility of the senses, and has rendered them so adroit in the examination of the symptoms of disease—has doubtless had an injurious effect on the development of the higher faculties of thought and reflection, reducing them often to a sort of scientific incapacity. Although, long before Bacon had invented his method, it is easy to discover a certain number of learned men, and more especially of physicians, who had followed it out in their enquiries, there cannot be a reasonable doubt that the illustrious Chancellor of England, by first clearly laying down the rules for its application, powerfully contributed to popularise it, and that he thereby conferred a very signal benefit on all the physical sciences. Medicine, like the rest, hastened to avail itself of the new method, and to apply its maxims to the investigation of the phenomena of living bodies. And none will deny that, under the guidance of this philosophy, many obscure and perplexing questions have been solved, and that, although men of narrow understanding are continually abusing it by erecting it as the only *organ* of science, (to use an expression of Bacon himself,) much, very much, may yet be achieved by strictly following the laws which it inculcates. We need only point to the recently-opened fields of Organic Chemistry and Microscopic Anatomy as two

departments of scientific research, from the careful pursuit of which great benefits may be expected to flow.

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"But even without departing from the circle in which the Baconian method confines itself, and exclusively upon the domain of practice, with what accuracy of observation and with what sagacity of comprehension must the medical man be endowed, to seize the sense of every phenomenon on the living tablet of disease, to distinguish the reaction that is sympathetic from the primary morbid movement, and to recognise, under the disguise of different symptoms, the identity of a morbid affection, or to detect the radical difference of the disease under identical symptomatic appearances! It is obvious that it will require much severe mental discipline, before any one can reasonably hope to attain to such intellectual sagacity."

A very common error, among young medical men, is a tendency to exaggerate the truth of the doctrines, and the power of the art of medicine. Hence the boldness and often dangerous temerity of their practice. How is this evil to be counteracted? Hear what M. Simon says:—"A profound study of the traditions of science, a serious reflection on the causes of the decline and decay of doctrinal systems, which at one time had been received with an almost fanatic enthusiasm, and, above all, a most attentive observation and study of the marvellous resources of Nature to liberate the body from the influence of disease; such are the chief sources from which they will draw that circumspection of conduct, and that prudence in determination, without which the most useful art may become the instrument of irremediable mischief. * * * Prudence is the moral that every well-disciplined mind will draw both from the thoughtful history of the vicissitudes of scientific doctrines, and from the daily lessons of experience; it is the corrective alike of the uncertainties of science, and of the limited capacities of him who applies it to practice."

There cannot be a more dangerous guide in the practice of our profession than in the axiom, *melius est progredi per tenebras quam sistere gradum.* The very reverse ought to be the lesson that is taught. Baglivi, with his accustomed wisdom, advises the physician *multa scire, pauca agere.* Valesius remarks, with almost sarcastic force: *periculosius incidere in medicum qui nesciat quiescere, quàm qui nesciat contraria adhibere.* Morgagni uses nearly the same words: *plures sunt medici qui ob id ægros interimunt, quòd nesciunt ipsi quiescere;* and Hoffmann says that on many occasions, *optimum remedium est nullo uti remedio.* Alas! there are too many in the present day who seem not to appreciate the wisdom of these sentences; men of such extravagant activity that they would take a cudgel to kill a fly! How different has been the conduct of all the best authorities in our profession! Sydenham expressly says: "not only in hysterical affections, but in all diseases, when I do not feel confident that I can do good by my remedies, I think it my wisest part to do nothing at all." In many other passages of his writings, he gives utterance to the some sentiment. Stoll, and indeed every enlightened follower of the Hippocratic school, has followed the same rule.

"In practice they never forgot this inflexible moral precept, *saltem non nocere*—an aphorism, which was inculcated two thousand years ago, by the Coan sage, in words which it cannot be out of place to recall here: περι τα νουσηματα, δυω: ωφελησιν, ἡ μη βλαπτειν, *i. e.* concerning diseases, there are two things: do good, or

do no harm. Listen also to the comments of Galen upon this point: it is such an important question that we cannot accumulate too many authorities to throw light upon it. 'There was a time,' says the illustrious physician of Pergamos, 'when I regarded this precept as of little importance, and as unworthy of Hippocrates. It appeared to me evident that the duty of the physician was to try by all means to relieve his patient. But, after having seen many celebrated physicians justly blamed for the treatment they had pursued, whether by bleeding, or by ordering baths, purgatives, wine or cold water (which became injurious), I then understood that Hippocrates, like many other practitioners of his time, had experienced similar mistakes, and that I should henceforth take all measures, when required to prescribe an important remedy, to calculate beforehand not only what relief the patient might derive if this remedy effected its end, but what harm he might sustain, if it failed. I have never, therefore, administered any thing, without having taken care not to injure the patient, if I failed in my object. Some physicians, like those who throw the dice, prescribe remedies which are very prejudicial to the patients, if they fail in their object. Those who are beginning the study of our art, will think, I know, as I also thought, that the above precept is not worthy of Hippocrates; but experienced practitioners, I am quite sure, will comprehend all its importance; and, if ever they should happen to injure their patients by the unskilful administration of some powerful remedy, then, above all, will they feel the value of the counsel which Hippocrates has left them.' The precept of Hippocrates, and the reflection of Galen, adds M. Daremberg, who has translated this passage, will find more than one application in our days; there are, unhappily, many physicians, to whom the patient is but a subject of experiment, for which science is the pretext, but whose real end is too often only personal interest."

We have already seen how earnestly our benevolent author inculcates upon medical men the paramount duty of cultivating a spirit of kindness and charity towards all with whom they are brought in contact. We shall make no apology for introducing some of his reflections to the notice of our readers.

"While the rich man finds always around him friendly hearts whose sympathy alleviates his sufferings, to the poor man the physician is often the only person before whom he can pour out his grief. Let him at least believe that its expression reaches a heart which understands it. Even hospital physicians owe this token of sympathy to the unfortunate beings whom public charity commits to their care. We are not ignorant that the very considerable number of patients under their charge places them under the cruel necessity of giving to each too limited a time, for it to be devoted to other attentions than those which the chief object of art rigorously imposes upon them. When a legitimate scientific curiosity leads them to be a little less sparing of their time for some of these unfortunate beings, let them at least lend a benevolent attention to the emphatic eloquence of suffering. This duty is the more imperative, as it is the only means of concealing from their eyes that scientific curiosity, to which they scarcely ever submit but with repugnance. 'It is thus,' says Vicq d'Azyr, 'that Lorry became the very comforter of those unfortunates who, generally without parents, without friends, are disposed to receive even curiosity for interest, when it is accompanied by commiseration.' Yes, mingle some expressions of sympathy with the questions you address to them in the abstract interest of science: you will easily deceive them, and may not a little console them. That is so easy, and may do so much good to these poor creatures, whose sufferings even become a source of instruction, which science turns to the advantage of the more fortunate part of mankind.

"Out of the hospitals, the sacred mission of distributing the aids of science

to the poor devolves upon the young physician. If they should draw from thence their first lessons of experience, who does not see that this intercourse with the unfortunate becomes, at the same time, a touching initiation in the virtues which they ought to practise?* There they may learn pity, disinterestedness, devotedness, in a word, all the virtues which they should bring to the practice of their benevolent art. May this precious education not be fruitless to them; may it develop in their souls that feeling of respect and tender commiseration which they should ever bear towards misfortune! Fothergill, after he had become one of the most successful physicians of England, never forgot amidst the palaces of the aristocracy, those unhappy beings who had opened to him the path of fame. His heart reverted towards them, as to the source from whence he had drawn those qualities which had secured his prosperity. How many physicians, after the example of Hecquet, Andry, and others, have devoted the last years of their life to the treatment of the poor! thus showing us that the sweetest recompense the physician can obtain is the sentiment left in the soul by attention to misfortune.

Many indeed are the cases which baffle all the resources of professional skill, and in which it is the painful—but let it not be said, the useless or unprofitable—part of the medical man, day by day, to watch the slow but sure and inevitable advances of a fatal, and it may be a painful, disease. It is the contemplation of such cases as these that has suggested the following beautiful reflections:

"It is above all when the physician encounters one of those numerous affections which make the *desespoir* of the art, one of those chronic diseases to which he can only oppose a palliative treatment, that it is required of him to supply the insufficiency of science by all the resources of an ingenious charity. Quintillian who sought to impugn the utility of the medical art by arguments borrowed from the doctrine of fatalism, says, in some part of his writings: '*Fato vivimus, languemus, morimur: medicina quid præstas, nisi juxtà te nemo desperet?*' We accept this judgment in part. Yes, even when the disease has passed the limits within which active medicine is obliged to confine itself, the physician may still be eminently useful to the unhappy beings whom the incurability of disease condemns to certain death. His kind and affectionate words will find, even to the close of the scene, the way that leads to the anguished heart; they will cherish hope; and perhaps even revive, in some cases, the lamp of life which was nearly extinguished. *Animi consolatio, quâcumque causâ fiat, aperit meatus et largam*

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* In the following sentence, the very same illustration that was applied by Mr. Travers, if we mistake not, to the character of the late Sir Astley Cooper (*vide* his life by Mr. B. Cooper) as a hospital surgeon, is made use of by our author:

"The habitual spectacle of pain will be apt to extinguish, in the heart of a materialist physician, the tender sympathy which the sight of suffering so naturally kindles in the bosom of others, and to lead him by degrees to look upon his patients only *as the florist looks upon a bed of tulips.*"

Such conduct will have its reward. The time will come when this botanic sort of interest shall cease; and then the mind, if it has no other inward satisfaction save that arising from successful ambition, will waste its waning powers, as old age creeps on, in disappointment and ennui. Far be it from us to accuse the departed of materialism and infidelity. Still we much fear that neither our distinguished countryman, nor his equally celebrated continental rival of the French metropolis, was actuated by the pure and lofty principles inculcated by our author. Both died disappointed men.

perspirationem facit. There is not a single physician, we are certain, however inattentive he may be to the progress of chronic affections, who has not had to observe the soothing influence exercised upon the general state of invalids by the disposition of mind alluded to by the celebrated professor of Padua in the aphorism we have just quoted. But it is above all the physician who can handle with success the instrument of this moral treatment; it is above all his friendly sympathising speech, which can shed hope upon this troubled soul. How much then must it be his imperative duty to bring to the practice of the art those moral qualities which ought, if I may so speak, enlarge and extend its limits."

That the inordinate and almost exclusive attention, which has, for the last forty years more especially, been paid to the physical effects of—or, in other words, the anatomical lesions left by—disease, has tended to materialise the intellectual character of medical men, we have already had occasion to maintain. That it is apt, at the same time, to deaden the sensibility, and check the outgoings of sympathy from the heart, will be admitted by many as well as by M. Simon.

"It is more especially since the study of Pathological Anatomy has introduced its important data into the science of Medicine, that the compassion of the physician for the sufferings of human nature has ceased to be perceived in works descriptive of disease, and that it no longer gives a colouring to the sad details of suffering and death. There, the heart never breathes any warmth into science; there, sentiment never beams upon the dark picture of the miseries of life. Whether it treats of a disease which, like pulmonary phthisis, seems to select its victims from among those into whose minds the hand of God has poured his most precious gifts, or of that terrible affection which strikes the mother in the midst of the sweet delights of maternity, and deprives the infant at its birth of that heart in which it should pass the first years of the second phasis of its life, as the first was passed in the cradle of flesh where it was conceived; in a word, in diseases too numerous to be named, where art can only deplore its impotency, as well as in those in which it displays all its efficacy, there is nothing which proceeds from the heart, there is nothing which evidences the sympathy of the physician for the sufferings of humanity; it would seem as if this humanity were become simply an object of natural history;—*absentem, marmoreamve putes.* There are even some of these works where not only you would search in vain for a reflection of that human sympathy which ought so naturally to arise in the heart of the physician, but where, in place of this sentiment, you will observe a kind of ill-dissembled joy, in the prospect of the fatal event which gives the opportunity of verifying an uncertain diagnosis."

Alas! is there not too much truth in the bitter reproof which these last words convey? It cannot be denied. The satisfaction at finding one's professional predictions confirmed by dissection, too often, we fear, sears the heart against any tender emotions which might otherwise have arisen.

We are glad to find that Dr. Simon, while he does not absolutely condemn the performance of experiments upon living animals, denounces with great force and truth that revolting spirit of reckless disregard of suffering which has characterised not a few of the physiologists of the present day. Even he however is, we think, far too tender to such men as Majendie and Flourens, in excusing these barbarities on the score of the "remarquables enseignements" which they have taught us. We attach infinitely less value to the vaunted discoveries of these experimenters than he and perhaps most medical men seem willing to admit; and for this very simple reason: have they taught us better than we knew before how to prevent

and cure disease, or to mitigate suffering, or to prolong and sweeten life? Oh no; but then they have solved some perplexing physiological problem, or corrected some prevailing physiological error, or established some grand physiological doctrine! Truly has our author said that, "before any one can acquire that special aptitude to discover truth amidst the proteiform reactions of struggling sensibility, one needs to have the placidity of a butcher, or of one of the knackers of Montfaucon."

Even to the dead relics of humanity, it is right and proper that some show of respect, some degree of religious decorum, be paid. It is quite possible to associate this sentiment of regard with the exigencies of science. There is no doubt but that he, who thinks of the change which that pale corpse is yet destined to undergo, will cease to view it as mere brute matter to be experimented upon. When a more generous philanthropy shall animate the zeal of medical men in behalf of their suffering brethren, they will be naturally led to view their mortal remains with a feeling almost akin to religious respect. Might it not be well to promote and encourage this feeling by inscribing on the walls of the dead-house and dissecting-room a moral sentence, a hallowed text here and there, to recall the frivolous or irreverent mind to thoughtful meditation? Think you that science would lose aught of what might be truly useful, if its pursuit was blended with an occasional solemn reflection? No, it would only become more sanctified and ennobled. A remark of Victor Hugo may be justly applied to this subject. "Through all things," says this distinguished writer, "see that a moral and sympathetic thought is made to circulate, and then there will be nothing deformed or repulsive. Associate a religious idea with an object the most hideous, and straightway it will become pure and holy. Affix God to a gibbet, and you have the cross."

It would have given us great pleasure to have followed our author in the examination of many of the other chapters of his work. Our object, however, has been rather to give our readers an idea of its general tone and bearings, than to analyse it in detail. It might, indeed, have been not without use in the present day, when there is so much laxity of professional decorum if not of professional principle also, to have considered some of the special duties of medical men to one another, as well as to the public. Want of space, however, entirely precludes us from doing so now; but we regret this omission the less, as the great object in every enquiry upon ethics and morality should be to trace them upwards to the source from whence they flow, the only sure foundation on which they can be built—Religion. If the spring be pure, the stream will be pure also; make the tree good, and its fruit will be good also.

There is, however, one chapter to which, as treating of our own gentle craft,—"De la critique en medecine"—we must afford a brief and passing notice. A single extract will suffice:—

"If all medical writers were influenced solely by the love of truth and by the sincere desire to benefit their fellow-creatures, the task of the critic would be alike an easy and a pleasant one. But unfortunately such is not the case, and not a month passes over our heads that we do not meet with some lamentable instance or another of medical works, that are the offspring of very different motives. Such publications, as we are now alluding to, might be passed over, in other departments of literature or science, as utterly contemptible and unworthy of notice. In medicine, however, it is otherwise; for even the meanest

of them is evidence in itself of suffering and mischief that have already been inflicted. Is it not therefore to be desired by all who have the good of their profession, as well as the public advantage, at heart, that there should be some controuling power to check the spread and counteract the influence of the many flimsy and misleading works that are continually issuing from the press? The difficulty of correct observation, the natural tendency of the mind to find out between facts a relation which an unprejudiced person will utterly fail to perceive, and the constant operation of those active vital forces which are invariably involved in the production of disease, and which continually serve to disguise the influence of the curative means employed—such are (some of) the principal causes which render it necessary to have the controul of the critic upon medical works. Some one will probably say, how is he (the critic) to judge of the value of many new proposals, before he has had an opportunity of testing them by actual experience? The argument is more specious than solid. How many *new* proposals are but the iteration of what has already been tried, and found wanting! And how often do the very facts adduced in support of their claims but serve to disclose most convincingly their fallacy and worthlessness! It is, moreover, always to be borne in mind, that when we talk of experience in medical matters, the trial has to be made upon a human being, who applies to us for relief, and is not to be regarded as brute matter on which we may experiment at our will."

Whatever be the subject under discussion, M. Simon treats it with high and elevated views. Every thing appertaining to the life of man ought, in his opinion, to have an air of seriousness cast around it. We think that, in the main, he is quite right. The medicine of the present day has too little spirituality in it; it savours too much of materialism. It is dissociated from some of its highest privileges, from its noblest ambition. It has mixed itself up with the mercantile spirit of the age; and its aims and ends have necessarily become secular and worldly. As long as this remains the character of our profession, it can never occupy the position in the social scale which it ought to do. It will be regarded as a highly useful art, but not as a dignified and dignifying science.

If medical men think fit to accept the eulogy of the Roman orator (quoted at the beginning of this article) upon their profession, let them remember that just in proportion to its claims to dignity and respect, so is the amount of its responsibilities and duties. Let them not suppose that these are summed up in the skilful administration of remedies, or in the dexterous performance of operations. Let them recollect that the being whom they undertake to relieve, has susceptible moral feelings and a thinking, reasoning soul, as well as a sentient and impressionable body; and that that body and that soul are so intimately, although mysteriously, bound together, and so mutually dependent upon each other in this state of existence, that the one cannot suffer without the other sympathising and being acted upon. Let them think of the fact that many corporeal diseases are aggravated, or even induced, by a feverish or perturbed state of mind; while, on the other hand, some of the sorest ills that flesh is heir to, are more soothed by patient resignation and animating hope than by "all the drowsy syrups of the world." Let them bear in mind that every affliction, bodily and mental, is no blind accident or chance event; but has been sent in mercy, and is designed for some gracious end, and may therefore become the instrument either of peace and happiness or of misery and condemnation to him who is tried thereby; and that every sufferer

whom they visit is hastening on to another world, and that every death which they behold, while it closes the pilgrimage on earth, is the commencement either of eternal joy or of everlasting woe. When they think of these solemn things, and remember at the same time that to all are the offers of peace and acceptance made; that all are invited to partake of the heavenly blessing; that there is still a fountain open whose waters have the power to heal, and that still a virtue goes out from Him, the very touch of whose garment sufficed to give health and vigour to her who had spent all her substance upon the physicians in vain—shall they who have taken upon themselves the sacred mission of ministering to the sick, not have one word of comfort or of kind admonition to offer to the poor sufferer, trembling on the very verge of dissolution, and who may have no other ear to listen to his prayers and sorrows save that of his physician? Do they not know how much the heart is softened by sickness, and how often the spirit, which has been hitherto thoughtless or insensate, is subdued under the chastisement of pain?—that the ground thus prepared, is ready for the good seed, if dropped into it; that a word in season may become the message of glad tidings to the soul; that a blessing is promised alike to him who gives and to him who receives; that the apprehension of alarming the fears, and thus of aggravating the danger of an existing disease, can never be a justification for neglecting so hallowed a duty; that, if the opportunity be lost, the hour of repentance and consolation may be for ever gone: for as the tree falleth, so must it lie; as death leaves the man, eternity shall find him. It is impossible for the physician to shut out these thoughts entirely from his mind; they will obtrude and force themselves upon his reflection, whether he will or not. A death-bed is too solemn a sight even to him, not to stir up, every now and then, a train of solemn meditation. Let him not wish to stifle or evade its appeal to his heart; let him rather seek to profit by the lesson which it conveys. We have been told that—

"An undevout astronomer is mad:"

What shall we say of an irreligious physician? The one has but to do with the marvels of the material universe, great and glorious as this is; the other is called upon not only to behold the fearful and wonderful mechanism of living sentient nature, but also to deal with the welfare and happiness of immortal men.

I. The Brain and its Physiology; a Critical Disquisition on the Methods of determining the Relations subsisting between the Structure and Functions of the Encephalon. By *Daniel Noble*, Member of the Royal College of Surgeons of England. Octavo, pp. 450. London: John Churchill, 1846.

II. Examen de la Phrenologie. Par *P. Flourens*, Secrétaire perpetuel de l'Académie Royale des Sciences (Institut de France) &c. Paris, 1842. Pp. 115.

III. Quelques Considerations en Reponse a l'Examen de la Phrenologie de M. le Professeur *P. Flourens*. Par *M. S. de Wolkoff*. Baden, 1846.

It is scarcely necessary to observe that of all the departments belonging to the science of organization, that relating to cerebral anatomy and physiology is the most involved and obscure. Even setting aside all the unprofitable speculations as to the ultimate relations existing between mind and matter, an enquiry that embraces the mental faculties of men and animals; that regards the respective shares of the mind and of the external senses in the production of sensation; and that attempts to define the connexions existing between individual parts of the brain and individual powers of the mind, such an enquiry must be sufficiently difficult in its prosecution, and still more so in its successful solution. We do not therefore desire to imply, that there are any difficulties in this investigation offering an insuperable obstacle to the attainment of as complete a knowledge of the actions of the brain as of any other organ in the body; on the contrary, the progress made during the present century has not only added a vast number of individual facts to the pre-existing stock, but, which is infinitely more important, it has, by disclosing the clue to the true anatomy of the nervous system, laid the sure and certain foundation for the discovery of the whole of its physiology. This clue we hold to be the fact that the brain and spinal cord have a *fibrous structure*, a truth for which science is essentially indebted to Gall.

"The most accredited method," says Cuvier, in the Report to the Institute of France, speaking of the ordinary mode of dissecting the brain, "the most accredited method of the schools, is to take away successive slices of the organ, and to remark the appearances offered by each. This is the easiest in practice for the demonstration, but it is the most difficult for the imagination: the true relations of parts, which are always seen cut across, escape not the pupil alone, but the master himself." In allusion to the merits of Gall, M. Flourens, in his recent brochure, justly observes, "Gall was a great anatomist. The idea which he had of following and tracing the fibres of the brain is, for the anatomy of this organ, the fundamental idea."—*Examen de la Phrénologie*, p. 102.

It is true, indeed, that both of these writers attempt to deprive Gall of the renown attaching to the founder of cerebral anatomy; but a careful

review of the whole question induces us deliberately to affirm, that to the German philosopher belongs the high merit of rescuing the anatomy of the brain from a state, in which it was impossible, by any talents or by any labour, to have discovered a single fundamental principle. In making this assertion, we are neither unmindful that the fibrous structure of the cerebrum was more or less known to Varolius, Leeuwenhoek, Vieussens, and others; nor that some of those anatomists insisted in their writings on the importance of tracing the relations of the several parts of the encephalon by following out the fibres. In the case of Vieussens particularly, although it is evident, from his having principally examined the brain by means of sections from above downwards, that he was not aware of the true method, it is remarkable that an anatomist who had so successfully traced the fibrous texture, as is indicated by several of his plates, especially the sixteenth, should neither have himself perceived the vast importance of his mode of procedure, nor yet have laid the true foundation for his immediate successors. Without enquiring how it came to pass that the interesting work of Vieussens (*Neurographia Universalis*) fell into the sort of discredit noticed by Cuvier, we would observe that fragments like those just pointed out, have almost invariably preceded in every branch of human investigation, the establishment of great principles; but they have never prevented posterity awarding to him, who, by his genius, his sagacity, and his industry, indelibly traces out a leading truth in science, the palm due to the discoverer, and so often withheld by envious cotemporaries."*

It may perhaps at first appear that the importance here assigned to the researches of Gall, is incompatible with the high claims of the late Sir C. Bell, as advocated in our article on the nervous system contained in a former number of this Journal—(*See Medico-Chir. Review, July* 1845.) Entirely abiding by the views previously announced, it would not, however, be difficult to show that, although our distinguished compatriot might have made out the respective offices of the roots of the spinal nerves, he could not have enunciated, or, at all events, have demonstrated the truth of those great laws which we regard as the fundamental basis of all neurology, had not the fibrous organization of the white substance been predetermined. How would it have been possible, for example, without such knowledge, to have established the all-important principles set forth in the following passages contained in the profoundly philosophic "Exposition of the Natural System of the Nerves of the Human Body?"

"The key to the system will be found in the simple proposition, that each

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* It would be an injustice to pass by unnoticed the admirable researches of Reil into the fibrous structure of the brain; nothing can exceed the accuracy and clearness of his representations taken from dissections of the parts hardened by alcohol, and which have served as a guide for most subsequent investigations. Reil's Essays first appeared in the 8th and 9th volumes of the Archiv. für die Physiologie (1807–8), published by him and Autenreith; but it is essential to state that Reil mentions he commenced this series of enquiries as early as 1795, about the time when Gall, after a constant study of 20 years, delivered his first course of lectures at Vienna.

filament or track of nervous matter has its peculiar endowment, independently of the others which are bound up along with it; and that it continues to have the same endowment throughout its whole length. If we select a filament of a nerve, (for example one of those in the compound nerve represented above,) and if its office be to convey sensation, that power shall belong to it in all its course wherever it can be traced; and wherever, in the whole course of that filament, whether it be in the foot, leg, thigh, spine, or brain, it may be bruised, or pricked, or injured in any way, sensation and not motion will result; and the perception arising from the impression will be referred to that part of the skin where the remote extremity of the filament is distributed."—P. 11.

"If we were successfully to trace a nervous cord, (we shall suppose from a muscle of the fore-arm,) it would be found a simple filament, thread, or funiculus. We should then trace it into a compound nerve; perhaps the ulnar nerve; which we call compound, because there are in it filaments of motion and filaments of sensation bound together. At the root of the axillary nerve we should trace it into the composition of a fascis, where it forms the anterior root of the spinal nerve. Being further traced, it would merge in the anterior column of the spinal marrow; and traced into the base of the brain, it might be followed as a *tractus*, a streak of matter distinguishable from the surrounding substance, until it was seen to disperse and lose itself in the cineritious matter of the cerebrum. In all this extent, however combined or bound up, it constitutes one organ, and ministers to one function the direction of the activity of a muscle of the hand or finger."—*L. c.* p. 14, *Edition* 1824.

Reflecting on these splendid truths, and then turning to Gall's beautiful representations of the course of the ascending fibres from the pyramids of the oblong medulla, through the pons varolii, crura cerebri, thalamus, and striated body, to their radiation and passage into the cerebral convolutions, all becomes significant, precise and clear. Does not the physiologist see at a glance in those invaluable plates the tracts by which, as through so many distinct paths, the mandates of the will are transmitted from the organ of volition, the cerebral hemispheres to the organs of motion, the muscles? Does he not follow, with as much facility as in regarding the wires of a galvanic trough, the conductor by which impressions made on the skin are carried to the organ of perception, the convolutions of the brain? Or, if the pathologist desires to know in what way it happens that a clot of blood effused in or near the optic thalamus or corpus striatum induces paralysis on the opposite side of the body, how clearly is that otherwise inexplicable phenomenon understood, by observing the decussation of the pyramids, and the radiating course of their fibres? We would even go further, and contend that some, and as it might seem, quite independent doctrines, if they have not sprung from the dissections of Gall, have at all events received from them some of the most satisfactory parts of the evidence in virtue of which they are assuming the character of fundamental truths. Of this number we conceive, are, for instance, the theories that the cerebral convolutions are the exclusive seat of intelligence —that the spinal system extends beyond the pons varolii—and that the gray, and not the white substance of the hemispheres, is the source of power.

We have been anxious on several accounts to place the anatomical researches of Gall in that pre-eminent point of view, to which we feel satisfied they are so well entitled. One of these reasons is that, as we shall be compelled in the course of the present article to dissent from the peculiar

physiological doctrines of that distinguished man, we were anxious to show that no indisposition to receive information from their author had influenced our judgment. But another, and much more powerful motive, has been the conviction that the majority of physiologists do not appear to estimate at their real worth Gall's anatomical labours, nor even yet to perceive that all consistent and philosophical acquaintance with the cerebral actions, must repose on that method of dissecting the brain which he was the first firmly to establish. That these assertions are well founded would be made apparent by referring to the original report of Cuvier, and to the reception the new anatomy of the brain experienced for so long time in this country; and that it is still necessary to vindicate this as the only true system, is evident from the expression of a physiologist of deservedly high reputation, especially in reference to the brain, M. Flourens, to the effect that, "the anatomy of the memoir of Gall is only a very ordinary anatomy." (Examen, p. 54.) that there are several errors in Gall's anatomical doctrines we are most ready to admit; but the existing state of knowledge would easily have enabled M. Flourens to point out these without passing such a sweeping condemnation on the whole system, which is even inconsistent with the praise contained in the passage previously extracted.

We have in the foregoing remarks anticipated in some degree the principal object of the present article, which is to consider what are the means best adapted for the successful cultivation of cerebral physiology. This task has been undertaken by Mr. Noble in his "Critical Disquisition;" and we are bound to say he has displayed considerable literary research, much judicious observation, and a desire to elucidate the truth. A work of this character, being a review of the labours of other men, is not indeed likely to contain much novelty; we are not, however, on that account inclined to quarrel with the author, because such an enquiry, when conducted in an enlightened spirit, is well calculated to render good service in the cause of science. Mr. Noble, in his preface, justly condemns the application of exclusive methods of investigation; and has in the following paragraph, shown the evils of such limited views with respect to the brain.

"Each division of natural enquiry is competent to yield its proper results; but when departmental students would deduce from any one branch of knowledge, conclusions that have no direct relation to it, error and confusion will be almost sure to characterise the issue of such attempts. When the vivisector anticipates, from his industry and dexterity, the determination of truths which do not sustain a disclosure by the method employed, he will not fail to be disappointed; and the same may be affirmed of the pathologist and the comparative anatomist. In the many efforts that have been made to shed light upon the functions of particular parts of the Encephalon, the fashion of the age, or the special pursuit, has generally settled the method of investigation; and so, in great measure, the custom continues to the present day. About thirty years ago, mutilations of living animals led to some decisive results in reasearches into the functions of nerves, and renown became justly attached to the experimenters; hereupon, vivisections for a time came into vogue, and every thing doubtful in physiology was to be tested by the fashionable mode. Later on, morbid anatomy became the subject of an all-absorbing interest, and thence was inferred an inexhaustible source of widely-extended discovery; how prematurely so, the world is beginning to see. The attention of some of our leading physiologists has of late years been more particularly directed to microscopic examination of the animal tissues, and to comparative anatomy; and these pursuits, as furnishing the means for

the solution of all doubtful points in physiology, are, in many quarters, coming into transitory fame. All these sources of knowledge are important; they have each most useful ends or aims, beyond which, however, they cannot legitimately advance. Neither the extent to which they are studied, nor the talent and zeal with which they are investigated, can ever determine their adaptation to the discovery of truths to which they are essentially inadequate. The application of this argument to the particular instance of the Brain and its Physiology, constitutes the subject and purpose of the following pages."—P. viii.

In his introductory chapter the author, after pointing out the deep interest and importance attaching to the functions of the brain, enquires how it is that the study of them has been so generally neglected by medical men. The most influential cause he conceives to be "the supposed hopelessness of any enquiry concerning the relations subsisting between mind and matter." In this view of the case we in some degree coincide, believing as we do that the whole question relating to psychical phenomena, as a branch of physiology, has come to be regarded as a profitless enquiry, owing to the fact that a large portion of the investigation has consisted of idle controversies upon the ultimate nature of mind, upon the essential connexion between the mind and the brain, and upon matters of a similar character. It is one of the best signs of the times in which we live, that the most distinguished men in every branch of science, have at length agreed, as by common consent, to investigate phenomena, to trace their relations, and to determine the laws on which they depend, thus obeying the practical impulse of the age, and leaving the dreamers in philosophy to struggle after the unattainable "ultimate facts" of Nature.

To this fundamental cause Mr. Noble would add errors of method; he neither approves of experiment, comparative anatomy, nor pathology as efficient means of investigation, taken singly or collectively; he "believes himself to be in a condition to show that each of these three methods, as leading to the primary fundamental evidence relative to the cerebral functions, is both defective and vicious; defective, by supplying insufficient data at the best: and vicious, by, in many instances, suggesting erroneous conclusions."—*L. c.* p. 13.

Having had some practical acquaintance with each of these modes of investigation, and moreover being convinced that they do in reality constitute, with the addition of a comprehensive knowledge of the human organization, the only means for unravelling this knotty question, we felt some curiosity to know what was the author's plan; and here it is:—"Structure associated with function—magnitude in the development of the former, in connexion with excessive manifestations of the latter—the size of certain parts of the brain, in alliance with corresponding powers of the mind—he believes to be the primary objects of enquiry."—(*L. c.* p. 14.) In other words, Mr. Noble is a thorough advocate for Phrenology, and occupies upwards of three hundred pages, or three-fourths of the entire volume, in illustrating and enforcing the doctrine of Gall. He has, however, given an interesting sketch of all the later researches of physiologists in general, with critiques on their value, and these chapters we proceed to notice.

The author commences his enquiry relating to vivisections with some

remarks which embody an error so fundamental that we must in the outset protest against them. He says—

"The impossibility of gaining additions to our knowledge of the cerebral functions by mere dissection of structure, would appear to be generally conceded. No examination of the intimate constitution of the dead brain could ever do more than exhibit its physical qualities. The most powerful microscope could but reveal its molecular disposition. Nevertheless, a minute anatomical investigation of parts, the functions of which have already been determined, may shed considerable light on the mode in which these are performed; and the sympathies subsisting between one animal function and another, are sometimes beautifully illustrated by demonstration of a correspondence in the organic arrangements. This, however, always presupposes that function is ascertained."—P. 15.

That these are the views generally adopted by physiologists, we entirely doubt; and therefore protest, in the name of the English school, against the proposition, that the anatomical investigation of the brain does not form the basis of *all* inquiries into its functions; to make structure follow function would be totally to reverse the mode of investigation adopted by all the great discoverers in our science, from Harvey to Bell. The latter, in reference to a point immediately connected with this question, says, "my conceptions of this matter (the functions namely of the roots of the nerves) arose by inference from the anatomical structure, so that the few experiments which have been made, were directed only to the verification of the fundamental principles on which the system is founded." And so it ever must be—accurate anatomy, and a comprehensive knowledge of structure, preceding an investigation of function; this will continue to be, as it has hitherto been, the fixed order for arriving at sound physiological induction.

Nor are we much better satisfied with the estimate placed by the author on experiments practised on living animals. That these have been multiplied most unnecessarily, and therefore unjustifiably; that they have been frequently misinterpreted; and that consequently they have often led to conflicting and erroneous inferences, all this we are as willing to admit as Mr. Noble himself; and if he had confined his criticisms to the instance of the brain, there would have been less ground for dissent. But when the great truth established by the celebrated experiments on the roots of the spinal nerves is, for the purpose of throwing discredit on vivisections, spoken of hypothetically (*l. c.* p. 31), we must again protest, and inquire if, from among the crowd of writers on the phrenological doctrine including the masters themselves, Gall and Spurzheim, a single fact can be brought forth from the inferences, assumptions, and probabilities, making up the main part of their evidence, which will for a single instant bear comparison with the fundamental principles that have resulted from Bell's discovery. Even the apparent inconsistencies which the ready tongue of envy attempted to proclaim as proofs of error, such as that irritation of the posterior or sentient roots induced under certain conditions, contractions in the muscles, were only completely explained by means of the vivisections connected with the next great advance in neurology, the discovery, by Dr. Marshall Hall, of the excito-motory power, itself essentially due to experiments akin to those here so much deprecated.

In the Chapter "On Comparative Anatomy as the primary Aid to discovery of the Cerebral Functions," there are some judicious observations well worthy of attention. Among these are the remarks illustrative of the important law now generally recognised that, *size or amount of nervous tissue, constitutes a direct element of functional power*—a law which, as the author observes, brings the brain and nervous system into harmony with all other created things.

"In physics, the law is obvious at once. Size of any given body is always regarded as not the least important property, in an estimate of its power; and, in the vegetable world, productive energy is always qualified by size or amount of structure. In the animal kingdom, independently of the nervous system, the influence of organic size upon energy of function is everywhere admitted. Capacious lungs are more likely to be vigorous in the execution of their office than smaller ones. A large heart will usually propel the circulating fluid with more energy than one that is small; and a like principle holds good with respect to the abdominal viscera; in short, observation has revealed the truth, that the law which regulates the vigour and energy of inorganic matter, of vegetable and general animal life, prevails also in the particular constitution of the brain and nerves. Thus, as a general rule, the sentient nerves bear a proportion in size to the degree in which the power of sensation is possessed. Desmoulins states that, in man, the posterior roots of the spinal nerves, being for sensation, in supplying the superior extremity, have at once an excess of volume and of surface, at least five times greater, both for each individual fibre, and for the bundle resulting from them, than the anterior roots which are appropriated to motion. And this fact corresponds with the exquisite sense of touch possessed by the human hand. The same author further mentions, that the single nerve of feeling ramified on the tactile extremity of the proboscis of an elephant, exceeds in size the united volume of all the muscular nerves of that organ."—P. 38.

It is evident that, to render comparative anatomy of service in determining the respective offices of the different cerebral masses in man, it is indispensably necessary to ascertain what particular parts correspond to each other in the several classes of vertebrate animals. But this pre-requisite is admittedly one of the most difficult points in comparative neurology. "Thus, with respect to the first, or anterior pair of knots of nervous substance, found within the heads of fishes, they are considered by Arasky, Serres, Desmoulins, Carus, and Tiedemann, to be analogous to the cerebral hemispheres in man; whilst Collins, Monro, Camper, Ebel, Treviranus, and Cuvier, regard them merely as connexions of the olfactory nerves. Again, the second cerebral mass is believed, by the last-named physiologists, to constitute the cerebral hemispheres; the former group consider it to form the analogue of the *corpora quadrigemina*." Then, again, if even these points were settled, there remains a greater difficulty, a knowledge of the precise actions, habits, and instincts of the lower animals, or what the author terms, by a new application of the word, their *psychology*.

We shall not follow the author in his somewhat tedious review of the various hypotheses, resting on comparative anatomy, which have been put forth in late years as to the respective seats of the mental powers, because it is certain that at present there is not sufficient evidence to disclose what will in reality constitute the last things to be known in cerebral physiology. One exception may be allowed in reference to the Cranioscopy of Carus, as it has attracted some notice among physiologists and is probably un-

known to many of our readers. The theory is that the encephalon is divisible, both as to function and structure, into three masses, the basis of the arrangement being obtained from the brain of the fish; the anterior consisting of the true cerebral hemispheres, the middle of the corpora quadrigemina, and the posterior of the cerebellum.

"In man, the characteristic feature is the enormous development of the hemispheres. Farther, I have shown that these three cerebral masses, which appear almost in the same relations in the early human embryo as in fishes (that is to say, the middle central mass is the largest), are always to be recognized as endowed with a peculiar function. The posterior cerebral mass is the centre of the primitive fibres of the muscular nerves, and of those of sex. In the middle cerebral portion, the primitive fibres of the reparative organs are collected; while in the anterior cerebral mass essentially, we find the primitive fibres of the organs of sense, through the medium of which we derive our ideas of sensible objects, and in a higher degree our knowledge. In short, the three cerebral masses stand in relation to the following psychological qualities:—

"1. The anterior cerebral mass (or the hemispheres) is related to the power of representing ideas, to that of recognizing and distinguishing them, and to that of imagination.

"2. The middle cerebral mass (corpora quadrigemina) is related to the feeling of the condition of our own organic life (common sensation); and to sentiment, or to the feelings which result from the combined action of all our moral faculties.

"3. The posterior cerebral mass (cerebellum) is related to will, desire, and the instinct of generation.

"As the fundamental elements of mental life are only three—to know, to feel, and to will—so are these three masses the essential portions of the cerebral structure. From these three proceed the three important nerves of sense, those of smell, vision, and hearing, which again correspond to three great regions of the cranial structure, the forehead, the middle head, and the hinder head."—*Critical Disquisition*, p. 55.

The evidence derived from pathology we regard as one of the primary sources of information elucidative of function; not so Mr. Noble, who affirms that these researches, however much they may corroborate truths otherwise obtained, "are, in their very nature, unsuitable as *direct* guides in the prosecution of Cerebral Physiology." So far from this being the fact, some of the most essential, most certain, and, for future progress, most promising facts, *have been proved*, whatever might have been from other investigations surmised, by carefully comparing symptoms during life, with the lesions discovered after death. The general results thus gained are briefly but well stated by Dr. Brigham, an American writer, in the following passage, quoted by Mr. Noble: "First, we have, it appears to me, ascertained from pathological observations, that the functions of the cineritious and the medullary portions of the brain are quite different; that the cineritious portion of the brain is more particularly concerned in intellectual operations, while the office of the medullary part is to conduct sensation and volition; that when the medullary part is alone affected, disturbance of motion ensues, but not of the intellect; and that when the cineritious portion is diseased, the intellect is alone affected."—(*L. c.* p. 85.) Too much importance is, perhaps, here attached to pathology with respect to the light thrown by it on the distinctive actions of the gray and fibrous structures; but every day's experience corroborates the justness of

the other statements. Do we not find that, when the gray convolutions are in any way implicated, as by the pressure of a clot of blood, by an abscess, or by inflammation of the membranes, invariably the intellect is affected, that there is either coma or delirium; whilst in effusion of blood into the substance of the organ, or about the optic thalamus and corpus striatum, and in deep-seated inflammations of a local character, is it not the fact, to borrow the language of Dr. Stokes, that "the most important symptoms are those which are derived from the sympathetic affections of the muscular system," and to which we would add, also, from special deragements of sensation? We speak here of well-marked cases and of permanent results. It is doubtless true that the motor power is often affected in irritation of the surface of the brain, as from spicula of bone in the dura mater introducing convulsion; and also that effusion into the substance may affect the intellect; but the best pathologists are agreed that these phenomena arise from a combination of morbid changes, of which the most essential is a disturbed state of the vascular system within the head. It is the frequency of such general implications which has caused so much difficulty in drawing exact conclusions; but the improved acquaintance with the laws of nervous action that has resulted from the enquiries of the last 30 years, will ultimately enable the physiologist to separate the incidental from the essential, and thus to fix with precision the offices of the various masses of the encephalon.*

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* It is not an unusual thing to hear complaints of the unsatisfactory character of cerebral pathology from others besides the author; not a few practitioners holding all evidence derived from this source, when minuteness and therefore accuracy is attempted, as being, to say the least of it, doubtful or speculative. It is so essential to a more sound knowledge in this interesting class of disease, that confidence should be placed in the morbid anatomy of the brain, that we are induced to insert the following instructive case, related by M. Mahot (Archiv. Gen. de Medicine, 1845). A soldier aged 22 years, was found insensible on Jan. 8th, foaming at the mouth, and with the *left* arm and leg rigidly contracted. Having been restored by dashing cold water on the face, he had a second fit in four hours afterwards. Extreme weakness of the left arm and leg with imperfect motion continued for ten days. Subsequent to an attack of measles, paralysis as to motion, but not of sensation, was observed, on the *left* side of the face, arm, and leg: the intellectual faculties perfect. Pulse 50 to 55. On the 24th of January a fit of insensibility with stertorous breathing; and on the 26th the patient could not turn the *right* eye outwards. Repeated attacks of vomiting and convulsions supervened and continued till April 17th, when the patient died. The post mortem disclosed extensive effusion, &c. but the most interesting fact was the existence of a firm tumour, of the size of a chestnut, seated on the *right* side of the Pons Varolii, close to the median line, pressing on the origin of the 6th or external motor nerve of the eye. With the aid of a comprehensive knowledge of the organization of the cerebro-spinal centre the interpretation of the complex symptoms of this case becomes plain and simple:—the rigid contraction of the arm and leg in the first instance, arose from the irritation of the motor apparatus of the top of the spinal cord by the tumour, which at this period did not produce compression; the paralysis of the left arm and leg resulted from the pressure on the right side of the medium plane of those motorial fibres, which subsequently, in the decussation of the anterior pyramids, get on the left side of the body: the paralysis of the abductor oculi on the right side depended on the

We now approach that which the author, with other phrenologists, believes to be the *true system of cerebral physiology.*

"Development of the brain constantly influences the manifestation of mind; if the quantity of cerebral structure in particular regions be considerable, some related power or quality of the conscious principle will, under ordinary circumstances, be displayed in unwonted energy; and, with great deficiency in volume of brain in the same region, the corresponding power or quality will in every case be but feebly manifested; and not only so, but changes wrought in one set of conditions will often be recognisably induced in the other. Under all these circumstances, the idea of causation becomes irresistible; the induction, that the particular power is organically connected with the particular structure, is philosophically gained."—*L. c.* p. 97.

The evidence of physiology in general is affirmed to support this doctrine, "large size and perfection of structure, associated with great vigour and vivacity in function, modifications in the one corresponding with those in the other—the observation of these things has led to the existence of physiology as a science." But here commences the contrariety of opinion between, we may say, physiologists on one side and the phrenologists on the other; the position last quoted is granted by both parties, but how is it to be interpreted as regards the brain? For it may be said the cerebrum is one organ, the seat of consciousness, intellect, mind, or whatever term may be used to signify the intellectual faculties collectively, and that it enlarges in the ratio of mentality; and this view is as compatible with the principle in question as with that which assigns to the brain a multiplicity of organs, each being the independent seat of a special faculty.

As the ultimate fate of Phrenology cannot, it is certain, be resolved by the method more ordinarily pursued, at least in this country, that namely of comparing the configuration of the head and the relative development of the various convolutions, with the mental habits and aptitudes of the individuals examined, it is desirable to return to the more philosophic procedure of the founder of the system, and to inquire how far it is in harmony with the general laws of physiology. "Gall," says M. Flourens, "argues from the independence of the external senses to the independence of the faculties of the soul; he reasons upon an apparent analogy, which in reality conceals a profound dissimilitude: this is a capital and general error.—(*L. c.* p. 83.) The pupil, and to a certain extent collaborateur, of Gall, Spurzheim, reasons in the same manner: "in the nervous system, says he, we find the five external senses, separate from and independent of, each other; their functions are attached to different organs, and so can exist separately: it is the same with

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compression of the sixth nerve, situated immediately below the tumour: the disturbance of the breathing and the vomiting proceeded from the irritation of the great centre of those excito-motory actions, the medulla oblongata, and to the same cause the convulsions were owing: the intellect was unaffected, because all the permanent mischief was seated below the organ of the mind, the cerebral hemispheres, and these consequently were only occasionally involved from some incidental and transient cause, most probably deranged circulation. The only symptom involved in any obscurity, is the paralysis of the motor root of the left fifth pair: but the precise origin of this is not known, though no physiologist will doubt that it is such as would, if determined, explain the facial palsy.

the internal senses, and we maintain that there is a special organ for each species of feelings and thoughts, as for each kind of external sensation." As the distinguished Secretary of the Institute and now Pair de France, does not show in what the error he denounces consists, we shall take the liberty of supplying, as far as our humble ability will permit, the omission. And we are the more induced to do this, because among thoughtful enquirers, one of the strongest grounds for regarding phrenology favourably has been the belief that the brain was investigated by Gall in strict accordance with the principles of physiology; and that, in attributing a plurality of independent organs to that body, he was adopting precisely the same plan as that recognised by all authorities in respect to the rest of the body.

If, in the above comparison, any inference is attempted to be drawn from the existence of distinct organs of sense, apart from their associated nerves, and this we imagine is the way in which the question has been usually regarded by the advocates of phrenology, a little consideration will show that the things attempted to be compared are totally dissimilar. The existence of distinct sensual apparatus, is a provision made necessary by the different qualities of matter: the apparatus of the eye is essential to the refraction of light, that of the ear to receive and transmit the vibrations of sound, and so on. If this be the comparison signified, what it may be asked can there be in common between mere physical instruments, for such are the mechanical parts of the organs of sense, and the subtle centre of animal life? If, however, it be the *nerves* of sense which are intended, we admit that each of them has its own peculiar endowment, like the nerves of motion and of common feeling, one being capable of transmitting the impression of light, another that of sound; but here again a necessity exists, for it is apparent the several nerves of special sensation, must be diversely endowed to transmit the impression of different physical agents. To have a true basis for the inference attempted, the phrenologists ought to be able to prove, not merely that there are, in obedience to the laws of innervation, differently endowed conductors leading from the organs of sense, but also *different organs of perception*, one for light, another for sound, &c. Now this evidence is not only entirely wanting, but the general conviction among the successful cultivators of physiology is, that there is only one central organ of perception, or sensorium commune, a conviction resting on observation and experiment. It is justly observed by M. Flourens, that when portions of the hemispheres are removed in living animals, no matter from what region, whether in front, behind, above, or on the sides, instead of any one particular faculty being thereby destroyed, the general intelligence is at first weakened, and afterwards, when the ablation passes certain limits, extinguished; or, in other words, the operator does not find, which ought to be the case if Gall's hypothesis were correct, first a loss of sight, then of hearing, then of feeling; but what he does observe is that "so soon as one sensation is lost, all are lost; that so soon as one faculty disappears, all disappear."—*Examen*, p. 23.

The result of this, which may be termed the philosophic mode of treating the question, is then that Gall has compared together things which are essentially different—the brain, the instrument of power, and the nerves, the mere conductors of impressions: as well might it be said that, because

there are in the galvanic telegraph special conducting wires to the several stations, there must also be a distinct battery for each.

Another of those general questions involved in the reception or refutation of Phrenology, is that relating to the nature of the mental powers. "The psychologist," says Dr. Todd, "must determine what are, and what are not, fundamental faculties of the mind, before the physiologist can venture to assign to each its local habitation." Now, although we are willing to grant that the principle which assigns special organs to special faculties may be true, whilst the precise number of the faculties and the exact situation of the organs themselves may be still sub judice; yet, when we come to weigh the evidence pro and con, the objection just quoted on the one hand, and the analysis of the mind advocated by the new school on the other, must enter as important elements into the enquiry. The first thing which challenges scrutiny is the minute subdivision of the primitive powers. Some of these are so closely allied that no unbiassed person would hesitate to attribute them to the same mental quality; whilst others are so insignificant, such as "weight and resistance," that the wonder is how they ever could have found a place among the fundamental "facultés intellectuelles." It is not surprising when refinements so subtle as these are essayed, that there should be discrepancies, even between the leaders of the sect; that, while Gall should admit 28, Spurzheim, by only adding eight new fundamental faculties, should raise the number to 35!

There is, however, much more profound disagreement than this; for Gall, not content with his 28 primitive powers, attributes to each individual faculty—perception, memory, judgment, imagination, will, &c., so that in fact each faculty becomes an individual intelligence complete in itself. Spurzheim dissents from these views, affirming that the facts adduced "do not prove the conclusion." We would go farther and enquire with Flourens, "with all these individual intelligences, what is the general intelligence properly so called? It might be, just as one pleases, either an attribute of each faculty, or the collective expression of all the faculties, or even the simple result of their common and simultaneous action: in a word, it would be no longer this faculty, positive and unique, which we understand, which we conceive, and which we all feel within ourselves, when we pronounce the word, soul or intelligence."—*L. c.* p. 25.*

* It may be thought that the discrepancies noticed above were dependent on the infancy of the science, or even upon the well-known controversy existing between Gall and Spurzheim. We find, however, the same kind of discordant statements still prevailing: thus M. Vimont, "phrenologiste très decidé et anatomiste très habile," according to M. Flourens, and highly lauded by Mr. Noble, affirms that "the work of Gall is more adapted to induce error than to give a just idea of the seat of the organs;" whilst, with reference to Spurzheim, it is contended that he has placed one organ on the frontal sinus, and another upon the muscles inserted in the occiput. It is somewhat remarkable after such critiques that M. Vimont should have inscribed on the cranium of a goose the names of no less than 29 faculties; among which we find, "extent, distance, geometrical sense, resistance, order, time, language, eventuality, construction, musical talent, imitation, comparison, sweetness"! All we can say is, that this must have been indeed a marvellously clever goose, and a worthy rival of the learned pig.

If the general arrangement of Gall be a difficulty to the uninitiated, assuredly his more minute analysis of the inner-dependencies and correlations of the several mental faculties, will not be more satisfactory. Who, for instance, would have thought of seeking in the propinquity or remoteness of the organs of colour, form, and imagination, for an explanation of those mental aptitudes which make or mar the painter? And yet we are told that, "as the organ of the arts is placed far from the organ of the sense of colour, this circumstance explains why the painters of history have been rarely colourists:" this is in truth mapping out the mind and enchaining the ethereal spirit.

Having already devoted so much space to this subject, we can only select one or two of the individual faculties, as examples of the kind of evidence current in the phrenological school. It was the opinion of Gall that the two organs of which the localization was the most surely established, were the cerebellum, the seat of the instinct of propagation, and the posterior cerebral lobes, the organ of the instinct of progenitiveness. In this view Mr. Noble coincides, for he states he "is one of those who think with Georget, that the organic connexion of the sexual instinct with the cerebellum is probably that particular point of the phrenological doctrine in favour of which the largest amount of proof is obtained"—*L. c.*, p. 252. In support of this opinion, several cases are adduced, which have already been published either by Gall himself, or by Dr. Combe and others. In two of these impotence was induced in consequence of blows from fire-arms which had grazed the nape of the neck; in another case a ball which had traversed the muscles of the neck, grazed the inferior occipital swellings, and atrophy of the testes and penis occurred for a time, though in two months all the symptoms disappeared. Two cases are also quoted from Dr. Stokes, in which it was supposed by that able physician that a connexion was exhibited between disease of the cerebellum and visible excitement of the external organs.

Of the major part of this evidence, all we can say is, that no unbiassed physiologist would receive it in proof of the function assigned to the cerebellum. In the three first cases related by Gall, there was no evidence of that organ being specially or even at all implicated; in the first of Dr. Stokes's cases there was not only effusion of blood into the cerebellum, but also in the hemisphere of the cerebrum; and, moreover, paraplegia existed, showing mischief in the spinal cord, to which the priapasm ought doubtless to be attributed. In the injuries inflicted on the nape of the neck, it might at all events, with equal or greater probability be inferred that the peculiar affection of the genital organs was the result of the upper part of the spinal cords being irritated. Many facts might be adduced in support of this view of the case; thus in injury of the spine producing paraplegia, priaplasm is a most frequent occurrence; and the emissio seminis that takes place in hanging, has been attributed, we think justly, by the most sound physiologists, to spinal irritation. But more than this, there is positive proof both of the sexual desire and power existing where the cerebellum could not by possibility exert any influence. In the first place, there is the well-known case related by Andral of a half idiotic girl, 12 years old, who showed a precocious tendency to the passions of her sex, was addicted to masturbation, and in whom it was found

after death that the uterus, fallopian tubes, and ovaries were fully developed, and yet the cerebellum was totally absent. Then, again, Brachet details an instance in which a man perfectly paraplegic became a father; to which may be added the more questionable case recorded in history of Albert the second, Duke of Austria, who was attacked with a paralytic disorder which deprived him of the use of the lower extremities, but who at a time when his increasing disorders seemed to preclude all prospect of issue, became the continuator of his race, and the father of four sons.

When so much difficulty is experienced in assigning the special actions of the cerebellum, a part marked out by recognisable limits, for it should be remembered that, although the phrenologists have settled the matter to their satisfaction, the great majority of physiologists more or less agree with Flourens, that it is connected with locomotion—it is not surprising that, in proceeding to the convolutions of the brain, the obstacles in the way of localizing their actions are infinitely increased. An objection which must with all unprejudiced persons have great weight, is the fact often urged, that there is no natural limitations of the convolutions corresponding with the assumed special cerebral organs; on the contrary, the anfractuosities run continuously, and often to a great extent in the various regions of the hemispheres; so that, as may be seen by contrasting the brain with the various plates of Gall and Spurzheim, figures, indicating several distinct "organs," are placed on different parts of the same continuous convolution. Mr. Noble contends that these objections arise from an "utter ignorance of Nature's plan in the development of the nervous system, *which, as a rule, exhibits no mechanical divisions coincidently with distinctness in function.*"—(*L. c.*, p. 237); and various instances are adduced in corroboration of this position. These supposed proofs, we are compelled to say, are not judiciously chosen, and most of them are moreover erroneous. The first assertion is, that "the trunks of the nerves are composed of filaments in subservience to various functions, and that the several filaments are absolutely undistinguishable in their relation to functions." With the glaring instance of the two distinct roots of the compound spinal nerves before our eyes, this, to say the least of it, is a bold assumption; but we go farther, and affirm that cautious microscopists can detect physical differences both between the nerves of special sense, as the optic and the auditory, and the nerves of motion, and even between these last and the nerves of common sensation. The second assertion is, that there is no reason to believe that the distinct columns marked out by the longitudinal furrows on the spinal cord and medulla oblongata have distinct offices; but, in opposition to this, it would not be difficult to show that the best physiologists think the above fasciculi do minister to separate functions, although these at present are not determined, and in this opinion we entirely coincide. The last reason embodies, quite unintentionally we are satisfied, an anatomical error; the author, in noticing the opinion entertained by many investigators, that the glosso-pharyngeal nerve has more offices than one, adds that this inference is deduced from the supposed manifestations, and not from any mechanical division. Now that which to our mind has always been the most satisfactory proof of the motor as well as sentient office of the nerve in question is the fact, that it

precisely resembles the compound spinal nerves, having two distinct roots with a ganglion (*g. jugulare*, of Müller) on its posterior fasciculus.

A careful consideration of the nervous structure has brought to our mind the fixed conviction, that diversity of function is invariably accompanied by diversity of structure; and this being the case, the question recurs whether the apparent identity of structure and fusion of substance exhibited in the several convolutions of the brain, these being extensively continuous, is compatible with the assigned distinctness of function. We do not wish to affirm that there may not exist intimate and yet essential structural modifications in those bodies undiscoverable at present; all we desire to show is, that up to this time no such organic difference has been detected, and especially that the position taken up by the author is untenable. In reference to the locality of the convolutions there is a difficulty of which we have seen no satisfactory explanation: what is the office, the rôle being already complete without them, of those numerous convolutions placed at the base of the brain, of the insula for example, and on the flat sides of the hemispheres, of which no sufficient account is given, unless, like waifs claimed by the lord of the manor, they are to be thrown to their nearest neighbours? We cannot pursue this branch further, and therefore we must refer our readers to the chapters in which the author endeavours to prove the harmony of Gall's physiology with structural and comparative anatomy.

One of the most important points connected with Phrenology is the great light it is believed by many eminent physicians to throw on the causes, management, and results of mental diseases; and we at once admit that the opinions of these gentlemen are deserving of the most respectful attention. The author, who has two chapters on this subject, is most hopeful of the advantages to be derived from his favourite doctrine.

"Gall's method of observation having proved—what previously was but a well-grounded hypothesis—that the brain is the organ of the mind, it follows that a morbid condition of this structure should tend to the depraved manifestation of the mental phenomena. Phrenology, moreover, having shown that the encephalic mass is a congeries of organs—the office of each being to display an individual faculty of the mind—has afforded a flood of light to illumine the once obscure path pursued in the investigation of mental derangement; and, indeed, it bids fair, if but once generally understood and assiduously cultivated, to dissipate much of the intricacy and mystery that formerly involved this subject of enquiry—for so many ages the opprobrium, not only of medical men, but of the civilized world at large.

"By demonstrating that diseased manifestation of the mental functions is associated with corresponding derangement of the material organ; by affording a reasonable and practical, if not perfectly accurate, analysis of the human mind, in an enumeration of most of its primitive powers and inclinations; by exhibiting the organic connexion between special parts of the brain and determinate mental faculties; by showing that the phenomena of insanity must be regarded as pathological conditions of the brain—phrenology (employing this term in its widest acceptation) offers for the first time a distinct clue to a system of cerebral pathology and cerebral medicine, as experimental in its nature, and as rational, as that which is afforded by other branches of the healing art."—*L. c.* p. 336.

The history, especially of monomania is supposed to receive great elucidation from Gall's physiology; for as there are so many independent

organs, one of these may be morbidly disturbed, and with it the associated functions, leaving the rest of the cerebral organization and actions unaffected. This is doubtless a plausible mode of accounting for the well-known peculiarities of monomania; but so to some persons was the explanation offered by Dr. Wigan of certain forms of mental aberration; and indeed there is nothing in the supposition of two cerebra and a dual mind existing in one individual, more opposed to the common views of mankind, than the doctrine of Gall concerning multiple brains and distinct intelligences. These considerations induce us to request our readers to contrast the past progress of phrenology, with the principles in accordance to which it is now agreed science ought to be cultivated; and to remember that, in the investigation of abstruse natural phenomena, something more than plausibilities are required. Great truths have been invariably established by a few simple and unquestionable facts; so much indeed is this the case that it is justly regarded as a suspicious circumstance, when any theory requires of its advocates a crowd of probabilities and inferences for its support. Now it is precisely in these two respects that phrenology fails in realizing the tests of truth; it has been overlaid with an endless amount of arguments, inductions, and deductions, and is supported scarcely by a fact that is not either questionable or questioned. And yet, considering how long and how eagerly the subject has been prosecuted, the time is come when more positive proofs ought to exist.

Even as regards one of the most promising sources of evidence, morbid anatomy, the results hitherto afforded have been insignificant; and we entirely coincide with those authorities who, to borrow the words of Mr. Noble, have objected, though as he contends unjustly, "that the morbid anatomy of insanity does not furnish that corroborative evidence of the soundness of Gall's physiology of the brain which general physiology receives from the same source." Considering the multiform varieties of insanity—the marked cases so constantly recurring, in which the disturbance is determinately restricted to some peculiar hallucination, all else being sound—the manifold instances in which for a long series of years, certain faculties are permanently deranged, having, it is affirmed, cerebral organs widely separated from each other, and in which, consequently, morbid changes of structure should, by contrast with the healthy surrounding parts, be strikingly apparent; considering all these things and the slight indications of corresponding, that is local, morbid changes detected, the conclusion forced upon the mind is, that the actual evidence falls altogether short of those precise allegations in which phrenology abounds. How often, for example, is it found that where special faculties only have been affected, the morbid changes in the brain and its membranes are general, instead of being local and restricted; this general organic disturbance is, in fact, by far the most common alteration detected, whatever may have been the peculiar form of insanity. It is often asserted in answer to all these objections, that "aberration of function is not *always* succeeded by appreciable change of structure." But we might appeal to pathologists and ask, if, in chronic cases of mental derangement this is not the exception; the rule being, that to the practised eye morbid changes, usually as we have said more or less general, such as opacity and thickening of the arachnoid, effusion of serum, alteration of the gray and fibrous substance,

&c., do not present themselves. The fact is then, that in chronic insanity there is usually the evidence of pathology, but it indicates a general and not a local disease of the encephalon; the proof exists, but it is not of the right kind.

The author observes, "it is a very interesting circumstance to notice, that *all* who have to deal practically and professionally with mind, and who at the same time understand phrenology, recognise the light which it sheds on their pathr."—*L. c.* p. 404. Testimonies from various individuals are then adduced in support of this position from one of Dr. Combe's works. That much benefit has resulted from the systematic study of the mental powers, which has attended the discussions of Gall's theory, and especially from the great attention that has been paid to the broad distinctions so much insisted on as existing between the affective and intellectual faculties, we are quite prepared to grant; and herein the phrenological school has conferred great and lasting benefits on society. But beyond this, we believe little has been gained; at all events we must enter our protest against the author's assertion, that *all* persons engaged in mental training have derived the advantages from phrenology indicated in the above passage.

In the course of this article several allusions have been made to the pamphlet of M. Flourens. We have only to add, that it contains many judicious observations all worthy of consideration; but we agree with M. De Wolkoff, that it is most reprehensible to mix up, as M. Flourens has done, religious doctrines with mere matters of science. As to the "*Reponse*," of M. De Wolkoff, it contains little worthy of remark; this author, however, although he informs us he had great joy, after a painful study of the metaphysicians, when the work of Gall fell into his hands, believing he had at last found the treasure for which he was so long seeking, does not, after all, seem very much satisfied with having, for these twenty years past installed in his library the works of the phrenologist in the place previously devoted to those of the metaphysicians. The following passage would rather indicate that he has yet to seek for that ultimate rest, all engaged in philosophic speculations so earnestly desiderate, a fixed conviction: "in discovering in the human brain a constant disposition of the convolutions, it is not surprising that the phrenologists thought they might well be the seat of the diverse mental faculties. But beyond this it is only a supposition, and, however probable this supposition may appear to be, it would be wrong to regard it as an indubitable truth."—*Quelques Considérations au Réponse*, &c., p. 8.

Before concluding our remarks, it is due to Mr. Noble to state, that although we have, in the discharge of our impartial duty, felt it to be necessary to oppose the physiological views he has adopted, we are most ready to bear our testimony to the talent and extensive information he has evinced; and we cannot for this reason but regret that he had not selected a more promising field for their exercise.

1. DELLE ALTERAZIONI PATOLOGICHE DELLE ARTERIE PER LA LEGATURA ET LA TORSIONE. Esperienze et Osservazioni di *Luigi Porta*, Professore di Clinica Chirurgica nell' I. R. Università di Pavia. Un vol. in 4to. grande, di pag 439, con 13 tavole in rame. Milano, tipografia di Giuseppe Bernardoni di Giovanni, 1845.

Experiments and Observations upon the Pathological Changes produced upon Arteries by the Ligature and by Torsion. By *Louis Porta*, Professor of Clinical Surgery in the University of Pavia. Large 4to. pp. 439, 13 copper-plates. Milan, 1845.

II. ON WOUNDS OF THE ARTERIES OF THE HUMAN BODY, WITH THE TREATMENT AND OPERATIONS REQUIRED FOR THEIR CURE. By *G. J. Guthrie*, F.R.S. 8vo. pp. 97. London, 1846.

IN the remembrance of what has been effected for the investigation and the illustration of the pathology of the arteries and for the determination of the surgical proceedings their diseases and injuries require, by means of the labours of Hunter, Desault, Scarpa, Hodgson, Jones, Bell, Cooper, and last, though not least, Guthrie and the other army surgeons, we might have been disposed to declare that no part of the domain of surgery ought to be so assured as this, and none so securely regulated by fixed and well-established principles and laws. And yet here we have, just fresh from the press, an elaborate experimental treatise upon the effects of ligature and torsion from the pen of Professor Porta, and the report of a short course of lectures upon wounds of the arteries just delivered by Mr. Guthrie, in which he censures with a just severity the retention of modes of procedure in reference to these, which the information the profession has for years been in possession of, ought long since have induced it to discard. We certainly were not aware, prior to the perusal of these Lectures, that sound surgical and anatomical principles were still so much unknown, or so little regarded, even by men of great note, as the instances cited prove to be the case. It says little for the real progress of our art that doctrines which were laid down in 1815, as the results of extensive and diversified experience, and the correctness of which has never been successfully impugned, should require reiteration in 1846, and should receive additional means of illustration from some of the instances of the consequences of neglecting them, which have occurred in the interval.

The treatise of Professor Porta is a very able performance, proving its author to be a worthy successor of the celebrated man who formerly adorned the University of Pavia. But, while testifying to the acute spirit of investigation, the felicity of exposition, and the pains-taking desire to arrive at the truth, which it manifests, we cannot conceal from ourselves the conviction that a considerable portion of it is a work of supererogation. Surely, after the elaborate investigations of Jones, Hodgson, and others, upon the effects of ligatures on the blood-vessels of animals and men, hundreds of additional experiments upon the former were scarcely called for; and indeed this portion must be looked upon rather as an elaborate demonstration or exposition of the whole case of the ligature, (or as

a repetition of such demonstration in a more methodical form,) than as the receptacle of any discovery, or the herald of any improvement. The author arrives at the conclusion that the present universally adopted mode of applying the ligature is the best—a conclusion he could have as well reached had he spent a somewhat larger portion of the nine years over which these investigations have been carried in his library, instead of in the performance of numerous cruel and unsatisfactory experiments. Yes, cruel; for, surely every unnecessary experiment is a cruelty, and many, very many, of these upon the different kinds of ligatures, which universal consent has abandoned the use of, and for the re-ascertainment of points already established, must be so considered. The conclusions, too, derived from the experiments upon the blood-vessels of animals are more unsatisfactory than those which bear reference to any other portions of the economy; these tubes being gifted with very different properties in different brutes, so as to render the reasoning from analogy respecting them an unsafe procedure.

"Although," says Mr. Guthrie, "there is so general a resemblance between the arteries of man and animals, as to render them apparently similar, their structure is not exactly alike. A second cellular coat, for instance, is found between the external and middle tunics in the ox. It has not been practicable to cause an aneurism in dogs, and the apparent similarity of these vessels, with reference to the effects which may result from injury or disease, cannot be depended upon: nor can any confidence be granted to the numerous experiments which have been very cruelly made upon animals with the view of elucidating the various processes which occur in man."

But while protesting against this part of the work as uncalled-for, though ably executed, we admit that other portions relating to the demonstration of the collateral circulation are more original in their anatomical demonstrations, though still leading to no novel conclusion or improvement in practice. The numerous facts cited from his own practice and that of various celebrated surgeons are however valuable; and altogether the work is highly useful to those (as we suppose is the case with many in Italy) who have not access to the other sources of information we have referred to. A brief analysis of its contents may also be interesting to our readers.

In the first Chapter the *Anatomy* of the *Arteries* is treated of. The author, like other anatomists, describing these as consisting of three coats, the cellular, fibrous, and inner. Blood-vessels, which are found abundantly supplying the outer tunic, become smaller and smaller, and are gradually lost as they penetrate the middle one, so that they cannot in any wise be traced upon the innermost.* The nerves and lymphatics do not penetrate beyond the cellular coat. The *external coat* is then an eminently vascular membrane, and pathological changes in man, as well as experiments in brutes, alike prove it to be the seat of the principal vital phenomena and organic metamorphoses which take place in the arteries. The *middle* one resembles tendinous or aponeurotic structure, and seems destined chiefly to give form and resistance to the vessel, and to preserve its calibre without presenting an obstacle to the impelling action of the heart. The

* The is a discrepancy in this statement of the author compared with that of the concluding sentence of the paragraph.

innermost tunic is composed of a fragile, coriaceous pellicle—an arterial epithelium. This epithelium being lined with a serous membrane, is pretty vascular, but it is by no means so prone to take on inflammatory action as represented by some pathologists, although inflammation of the proper tunics of these vessels, which is denied by others, and is really of rare occurrence, may yet undoubtedly take place.

In the Second Chapter the following questions are discussed:—*What becomes of a ligature attached to an artery? Does the material of which it is composed influence the results of the operation? What is the most eligible material?* Four descriptions of ligature were tried in the 300 experiments performed to illustrate these points. Catgut was employed 80 times, silk 120, thread 60, and horse-hair 40 times. The results are detailed at great length, but we need only advert to the general conclusions, viz. that of the 300 ligatures, 64 disappeared in periods varying from one day to two years, and 236 remained: of these last, 29 were found amid a lymphatico-cellular tumefaction, 60 within the intermediate cord connecting the two ends of the artery, 67 enveloped in common cellular tissue, 54 enclosed in a true cyst, and 26 in the midst of suppuration. Our author considers the 64 which disappeared were assimilated (or more correctly speaking absorbed), and found a much larger proportion of the *catgut* ligatures underwent this process than those of any other material. Those results observed in animals differ from those observed in man by the much greater frequency with which the ligature excites the adhesive process in the former by which it is retained, and the suppurative process in the latter by which it is discharged. Signor Porta enquires whether these ligatures, which after amputation have had their ends cut short, as recommended by Lawrence (but seldom put into force by him now we imagine), and as treated by himself both after amputation and the operation of aneurism, and which are sometimes retained after the wound was united and never discharged by the suppuration, do not become dissolved or encysted. It may be so, but seeing how very rarely such a result can be calculated upon, we think so generally an unsuccessful practice should be discountenanced.

In the Third Chapter the author discusses the various modes of applying the ligature and performing torsion, and the pathological changes which are produced upon the arteries by these.

1. *On the Application of the Ligature.*—Four kinds of ligature are described, viz. the common or circular, the temporary, the mediate, and the double.

(A.) *The Circular Ligature.*—With this ligature formed of various materials, the ends being cut off and union sought to be obtained, 140 experiments were tried on various arteries of different animals. From these the ordinarily described mechanical effects, division of the proper tunics, strangulation and approximation of the cellular tunic, and the formation of a coagulum on each side the thread, resulted. The obliteration of the vessel is due to the subsequent inflammation, which is so much more commonly of an adhesive character in animals than in man, that in 140 ex-

periments the ligature was only discharged with suppuration 19 times. In man, a mixed process, termed by the author plastico-suppurative, takes place.

"Operating by whatever mode we will, we can never hope to obtain in surgery the happy results derived from tying arteries in animals; but, judging from analogy, and from known data, I believe that when the ligature is applied in man with greater delicacy than it has hitherto been, choosing the most homogeneous material and the most simple procedure, respecting the cellular matrix of the vessel, cutting off the ends of the ligature, closing up the wound, avoiding the proximity of the aneurism, and enjoining the greatest quietude to the patient, we shall obtain adhesion much oftener; and that when, in spite of our efforts, suppuration does occur, it will be circumscribed, and will not impede the plastic exudation which closes up the mouths of vessels, and prevents those fatal accidents which, so exceedingly rare in animals, have so frequently endangered the life of man."

As Mr. Guthrie's account of the effect of the ligature upon the vessels differs from that usually given, we will here extract it.

"The inner and middle coats are not only divided, but the inner one particularly appears curled inwards on itself, so that the cut-edge of one half or side is not applied to its fellow in the usual way of two surfaces, but by curling inwards meets its opponent on every point of a circle, and in this way forms a barrier inside that of the external coat, which is tied around it by the ligature: so that, in fact, when a small ligature is firmly tied, its direct pressure is not applied to the inner coats, which have been divided, and have curled away from it, but to the outer one, which is in consequence of that pressure made to ulcerate or slough, which processes could scarcely fail to take place also in the other coats, if they were subjected to pressure in a similar manner. The cut-edges, being from this provision of nature perfectly free, are capable of taking on the process of inflammation, which stops at the adhesive stage. This they do by the effusion of lymph or fibrin, both within and without, to a greater or less extent as the case may require. The outer coat of the artery must either yield by ulceration or sloughing, or the ligature must remain until it is decomposed and destroyed. The artery usually yields by sloughing, or the ligature is left at liberty by the ulceration which takes place in the sound part of the artery immediately above and below the part strangulated by the ligature, and which part is frequently brought away in the noose. The artery does not always yield by sloughing, particularly if it is a large one, and the ligature has been thick and soft. In this case, a part of the outer coat, from its folding or plaiting under the ligature, seems to escape that degree of pressure necessary to destroy it, and when the remaining part yields, it remains entire, and is only removed by a subsequent process of ulceration, occasioned by its irritation, as an extraneous body. I have had the opportunity and misfortune of examining great numbers of stumps after amputation and death, and I have seen this occur in so many instances as to leave no doubt of the fact. In these cases, the external cases could not close around the inner ones; and this shows that they are capable of forming an effectual barrier without it, although it materially assists in giving greater strength to the cicatrix by the effusion of fibrin, which takes place within, without, and around.

"Whilst this process is going on without, and at the very extremity of the artery, the vessel is gradually contracted above it, and its coats become more or less inflamed, soft, and vascular. The inner coat is seen to be wrinkled transversely, and a small coagulum of blood is formed within it. This sometimes completely fills the artery, but it is more common for a small tapering coagulum to be formed, adhering by its base to the extremity of the inner coat. My ob-

servations have led me to believe that a coagulum is not absolutely necessary to the permanent closure of an artery, although it certainly assists in maintaining it."

Although an artery is said to contract until the next collateral branch, Mr. Guthrie has met with several cases in which the principal vessel has continued pervious below such. Again, it is an error to suppose that a collateral branch immediately above the ligature always interferes with the closure of the canal. It sometimes may prevent it, and generally delays consolidation. "I have so often seen large arteries heal after division close to the giving off of a considerable branch, that I consider them always capable of doing so if they are naturally sound. The power which suppresses hæmorrhage in a bleeding artery, resides at the very extremity of the vessel itself." *Ligatures* should be round, and as small as consistent with strength. The surgeon should acquire a knowledge of the strength with which they require to be tied on the dead body, and he will generally find that he has estimated the necessary force too highly. "A ligature composed of one strong thread of dentist's silk, well waxed, is sufficiently firm for the largest artery."

(B.) *The Temporary Ligature.*—With this 50 experiments have been performed, leaving it on the vessel for periods varying from a few minutes to several hours. The proper tunics of the vessel are divided, and slight inflammatory action induced. By the third or fourth week the artery has entirely resumed its normal condition, scarcely any thickening of the cellular membrane even remaining. The blood never ceased flowing through the vessel, except in 6 or 7 cases, in which coagula, and in 5 or 6 others, in which lymph, partially or entirely, but temporarily, obstructed its course. These results being so different from those produced by the experiments of Travers and other English writers, 24 other additional experiments were resorted to, in which two, three, or four ligatures were left on the vessels very near to each other for spaces of time varying from 2 to 12 hours. In some of these, coagula obstructing the vessel formed, but in the majority no such obliteration took place.

(C.) The *Mediate Ligature* so warmly advocated by Scarpa, and still employed by M. Roux, was the subject of 120 experiments, in 35 the ligature being permanent and in 85 temporary. The *permanent mediate ligature* (a small waxen cylinder being interposed between the thread and the artery) induced four changes in the vessel: 1, an arteritis of the cellular membrane; 2, an erosion of the proper tunic, which commenced from the 2d to the 7th day, at the point over which the little cylinder was applied; 3, the section of the artery took place sometimes in 60 or 70 hours, but generally not before the commencement of the 2d week. The section was followed by a retraction of the ends of the artery which were divided by an interval of from 2 to 6 lines, and by the separation of the cellular deposit into two portions, so that the extremities of the vessel were only now constituted by the internal coagulum. 4. The separate cicatrization of the two ends of the vessel. In the 85 instances of *temporary mediate ligature*, (the thread being retained on from 1 to 5 days,) in 33 the vessels had undergone no change whatever, and in 21 they were scarcely eroded. In 9 they were eroded and cicatrized, in 16 divided right through, and in 6 reduced to a cellular cord. The vessel was completely obliterated in 61 cases, 34 times

by reason of the presence of a coagulum, 11 by deposition of lymph, and 16 times by the section of the arterial walls. In the other 24 cases the arteries remained permeable. In 7 there was hæmorrhage.

(D.) In 45 cases the *Double* ligature (the thread being applied at a few lines distance from each other) was tried, in 35 of which, simple section of the interqening artery was performed, and in 10 a portion of the vessel of varying length was removed. The mechanical alterations produced in the vessel were the same as in the case of the circular ligature, *plus* the excision of the artery, the ecchymosis and the retraction and coarctation of the ends of the vessel. The subsequent inflammation of the cellular tissue gives rise to a considerable deposit of plastic lymph which is modelled into a cylinder, completing the continuity of the vessel, and being sometimes impervious, and at others containing a cavity filled with sanguinolent serum. It is at first of a larger diameter than the artery it replaces, but gradually atrophies into a thin fibrous cord, and eventually becomes absorbed. If only one or two centimetres be removed from the artery, it comports itself as just described; but, if more be removed, its two ends cicatrize separately. In three cases hæmorrhage was observed, which was never the case in the experiments with the circular ligature.

The author concludes this section by an estimate of these various measures.

"The *temporary* ligature must be looked upon in the light of a simple experiment, destined always to remain fruitless by reason of its inefficiency. The *permanent mediate* ligature, once recommended by prejudice, should be consigned to the just neglect which has so long attended it. As to the *temporary mediate* ligature, so seducing at first sight, we may declare it, from the preceding demonstration, to be a difficult, complicated, and uncertain method. To these faults the *double* ligature adds violence, complexity of procedure, occasional impossibility of execution, copious suppuration, and a greater liability to hæmorrhage. There only remains then the *circular* ligature to which good sense and general custom had accorded a preference that science approves and justifies. By a fortunate combination, all the data to which we are accustomed in surgery to have recourse for the appreciation of a method are propitious to this. The simplicity of the mechanism of the operation, the little irritation it gives rise to, the regularity and mildness of the process by means of which the artery becomes obliterated, and especially the promptitude of the cure and the large proportion of success, are the prerogatives which render, and will always render, this superior to all other methods. The circular ligature is not faultless, and reverses may occur under its use, but for its choice it suffices to know that it is the most easy and most useful procedure. The idea of closing an open artery with a circular thread seems so natural, that it must have presented itself to the first operator and signalised the origin of the operation. The disasters which in the course of time attended its employment, and ignorance of their true causes, suggested a multitude of procedures, which, received without criticism, have come down with various vicissitudes, even to our own times. But, at the present period, science and practical observation have so thoroughly examined all these methods, that the danger of following an error upon the authority of others, or fostered by a blind empiricism, is removed."

SECTION 2. *On the Torsion of Arteries.*—The author is a warm admirer of this practice, which has met with so little encouragement in England. Torsion produces both mechanical and organic effects. To the first we

refer the circular laceration and the introflexion of the proper tunics of the vessel which sometimes takes place, the strangulation of the cellular tunic, and the formation of the internal clot. The second are analogous to those which result from ligature. We are here presented with the results of 60 experiments made on animals, and, what is of far more importance, the relation of the author's experience of this means applied to man. He has submitted to torsion during the course of the last nine years, both in public and private practice, a very great number of arteries, such as the occipital, temporal, maxillary, superior thyroid, external pudendal, spermatic, digital, and some branches of the subclavian, and indeed all the arteries of the fourth and fifth rank when wounded at the surface of the body, and always with entire success. In regard to those of the extremities, he possesses accounts of 65 cases of torsion after amputation or disarticulations performed by him, and of those of the upper extremity all succeeded except two, in which it was necessary to recur to the ligature; and of 23 cases of torsion of the femoral and popliteal, the ligature was subsequently required in four instances. "It is not unfrequent, however," he observes, after describing the rapidity with which the proceeding may be conducted, "for a first torsion to fail on some arteries, either because they have not been sufficiently twisted, or the vessel having been ineffectually separated from its connections, the laceration of its proper tunics has not taken place. We must repeat the torsion a second or a third time, until we do succeed. After some observations upon the ulceration of the twisted portion, and the partial rupture of the cellular tunic at the borders of the seat of torsion, accidents which, however rare, may yet occur, and give rise to a hæmorrhage only to be restrained by the ligature, the author thus concludes:

"Torsion should be considered simply as succedaneous to the ligature in wounds in the arteries, and with this indication we may, without hesitation, pronounce it a preferable and an entirely safe operation for vessels of a moderate size, and in the majority of cases, if not in all, a successful one for the larger vessels of the extremities."

CHAP. 4. *On the Collateral Circulation, established after the Obliteration of the Arterial Trunks.*—This is a most elaborate and able chapter, in which Professor Porta, by the aid of experiments on animals and observation on man, exhibits the most complete view of the collateral circulation with which we are acquainted. He treats successively of the direct and the indirect collateral circulation.

(A.) *The Direct Collateral Circulation.*—Our want of space quite prohibits our attempting to follow him in his demonstration of this; and it must suffice to observe that he applies the term to the enlargement of prior existing, and the production of new capillary vessels, constituting an immense net-work of *vasa vasorum* surrounding the two ends of the vessel and the newly deposited lymph, and springing from the internal coagula as a matrix. At first these distended vessels exist in countless numbers, but, as the phlogosis becomes subdued, some of the more direct and larger branches only acquire and retain a great size, the others resuming their former condition. The author's observations upon direct anastomosis have been carried on by means of experiments on dogs killed at periods varying

from a week to 2½ years after the ligatures; but in certain necroscopies he has had the opportunity of making upon individuals, who have died some years after the tying of large arteries, the same system of direct collateral circulation was visible enough.

(B.) *The Indirect Collateral Circulation* is accomplished through two different routes, the *deep-seated* or *muscular*, and the *superficial* or *sub-cutaneous anastomoses*. The author details with great minuteness the changes which are operated in the anastomotic system from the first day to the third year after the application of the ligature upon all the principal vessels, as the femoral, carotid, iliac, &c. It is obviously impossible that we should follow him through this elaborate demonstration; but we may advert to his observations upon the alterations which the general arterial system undergoes subsequently to the application of the ligature upon a principal central trunk. These are exhibited in three distinct places, viz., the trunk that has been tied, the collateral trunk, and the anastomosing branches. 1. *The ligatured part.* The two ends of the divided artery gradually diminish in size, although in corresponding points they preserve a certain calibre for the reception of the anastomoses destined to maintain the circulation. The *vasa efferentia* of the inferior portion, converted now into *vasa inferentia*, preserve its natural calibre from their origin below, and restore to it somewhat later a part at least of its natural pulsation; which proves that the indirect flow of blood by the anastomosing branches is sufficiently abundant from the earliest period to maintain the movement and capacity of the inferior portion. 2. The metamorphosis to which the *collateral trunks* are subjected, consists in their amplification. The superior branches are gradually enlarged to the extent of being able to supply the place of the obliterated trunk, conveying as much blood, or nearly so, as had passed by it. Nevertheless, we cannot demonstrate upon the dead body for some weeks an unusual enlargement, and permanent dilatation is of so gradual a formation that it does not become completely developed until after the expiration of several months. But, by reason of the dilatability of the arteries under the force of the heart's impulse, or by means of an organico-vital action exerted upon the anastomotic ramifications of the smaller branches, a distension, which cannot be exhibited after death, yet does occur during life. The dilatation is much more irregular in the collateral branches connected with the lower portion of the artery than in those arising from the upper. When the collateral circulation is matured, the sum of the efferential and inferential branches is about equal to the calibre of the lost artery, and the change operated in them, upon the occasion of the use of the ligature, is permanent, although, after the death of the individual, we may find some of them not larger than in the limb which had not been operated upon, either because their dilatation had not yet become organic and permanent, or that they had taken none or a very slight part in the new collateral circulation. 3. The *anastomozing branches* undergo changes as respects their calibre, form, and structure. Their unusual dilatation is observed from the earliest period by the aid of injections. Throughout the anastomozing system the medium branches are first seen in a few days to become turgid, then the secondary ramifications, and, lastly, the lateral trunks, which communicate with the two extremities of the obliterated artery. Within the first week the anastomotic branches

double their capacity and change their form. It is the anastomotic arches which occupy the centres of the muscles and proceed directly towards the distal end of the artery that first enlarge, and then those which pursue a longer and more tortuous course towards the periphery. The entire anastomotic system does not acquire its full development for several months or even a year. Observed at a more remote epoch, no ulterior progress is noted, and vessels of three sizes are then discernible. The larger have a calibre of two millimetres; the medium of one, and the smallest little more than that of the ordinary capillaries. The deep anastomotic arches, while enlarging, take on very different forms, being sometimes simple, at others complex, rectilinear, circular, elipsoid, rhomboid, &c.; sometimes they are twisted into small knots, resembling tumours, purses, &c., and then regain their regular form. At first, the walls of these vessels are soft, transparent, and fragile, but eventually they become dense and resisting. The vessels, in enlarging, may become twisted upon themselves in a serpentine or convoluted manner, and very small lateral branches, which perhaps may have pre-existed in a rudimentary state, are seen to spring forth, and intercommunicating, describe simple or complex causes, and return at various distances to the principal arch, thus increasing the vascular interlacement and the facility of communication.

Professor Porta believes that these facts observed so constantly, positively, and clearly as regards the system of indirect anastomoses in animals, must serve as a foundation for any true theory upon the subject, and that, if the same experiments could be performed on man, very similar results would probably be observed. He relates the history of the pathological changes in the arterial system after the application of ligatures on some of the great arteries observed by himself and others, and, comparing these with the inductions drawn from experiments upon animals, he arrives at the conclusion that the analogy is very striking, and that the sole differences are the following:

"1. In animals they are generally the same vessels which form the collateral circulation, and these always undergo the same alterations. In man, such constancy is not observed; sometimes the branches of one side, and sometimes those of the opposite, form the principal communications. 2. In the former, when the principal trunk is closed at one point, the collateral branches conduct the stream of blood to the same point of the inferior portion of the artery; but in man, on account of the presence of an aneurism which has required an operation, the trunk becomes obliterated over a long track, or in several distinct places; whence, instead of one there is often formed a chain of two or three anastomotic rings along the whole course of the limb. This is seen especially at the hip, where the blood of the circumflex and perforating arteries only reaches the leg through the articular and recurrent tibials. 3. Comparing the pathological observations with our zootomic experiments, it results that the blood, which at first flowed through numerous small vessels, is afterwards included in a few large channels, to which the collateral circulation is circumscribed. 4. Just as the phenomena of the collateral circulation are simple and constant in animals they appear in man complicated, anomalous and versatile. It is possible and occurs in the human body, according to the pathological condition of the limb or the individual, that the arterial system, impeded in its natural direction, may change in different cases the route of the blood, and may limit the collateral circulation to a few principal vessels. But the fact, in animals, has been rendered clear by repeated experiments and dissections; while, in man, it is vague and unde-

termined, in consequence of the uncertainty of the observations which support it."

Mr. Guthrie also offers some interesting observations upon the *collateral circulation*.

"Two distinct kinds of collateral circulation are at present acknowledged: one by direct large communicating arteries; the other through the indirect medium of the capillary vessels, inosculating with each other. Where the direct communicating arteries exist, little subsequent change takes place in them. It is otherwise with the indirect capillary vessels. When the radial or ulnar artery is divided in the hand, the blood will not only readily flow from each end of the vessel, but equally red and arterial from both; the communication being through direct arterial branches from one vessel to the other. It will be also red and arterial if the division take place at the wrist; and may be so in the brachial. But if the femoral, in the lower part of the thigh, be wounded, the colour of the blood issuing from the lower end of the artery, if any issue at all, will be venous. It is so, because it has been obtained from the capillary arteries, which, in this case being empty, receive blood by regurgitation from the veins, the valves of which, when present, do not prevent its reflex course. If a limb be injected and carefully dissected four or five days after a ligature has been placed during life high up on the principal trunk, the capillary vessels will be seen to be well injected; but few or none will be found large enough to admit of their inosculation being traced throughout. If another limb be injected, and dissected some sixty days after the ligature has been applied, a difference will be distinctly observed between the two preparations. In the latter, the capillaries will not appear to be so fully injected, but several larger and more tortuous vessels will be found in situations where they were not expected to exist; and the anastomoses of these, one with another, and generally by arches, may be traced to their communication with the principal trunk, both above and below the obliterated parts. If an incision were made in the nearest pervious portion of the lower part of an artery of a person who had undergone this operation, arterial blood would issue from it. The communication would have become directly communicating branches, and the capillaries would have returned to their accustomed duties. * * * * * * * * It is asserted by some sanguine supporters of the all-powerful influence of the collateral circulation, that it is sufficient at all times, and under all natural circumstances, to maintain the life of the extremity. The practice of the Peninsular war proved the fallacy of this opinion in too many instances, to admit of any doubt of its inadequacy to do so in the lower extremity, after the division of the femoral artery, under ordinary circumstances."

(c.) *On the Accidents which may accompany the establishment of the Collateral Circulation.*—Professor Porta considers those, which arise from an excess or deficiency of the circulating fluid, consequent upon the application of a ligature. To the former he refers plethora, inflammation, and the relapse of aneurism: to the latter paralysis, gangrene, and atrophy of the limb operated upon. The accidents arising from *plethora* or *inflammation* may exhibit themselves by their proper symptoms, either in the part affected or in some distant locality. The brain, according to the author, is a very common seat of their manifestation; and thus, of eleven cases of ligature of the carotid, five offered evident signs of cerebral plethora, requiring the detraction of blood. All who are familiar with the history of this operation must be aware of the frequency with which encephalic disturbance, or even disease, has followed its performance; but it is quite new to us, and

not very intelligible, that cutting off a large supply of blood to the brain should induce a state of plethora of that organ. Ligatures of other large arteries, Dr. Porta continues, give rise to similar consequences; and inflammations of the coverings or the viscera of the chest and abdomen have not unfrequently been induced; and severe inflammation of the limb operated upon is less common than that of the organs now alluded to.

"The other accident arising from the exuberance of the collateral circulation and the excessive dilatation of the anastomosing branches, is the reproduction of the pulsating aneurism. This has been observed in the neck, the hip, and in all the limbs. Oftentimes it is only temporary, because the blood having lost, during its course through the winding anastomosing vessels, the impulse that conveyed it from the heart, is no longer enabled to prevent the coagulation in the tumour. Nevertheless, it suggests to practitioners the necessity of retaining the limb and the patient himself in a state of quietude, and to ensure, sometimes with the aid of antiphlogistics, cold, or compression, the exit of the ligature. In some cases, the natural anastomoses are so large and direct, or dilate themselves with such rapidity, that they retard or prevent the coagulation, and thus impede or prevent the success of the operation."

Passing now to accidents of an opposite description, the author observes that *paralysis* and *atrophy* may simultaneously or separately follow the ligature of the artery. The nervous system presiding over the other parts of the economy receives its nutriment from the circulating system, and whatever diminishes or interrupts the course of this latter, must weaken or suspend the actions of the nervo-muscular system, sensation, and motion. In animals, paraplegia is observed to result from ligature of the aorta, and in man, paralysis of the limbs has followed the tying their principal blood-vessels. Several examples, after the ligature of the humoral artery, are cited. At other times, atrophy of the limb follows as a sequel to paralysis, or it may exist without any prior affection of the nervous system, owing to a paucity of nutrition after the obliteration of the principal vessel. Several cases from his own and others' practice are cited in proof.

Instances of *gangrene* supervening upon ligature of an artery are, however, far more frequent in man than those of paralysis or atrophy. "Of 600 cases of ligature of the large arteries on record, gangrene has occurred in 50. Of 132 operations in the carotids, it occurred in 1; of 150 cases of ligature of the innominata, subclavian, axillary and humoral, in 7; and of 302 operations in the lower extremities, in 42." As to the superior extremity, Signor Porta believes that unless some grave complication existed, such as the severe nature of the wound, the bad performance of the operation, hæmorrhage, affection of the chest, the inclusion of nerves in the ligature, enormous ecchymoses, gastric fever, phlegmon, &c., &c., we should perhaps never meet with such an accident. Its occurrence in the lower extremity is favoured by the greater distance from the centre of circulation, the size of the limb in proportion to the body, the small size and circumvolutions of the anastomoses, the disposition of popliteal aneurism, which easily compresses and annihilates the lateral arterial radicles. "In consequence of these dispositions, we almost always find, after the ligature of the iliac or femoral arteries in man, there are present, during the first days, symptoms of paralysis, formication, chilliness, and heaviness of the limb, which are sometimes followed by gangrene." The synoptical

table of 600 operations, undertaken by surgeons of various nations from the time of John Hunter to the end of the year 1843, is a valuable statistical document.

In taking leave of Dr. Porta's work, we beg again to express our admiration of the patient industry and philosophical spirit of enquiry with which he has pursued and attained the object in view; although, as we before observed, we should have been better pleased had he caused less animal suffering by abstaining from many experiments,* the repetition of which was uncalled for, and has led to no novel conclusions.

Mr. Guthrie's Lectures are full of valuable facts and important critical observations, condemnatory chiefly of performing the operations for wounded arteries which are only fitted for them when in their aneurismal condition. One hundred and thirty already-published cases are briefly detailed and commented upon, furnishing the student and practitioner with some excellent precepts and cautionary remarks. Although the extract is a long one, we cannot forbear laying before our readers Mr. Guthrie's

General Conclusions.

"1. The Hunterian operation for the cure of an aneurism is not applicable to the treatment of a wounded artery, inasmuch as the wound of the artery communicates with the external parts, and nothing intervenes to prevent blood flowing from the wound in its side, or from its cut extremities.

"2. When a large artery is divided and bleeds, the wound should be enlarged if necessary, and a ligature placed on both the divided ends; but if the artery be only injured and not quite divided, the ligatures should be applied one immediately above, the other below the injured part. The artery may or may not be then cut across, at the pleasure of the operator, but the limb or part should be placed in a relaxed position. A bandage should not be applied, and the edges of the wound should be simply brought together by adhesive plasters, which do not extend completely around the limb.

"3. No operation is to be performed on any artery unless it bleeds at the moment of its performance, inasmuch as hæmorrhage once suppressed may never return.

"4. The intervention of muscular fibres, or of whole muscles, is not a sufficient reason for tying the artery at a distant part. They must be divided, if it be possible, to the extent required for a due exposure of the injured artery and its accompanying veins and nerves.

"5. If the wound pass indirectly to the principal artery, from the back of the thigh for instance to the femoral artery in front, or from the outside of the arm to the humeral artery on the inside, the surgeon may (on satisfying himself of the part likely to be injured, by the introduction of a probe) cut down on the vessel opposite the part supposed to be wounded, by the most simple and approved method. When the artery is exposed, the probe will point out the spot

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* We were sorry to observe, in an article in a recent number of the *Lancet* (July 18), written with the excellent intention of inciting students to a more philosophical study of their profession than now prevails in this age of manuals and vade-mecums, a recommendation that they should engage in the performance of experiments as a means of awakening their energies and exciting their attention. "One good experiment," it is said, "is worth loads of paper and pseudo-physiology." "A properly chosen experiment" is also recommended as part of the means of testing the candidate's ability.

at which the vessel has in all probability been wounded. Pressure made below this spot on the artery, will cause it to be distended and to bleed, if the flow of blood be not prevented from above: the artery is then to be secured by two ligatures, and the lower one should if possible be applied first.

"6. The tourniquet should never be used in an operation for aneurism or for a wounded artery. Compression by the hand in the course of the wounded vessel is allowable.

"7. The blood from the upper end of a divided artery, or that nearest the heart, is of a scarlet arterial colour.

"10. The blood from the lower end of a divided artery, or that which is furthest from the heart, is of a dark or venous colour, when it happens to flow immediately after the division of the vessel. At a subsequent period it may assume more of the colour of arterial blood, but it rarely does so for several days after the receipt of the injury, and always flows, or at least until a very late period, in a continued stream.

"11. This regurgitation or flow of blood from the lower end of a divided artery is a favourable sign, inasmuch as it shows that the collateral circulation will probably be sufficient to maintain the life of the extremity.

"12. The collateral circulation is in almost every instance capable of maintaining the life of the upper extremity when the axillary artery is divided, and the colour of the blood which flows from the end of the artery, on its being divided, is not always as dark as in the lower extremity, and it sooner resumes its arterial colour.

"13. The collateral circulation is not always capable of maintaining the life of the limb when the femoral artery is injured. The best assistance which art can give is to rub the foot and leg in the gentlest manner, between the hands of one or two strong young women, for several hours, or even for the first three or four days: relaxing this process very little, even during sleep. When the vein is divided at the same time, or rendered impervious, the limb usually mortifies.

"14. The collateral circulation is sufficient to maintain the life of an extremity in almost every case in which an aneurism has existed for eight or ten weeks, although it may be incapable of doing this if the principal artery have been suddenly divided, without any previous disease having existed in the part.

"15. The theory and the operation for aneurism are never to be applied to the treatment of a wounded artery, which has caused a diffused or circumscribed aneurism, *whilst the external wound communicates with the artery*, unless it be impossible or impracticable to tie the bleeding vessel.

"16. When an artery has been wounded, and the external opening has healed for weeks or months, so as to give rise to a diffused or circumscribed aneurism, it may be treated according to the theory of aneurism occurring from an internal cause, if the case will permit it without danger; although with this difference, that as the artery is sound the operation may be performed close to the tumor. If any doubt exist as to the capability of the collateral circulation to support the life of the lower extremity, after the external iliac has been secured by ligature, the operation should be performed at the injured part by opening the swelling and enlarging the wound, as in the case of a wounded artery.

"17. When a circumscribed or diffused aneurism which has formed after a wound has been opened, whether by accident or design, it is placed in the situation of a wounded artery, and should be treated as such. If the aneurism has arisen from disease of the vessel, and the wound or opening into it cannot be permanently closed, the limb is in a worse state than if the artery had been wounded by accident; because a ligature or ligatures placed on a diseased artery are little likely to be successful. They are liable to all the difficulties and inconveniences attendant on the old operation for aneurism. If a case of the kind should occur in a popliteal or femoral aneurism, situated at or below where the artery passes between the triceps and the bone, amputation, if it can be done low

down, will be the best remedy. If the aneurism should have formed higher up, and the opening can be closed with any prospect of its healing, a ligature may be placed upon the artery above it; but on the recurrence of hæmorrhage which cannot be restrained by moderate pressure, the artery must be tied below, or recourse had to amputation. It is, however, to be observed, that amputation under these circumstances, when resorted to as a third operation, rarely succeeds.

"18. When an artery is wounded with a simple fracture of a bone, or with a comminuted fracture of smaller bones, with an external communicating opening, both ends of the artery should be secured, and the limb treated in the usual manner.

"19. When the bone broken is the femur, and the artery divided is the femoral artery, the operation of amputation will generally be advisable. It will always be so if the fracture is a comminuted one, or the shaft of the bone is extensively splintered.

"20. When the broken bone injures the artery and gives rise to an aneurism, the treatment is to be first of the fracture and then of the aneurism, as soon as circumstances render it advisable or necessary to have recourse to the operation for aneurism; which can only be after time has been given for the collateral branches to enlarge, so as to maintain the life of the limb.

"19. When mortification takes place in addition to, or as a consequence of a wounded artery, amputation should be had recourse to forthwith.

"20. The place of operation should be in almost all cases at the seat of the original wound; but there may be an exception, viz.

"21. When, for instance, the artery has been a mere cut, just sufficient to divide the artery and vein immediately below Poupart's ligament, and mortification of the foot supervenes, amputation should then be performed below the knee, or at the part where the mortification more usually stops for a time.

"This rule is founded on the observation, that great efforts are made by Nature to arrest mortification a little below the knee. Sometimes they succeed; when they fail, death is almost inevitable. The advice to amputate at this part is founded on the fact of its being infinitely less dangerous, when done there, than on the thigh, independently of saving a joint.

"22. When mortification has *continued for several days*, and is spreading without having once stopped, the constitution of the patient being implicated as marked by fever, amputation should not be performed until the mortification has been arrested and the line of separation has been well formed. In many cases, where there is great weakness of or irritability of constitution, it will be advisable to defer the operation to a later period, particularly if there be hope of the patient's becoming stronger and more tranquil.

"23. If the mortification has once stopped and then begins again to spread, it will never again cease to extend, and amputation may give some chance of life.

"24. Amputation of the arm should never be had recourse to, in consequence of a wound of the axillary artery, unless mortification takes place.

"25. When mortification takes place after the operation for aneurism, the surgeon must be guided by the state of the patient's constitution, in resorting to or refraining from amputation.

"26. When hæmorrhage occurs from the surface of a stump, the artery should be tied at the part from which the blood comes in the first instance, if it can be easily done. If this should not suffice, the artery must be tied higher up, just at such distance as will afford a fair hope of its not having been affected by the derangement of the stump, which has led to the failure of consolidation in the extremity of the artery; yet not too high to admit of the junction of any large collateral branches. If the bleeding proceeds from several small vessels, and cannot be arrested, the principal trunk should be tied immediately above the diseased part, and the patient removed to a purer atmosphere, without which an operation rarely succeeds in any case.

"27. When an aneurismal tumour mortifies, it is unnecessary and improper to

tie the artery above the tumour, because it will be obliterated if the mortification be arrested by the efforts of nature, which the operation may interfere with, and even prevent, whilst, if the mortification spreads, it will be a matter of supererogation, and only hasten the patient's dissolution. When an aneurism inflames, is opened by ulceration, and bleeds profusely, so as not to be arrested, it is a proper case for amputation, if such an operation can be performed."—P. 85.

THE ECONOMY OF THE ANIMAL KINGDOM CONSIDERED ANATOMICALLY, PHYSICALLY, AND PHILOSOPHICALLY. By *Emanuel Swedenborg*, late Member of the House of Nobles in the Royal Diet of Sweden, &c. Translated from the Latin by the Rev. *Augustus Clissold*, M.A. Two Volumes 8vo. London: W. Newberry, 1846.

SWEDENBORG, or Swedberg, as he was named before he was ennobled in 1719, is known, for the most part, as a well-meaning enthusiast, and as the founder of the religious sect named after him, "*Swedenborgians*." He was, however, one of the most distinguished philosophers of the age in which he lived; well versed in almost the entire circle of science; and, moreover, a practical anatomist. It has been affirmed that he had even anticipated Dalton, not only as to the atomic theory itself, but likewise as to some of its details. His scientific works are very numerous, and treat of some of the deepest and most fundamental truths; it was, indeed, the object of Swedenborg to systematize the vast accumulation of former ages, to select the leading truths from the various schools of philosophy, and to establish the principles which flow from them. To undertake at all a task thus vast and comprehensive, indicates the possession of the most varied information; that the effort was successful would not be expected by those who recal the state of knowledge at the period, the first half of the last century, when this remarkable man composed his scientific works. It is, however, interesting to observe, as an indication of the complexion of Swedenborg's mind, that he himself was convinced the time was ripe for the bold enunciation of the broadest principles in every department of science, including the most abstruse questions in physics, chemistry, and physiology. "In approaching the human body," we here borrow a passage from the interesting biographical sketch contained in that valuable work, the Penny Cyclopædia, "he again insisted on the necessity for principles and generalizations, without which, he said, 'facts themselves would grow obsolete and perish:' adding that 'unless he were much mistaken, the destinies of the world were leading to this issue.'" Even in these physiological enquiries he entered into metaphysics, being resolved thoroughly to examine "the world or microcosm which the soul inhabits, in the assurance that she should be sought for no where but in her own kingdom." It was about the time when the last of these scientific works were published, namely in 1745, that their author conceived he had received a divine revelation; and those who carefully compare his theological with

his previous philosophic speculations, will find in more than one respect the same train of thought running through them; as if his conviction that the time was come for proclaiming the ultimate principles of all science, had led to his notion that the second advent of the Lord and the Apocalyptic New Jerusalem were truly realised and witnessed by him.

A philosophic work like that before us, written by a man of such marked psychical peculiarities as Swedenborg, cannot fal to interest all who have leisure to peruse a production which, having been long since superseded by later and more sound writers, has lost all practical utility. It has indeed puzzled us to comprehend the motives of the learned and Reverend translator in bringing again into light a treatise, which has become well nigh obsolete. That no scientific views have had influence, we must infer from the omission of those copious notes, by which alone the multiplicity of errors long since exploded, but which pervade the whole work, could have been corrected and explained. As it is we have, mixed doubtless with much sterling matter, a multitude of fanciful hypotheses and details concerning the "animal spirits," "spirituous fluid," and the "corcula," or little hearts destined for their circulation, consisting of "the spherules of the cinéritious substance;" the whole clothed in a language, which, owing to the advance of physical, chemical and physiological science, has to a great extent lost its significance. The following extract relating to the composition of the blood and to the "spirituous fluid," will enable our readers to judge of the general style of the work.

"Blood comprehends in every one of its spherules, mere first principles, elements, and simples. Consequently it possesses potentially and virtually every single thing in the mundane system which is producible from first principles, elements or simples; that is, every thing which is possible. Those volatile ethereal substances which temper the spirituous fluid, are the first and only entities of their own and the following degrees; hence also they are the elements of those degrees. The volatile aërial substances are also the simples of their own and the next following degree; while the saline cube, which is the cement of the whole part, is the simple of its own degree."—Vol. 1, p. 76.

With respect to the "spirituous fluid," it is described as consisting of a subtle matter poured into the nervous fibres, from the blood-vessels, in the cineritious substance of the brain and spinal cord, and which, after circulating through the nerves, returns again into the vessels. The hypothetical notions of Swedenborg respecting the circulation generally, and the movements of this subtle fluid are thus explained:—

"In the vessels, equally as in the blood and membranes, there are three degrees of composition to be taken into consideration, all of which should be distinctly perceived. The vessels of the first degree are those commonly called blood-vessels; the vessels of the second degree are the exsanguious vessels; and the vessels of the third degree are the fibres of the nerves. In conformity with these various degrees of vessels, the circulation itself is subtriplicate; namely first, a less universal circulation, which is that of the red blood; secondly, a more universal circulation, which is that of the pure blood, and thirdly, a most universal circulation, which is that of the spirituous fluid.

While the red blood is passing from vessels of its own order into vessels of another order, it becomes divided into the purer blood, or into blood of the second order; the saline, urinous, or sulphurous atoms which had entered into the composition of that degree, being deposited at the mouths of ingress or division. A

corresponding operation is carried on when the blood passes from vessels of the second order into vessels of the first, or into the fibres.

"After reaching the fibres, the blood continues its passage through them, returns into the vessels of the second or third orders, and becomes again compounded by passing through degrees similar to those by which it had become divided."—Vol. 1, p. 98.

All this is mere surmise, and much of it is entirely disproved by the exact knowledge possessed in the present day with regard to the arrangement of the blood-vessels and nerve-tubes. Those who are accustomed to minute observation, are satisfied that the limits of the vascular system are definitively established; they are perfectly recognisable, and demonstrate that there is no such relation as that here surmised with the nerve-tubes. If it be argued that although Swedenborg was in error as to the path by which the "spirituous fluid" flowed from the blood, and that the transit may be effected as well by endosmose as by direct channels, it may be replied that suppositions of this kind, resting upon no ascertained facts, have abounded in every department of science, but that they are not the means by which truth has ever been elicited.

The volumes before us contain, as we have said, much valuable matter, consisting, however, for the most part, of extended extracts from the works of Malpighi, Leeuwenhoek, Morgagni, and indeed from most of the distinguished authorities of the 17th or 18th centuries; many of Swedenborg's own observations also afford interesting subjects for reflexion.

His account of the ganglionic or nervous corpuscles, as they are termed will be read with interest by those who are acquainted with the latest descriptions which have appeared of these important elements of the gray matter.

"The cortical substance, either when lying proximately beneath the pia mater, and watered, nourished and cherished by the purer blood, or when, under the name of the cineritious substance, it occupies various tracts more remote from the surface, may, by the naked eye, and more plainly still by the help of glasses, be seen to consist entirely of minute spherules nearly approaching to an oval form. The cerebrum and cerebellum themselves, also approach nearly to the spherical and oval form, and thus assume a shape like that of their parts. Hence these minute organic substances, inasmuch as they are like their whole, and have the same potency individually, which, conjointly and aggregately, is exercised in the compound, merit the name of *cerebellula*. The eye also, by artificial aid, is enabled to discover that these forms, spherules, or cerebellula, are clothed with and inclosed in, a membrane of meninx, much in the same manner as the brain itself, except that their membrane or meninx deserves the title of pia in the superlative degree, and that they are distinguished from their neighbouring and associate spherules of the same kind. It may also be discerned, that these most delicate coats are composed of villi and capillary shoots, of most minute arteries in multitude innumerable, in determination wonderful, and in order most beautiful, which diffuse in all directions a volatile and spirituous fluid, educed from the blood, and conceived by eminent generation in their most pure wombs. These cerebellula appear to be the internal sensories, which receive impressions and modifications from the external sensories, and which convey them afterwards higher up to the judgment-seat of the mind."—Vol. 2, p. 41.

It would be useless to occupy the time of our readers with any further account of this work, which the editor implies is not so much designed for physiologists as "for the educated public;" but assuredly the recollection

that the peculiar views of Swedenborg, both as regards the constitution of matter in general and the phenomena of animal bodies, have commanded no attention; joined to the knowledge that the present volumes abound in errors as to matters of fact and observation, will indispose the judicious enquirer to place any reliance on hypotheses based upon such materials, and opposed, in so many respects, to the ascertained truths of physics and physiology.

I. Animal Chemistry, with reference to the Physiology and Pathology of Man. By *Dr. Franz Simon*. Translated and edited by *George E. Day*. M.A. & L.M., Cantab, Licentiate of the Royal College of Physicians.

II. Dr. Day's Reports on the Progress of Animal Chemistry, in *Ranking's* Half-yearly Abstract of the Medical Sciences. London, 1845–6.

III. Grundlinien der Physiologischen und Pathologischen Chemie. Fur aerzte und Studirende. Von Dr. *Hermann Hoffmann*, mit einer Tafel Abbildungen. Pp. 340. Heidelberg, 1845.

Elements of Physiological and Pathological Chemistry, designed for Practitioners and Students. With a Plate.

IV. A Practical Manual, containing a Description of the General, Chemical, and Microscopic Characters of the Blood and Secretions of the Human Body, as well as of their Components, including both their Healthy and diseased States, with the best Method of separating and estimating their Ingredients. Also, a succinct Account of the various Concretions found in the Body and forming Calculi. By *John William Griffith*, M.D. F.L.S., Licentiate of the Royal College of Physicians, London, Honorary Member of the Philosophical Society of St. Andrews, and Senior Physician to the Finsbury Dispensary. Small 8vo, pp. 168. London, 1846.

For the work that stands foremost in the above list we are indebted to the Sydenham Society, and we believe that we are merely echoing the feelings of the great majority of its members in stating that it is the most original, and at the same time one of the most valuable books that has yet appeared under their auspices. Six years ago, all that was known of animal chemistry was to be found in the last volume of the great work of Berzelius, occupying something less than 300 pages. Here we have two goodly volumes, embracing no less than 950 pages, and literally crammed with analyses made for the most part by German and French chemists since that period. As it forms a much more complete system of animal chemistry than the works either of Hoffmann or Griffith, we shall adopt

Simon's order and arrangement, and merely notice the other volumes in the elucidation of some few special points. In the first place, we must premise that the Translator and Editor, Dr. Day, has added such a large amount of valuable matter as almost to entitle him to the claim of co-authorship with Simon; the additions to the first volume amounting to about 159, and in the second, considerably exceed 200 pages. We feel bound in common justice to make this observation, because we believe that very few of our readers can form an adequate idea of the extreme labour incurred in wading through the extensive regions of foreign periodical literature. With this preliminary remark we proceed without further comment to Dr. Day's Introduction, which occupies the first 86 pages of Vol. I., and is devoted to the chemistry of the proximate constituents of the animal body. These are, as usual, divided into the mineral and the organic. The mineral are arranged in three groups, comprising—1. Those which are of service in the animal body, in consequence of their physical properties—water, phosphate of lime, carbonate of lime, phosphate of magnesia, and fluoride of calcium. 2. Those which are useful by their chemical properties—hydrochloric acid, chloride of sodium, carbonate of soda, phosphate of soda, chloride of calcium, chloride of iron, and iron occurring in some unknown condition, as in hæmatin. And, 3. Those which being only incidentally present, may be eliminated without exerting any unfavourable effect on the economy; as, for instance, chloride of potassium, alkaline, sulphates, carbonate of magnesia, manganese, silica, alumina, arsenic, copper, lead, and the ammoniacal salts.

The organic constituents are arranged in two groups, the former embracing the nitrogenous, and the latter the non-nitrogenous, matters; in the first are considered protein and its compounds—albumen, fibrin, and casein; pepsin, ptyalin, gelatin, pyin, extractive matters, colouring matters, bilin, urea, uric acid, hippuric acid, uric oxide, and cystin; in the second, there are arranged the animal sugars, fats, and non-nitrogenous organic acids, namely, lactic, oxalic, and acetic acid. Of protein and its compounds we find a more comprehensive notice than in Hoffmann's Elements, or Griffith's Manual. Why Dr. Griffith has assigned to protein the formula C_{40} H_{30} N_5 O_{12}, instead of allowing it an additional equivalent of hydrogen, we know not. We were at first inclined to believe it a misprint for the ordinary formula C_{40} H_{31} N_5 O_{12}, but finding it originally in page 3, and repeated in the two following pages, we conclude that it is based on some assumed reason. Protein forms, as probably most of our readers are aware, two distinct oxides, viz. the *binoxide*, which is left in an insoluble form, after fibrin has been boiled with water for some hours; and the *tritoxide*, which is formed by boiling fibrin or albumen in water for many hours, precipitating by acetate of lead, and neutralizing the solution by ammonia. After collecting the lead by sulphuretted hydrogen, the tritoxide may be obtained by evaporating the solution. These oxides are continually being produced in the blood, and occur in great excess in inflammation. In fact, the process of inflammation is regarded by Mulder, and we believe correctly, as a higher grade of oxidation. "The albumen of the blood, which furnishes only tritoxide by ebullition, probably takes no part in this change; we may conclude that it is effected by the fibrin alone, which, as we know, absorbs oxygen from the air, and is with so

much comparative facility converted into binoxide and tritoxide of protein. During the height of inflammation there is, as we have already mentioned, a great excess of these oxides in the blood. Respiration may consequently be regarded as a true oxidation of the blood, or rather of the protein; and in inflammation, in which the blood contains a greater quantity of binoxide and tritoxide of protein than in the healthy state, this body becomes more thoroughly oxidized. Hence it is, that in acceleration of the act of respiration—in fevers, for example—inflammation so easily supervenes after any violent or sustained efforts. Every paroxysm of fever must necessarily cause the formation of a greater quantity of oxidized protein in the system, and every augmentation in the amount of oxidized protein must produce inflammation, which in its turn may determine fever. Hence it also happens that stimulating foods and drinks which quicken the respiration, or cold air which introduces more oxygen into the lungs, often give the first impulse to the development of inflammation in the organism. The buffy coat is formed when the oxides of protein predominate in the blood; when they accumulate in any particular part of the system, local inflammation is the result. In the latter case, morbid products, as for instance false membranes, are evolved, which are found on analysis to be composed of oxidized protein. Now, inflammation must be combatted by endeavouring to diminish the quantity of tritoxide of protein, and to hinder its formation in the lungs. Venesection proves antiphlogistic by directly diminishing the tritoxide of protein: increased secretion of the alimentary canal indirectly produces the same effect by accelerating the change of substances in the body, and consequently also the consumption of a greater quantity of protein and its oxides."—Vol. 1, pp. 12, 13, *Note*.

Many of our readers may not be aware that Mulder's protein-theory, which for the last six or eight years has been regarded as fully established, has, during the last few months, been fiercely assailed by the Giessen school—by Liebig himself, and by Laskowski, one of his pupils—who assert that it is impossible by Mulder's directions to isolate a matter altogether devoid of sulphur, and that his protein is altogether hypothetical. For our own part, we place great reliance on the accuracy of Mulder, and shall, at all events, stick by the protein-theory till we see his reply to these attacks.

Albumen, fibrin, casein, pepsin, and ptyalin, are next discussed, and the reader will find a considerable amount of information on these subjects not to be met with in any other English work. Gelatin is considered by Dr. Day under two heads, as chondrin and glutin—an arrangement, we believe, first adopted by Müller. "The extreme facility with which tannin precipitates gelatinous matter gives a clue to the medicinal action of astringent drugs in the human organism. They at once form insoluble compounds, (for tannin acts similarly on the protein-compounds,) and do not enter the blood; and this is the reason of their being comparatively innocuous. According to Mulder, a less amount of tannin than is contained in one ounce of cinchona bark would, if conveyed directly into the blood, cause instantaneous death." In page 28, we find an ingenious hypothesis (for we can call it nothing more) regarding the origin of glutin. As no traces of it have ever been discovered in the vegetable kingdom, we cannot suppose that, like protein, it arises from that source. In all proba-

bility it is formed by the action of alkalies on protein; since we know that protein submitted to such influence yields products which in their chemical composition approximate closely to glutin, and that the blood is sufficiently alkaline to effect such or similar modifications. The author then proceeds to illustrate by symbols a possible mode by which glutin and water may be formed from protid and erythroprotid by the ammonia which the alkali of the blood evolves from the protein-compounds, with the co-operation, in the lungs, of the atmosphere oxygen. In the present state of organic chemistry, it is impossible in most cases, as the author himself allows, to state with certainty how changes such as these take place. We can only indicate the possible and the most probable methods. That the gelatinous tissues are evolved from protein-compounds, in some manner or other, cannot admit of a doubt. From what other source can they be derived in the chick, but from the protein-compounds of the egg?

Passing over nine pages devoted to the consideration of the extractive matters, we arrive at the colouring matters of the blood, bile, and urine. The following extract affords a fair illustration of the style of this portion of the work. We select it because it clearly disproves the assumed connection between the red colour of the hæmatin and iron.

"Mulder has carefully examined the action of chlorine on hæmatin. He found that if a current of chlorine be transmitted through water containing hæmatin in suspension, the iron leaves the other elements and forms a chloride of iron, while the atom of metal thus removed is replaced by six atoms of chlorous acid, and a compound is formed, which is represented by the formula $C_{44}\ H_{22}\ N_3\ O_6 + 6\ Cl\ O_3$. During this process the red colouring matter is destroyed, and the new compound appears as a white flocculent precipitate. It must not, however, be assumed from this experiment, that the red colour of the blood is dependent on the iron, for that constituent may be removed from the hæmatin, without materially affecting its tint, as may be shown in the following manner. Let some dried blood be mixed with concentrated sulphuric acid, and after standing for some days let water be added. Hydrogen gas is evolved by the action of the acid on the dried blood, and sulphate on the protoxide of iron is formed. If the blood after this process, be carefully washed, a mixture of alcohol and sulphuric acid will extract from it red hæmatin in combination with sulphoproteic acid, but perfectly free from iron. Van Goudoever has reduced the following formula for this compound:

$$C_{84}\ H_{57}\ N_8\ O_{22},\ SO_3 = C_{44}\ H_{22}\ N_3\ O_6 \text{ (the organic part of hæmatin)},$$
$$+ C_{40}\ H_{31}\ N_5\ O_{12},\ SO_3 \text{ (sulphoproteic acid)},$$
$$+ 4\ HO.$$

Although this experiment affords conclusive evidence that the red colour of the hæmatin is not dependent on the iron, yet this metal is very firmly combined with the four organic elements of this constituent. Well prepared hæmatin may be submitted for several days to the action of dilute hydrochloric or sulphuric acid without the iron diminishing in the slightest degree.

"Hæmatin treated in this manner, left after incineration 9·40%* of peroxide of iron, the amount that is always yielded by well purified hæmatin. The condition in which the iron exists in hæmatin (whether as an oxide, a carbonate, a carburet, or in the metallic state) has long been disputed. The probability of its existence in a metallic state is strongly supported by the evolution of hydrogen that occurs

* This notation indicates per-centage.

when the clot is digested in sulphuric acid, and water is added; and Mulder suggests that this metal probably exists as an integral constituent of hæmatin in just the same manner as iodine occurs in sponge, sulphur in cystin, or arsenic in the kakodyl series.

"Numerous attempts have been made with the view to ascertain the proportions in which hæmatin and globulin combine, but the results have been very discordant. According to Berzelius, the hæmatoglobulin of human blood contains 100 parts of globulin, and 5·8 of hæmatin. Simon found the ratio to be 100 of globulin to 6·5 of hæmatin in the blood of a healthy young man, and 100 of globulin to 5·3 of hæmatin in the healthy blood of a stout girl. In disease, the variations are much greater. Simon has found as the limits 8·5 and 3·3 of hæmatin, corresponding to 100 of globulin. Regarding the origin of hæmatin, it must clearly be generated in the organism, since it does not exist in the vegetable kingdom. Mulder conceives that it is generated from the normal constituents of the blood in the course of the circulation. Its destination also is obscure. In common with all the constituents of the body, it must be generated, consumed, and reproduced; but in respect to the actual metamorphoses that it undergoes in the organism, or their object, we are perfectly in the dark. Mulder suggests that the products of the decomposition of hæmatin may be possibly traced to the bilifulvin of the bile. Hæmatin may be known, both in its coagulated and soluble state, by its colour. When combined with globulin in the blood-corpuscles, it may be recognized by the microscope. In its coagulated state it may be recognized by its insolubility in water, alcohol, and ether."—Vol. 1, pp. 40-42.

Since the publication of the first volume of Simon, the colouring matters of the bile and urine have received considerable attention, the former from Scherer and the latter from Heller and Martin.* A notice of Scherer's researches may be found in the Report on the Progress of Chemistry in Vol. 1 of Ranking's Half-yearly Abstract, p. 343. And an account of Heller's investigations is given in Appendix II. to the 2nd volume of Simon. Heller believes that, in addition to the ordinary colouring matter, there exists a yellow pigment (uroxanthin), which occurs in solution in a very small quantity in healthy urine, but is much increased in certain forms of disease. He states that by oxidation it becomes converted into two other pigments, one of which is a ruby-red tint (urrhodin), whilst the other is of the colour of ultramarine (uroglancin). These are both insoluble in the urine, forming purple sediments.

"That uroxanthin and its products are derived from urea seems probable, from the circumstance that uroglancin and urrhodin occur in diseases different in most of their characters, but similar in one—the presence of an excess of urea in the blood; thus they are found in Bright's disease, in cholera, and in suppression of urine. Further, when these products occur in considerable quantity, (especially when the blue sediment is spontaneously formed,) there is always much carbonate of ammonia, and very little urea (perhaps mere traces) in the urine, as is often the case in Bright's disease. Finally, Heller has observed the blue tint developed by nitrate of urea artificially prepared and kept moist, and has likewise produced it by adding nitric acid to an old solution of urea partially converted into carbonate of ammonia. The existence of a large quantity of uroxanthin in urine is indicated.

"1. By the clear light-yellow colour of the urine when that secretion is acid, as in cholera, and sometimes in Bright's disease.

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* Ueber das Urakyanin. Munchen, 1845.

"2. By the presence of the products of its oxidation, uroglancin and urrhodin, which either of themselves form a violet-coloured sediment, or communicate that tint to a sediment already formed.

"On allowing urine abounding in uroxanthin to stand for some time, it is observed that, after the formation of the sediment has ceased, the fluid from the surface downwards assumes a violet tint, and this change of colour takes place with a rapidity proportioned to the amount of carbonate of ammonia produced by the decomposition of urea.

"Hence, on keeping such urine in a high cylindrical glass, three distinct strata are observed; lowermost, a violet sediment; in the middle, yellow and nearly clear urine; and superiorly, a violet or purple turbid layer. On shaking the glass, the whole urine assumes a bluish-green tint, because the urrhodin, formed principally at the surface, becomes converted, by agitation with a full supply of atmospheric air, into uroglancin, which, mixing with the central yellow layer of urine, develops a green tint. The uroglancin thus formed, ultimately settles as a blue powder on the sides and at the bottom of the vessel. Hence there is obviously no fixed proportion between the quantities of uroglancin and urrhodin.

"3. If much uroxanthin is present, the crystals of uric acid (separated either spontaneously or by the addition of an acid) have a beautiful blue or amethyst tint.

"4. Lastly, If much uroxanthin be present, it may be recognized by the addition of concentrated nitric acid, (ten drops to half an ounce of urine), which at once communicates a brilliant violet colour to the fluid: if a smaller amount be present, the change of colour is developed more slowly. The nitric acid oxydises the uroxanthin, and converts it into uroglancin and urrhodin. Sulphuric and hydrochloric acids act similarly, but with less activity. If albumen is present in urine treated in this manner, it is either precipitated blue at once, or assumes that tint gradually, according to the amount of uroxanthin. This is constantly noticed in Bright's disease on treating urine abounding in uroxanthin, with an acid, and allowing it to stand for a couple of days; uroglancin separates in dark blue crystalline groups, visible to the naked eye, partly on the surface, and partly at the bottom of the vessel.

"To separate the two products of oxidation of uroxanthin, we collect on a filter the sediment thrown down by nitric acid, and agitate it with cold spirit of ·830, which takes up urrhodin (as also does ether); the residue is boiled for some time with spirit of the same strength, until the fluid becomes somewhat concentrated; we thus get a bright blue solution of uroglancin. To exhibit these substances in normal urine, the fluid must be so far evaporated as just to remain liquid. On adding concentrated nitric acid to the cold residue, a crystalline magma of nitrate of urea is at once formed; on adding to this a few more drops of nitric acid (and sometimes even this is unnecessary), it assumes a violet tint. If the crystalline mass is allowed to stand for some time, and is then dissolved in the smallest possible quantity of distilled water, after being left at rest for some time, it deposits a sediment in which urrhodin and uroglancin may be detected either by the microscope, or by extraction with cold, and then boiling spirit.

The action of nitrate of silver on uroxanthin is very singular. On precipitating the chlorine by an excess of nitrate of silver, from urine acidulated with nitric acid, and then carefully neutralizing the filtered liquid by ammonia, there is not only a pale yellow precipitate of phosphate of silver, but the fluid assumes a brown tint, and in a short time there is likewise a brown sediment.

"Heller has not yet succeeded in isolating uroxanthin. Uroglancin associated with urrhodin, occurs in urinary sediments in Bright's disease, and in cases in which urine, abundant in uroxanthin, has become alkaline in the bladder. Heller has noticed it in these sediments forming groups of delicate prisms. It likewise assumes this form when urine, abounding in uroxanthin, is treated with nitric, sulphuric, or hydrochloric acid. In this case it is principally found on the sur-

face of the fluid. When allowed to crystallize from its cold spirituous solution, it forms groups which appear nearly black, but are blue and transparent at the edges. Urrhodin appears to be a less oxidized product of uroxanthin than uroglancin, and usually occurs in much larger quantity. It is most commonly observed in cases in which the urine is alkaline before emission, in consequence of containing much vesical mucus, and its development in such cases is hastened by the addition of nitric acid. The method of isolating it has been already described; Heller has never succeeded in obtaining it from its spirituous solution in a crystalline form. It occurs in granules, which, under the microscope, appear of a beautiful rose colour. It is resinous in its nature, and burns with a clear flame."—Vol. 2, pp. 522-5.

The remarks on bilin in Dr. Day's introduction require considerable modification, to bring them up to the present state of our knowledge on the subject; the reader, however, who takes an interest in the subject, will find in the Half-yearly Reports a full account of Redtenbacher's Analysis of taurin, where it is proved that this substance contains a large amount of sulphur—a fact which entirely overthrows about one-half, if not more of Liebig's views, as laid down in "The Metamorphoses of Tissues," in the second part of his "Animal Chemistry." Thus, in page 155 of the second edition of the English translation, we meet with the following observation. "As a kind of general view of the metamorphoses of the nitrogenized animal secretions, attention may here be very properly directed to the fact, that the nitrogenized products of the transformation of the bile are identical in ultimate composition with the constituents of the urine, if to the latter be added a certain proportion of the elements of water:

1 at. uric acid	$C_{10}\ N_4\ H_4\ O_6$	=	3 at. taurin	$C_{12}\ N_3\ H_{21}\ O_{30}$
1 at. urea	$C_2\ N_2\ H_4\ O_2$		3 at. ammonia	$N_3\ H_9$
22 at. water	$H_{22}\ O_{22}$			
	$C_{12}\ N_6\ H_{30}\ O_{30}$			$C_{12}\ N_6\ H_{30}\ O_{30}$
1 at. allantoin	$C_4\ N_2\ H_3\ O_3$	=	1 at. taurin	$C_4\ N\ H_7\ O_{10}$
7 at. water	$H_7\ O_7$		1 at. ammonia	$N\ H_3$
	$C_4\ N_2\ H_{10}\ O_{10}$			$C_4\ N_2\ H_{10}\ O_{10}$

In reference to the metamorphoses of uric acid and of the products of the transformation of the bile, it is not less significant, and worthy of remark, that the addition of oxygen and the elements of water to the elements of uric acid, may yield either taurin and urea, or taurin, carbonic acid, and ammonia.

1 at. uric acid	$C_{10}\ N_4\ H_4\ O_6$	=	2 at. taurin	$C_8\ N_2\ H_{14}\ O_{20}$
14 at. water	$H_{14}\ O_{14}$		1 at. urea	$C_2\ N_2\ H\ O_2$
2 at. oxygen	O_2			
	$C_{10}\ N_4\ H_{18}\ O_{22}$			$C_{10}\ N_4\ H_{18}\ O_{22}$
		=	2 at. taurin	$C_8\ N_2\ H_{14}\ O_{20}$
			2 at. carb. acid	$C_2\ O_4$
Add 2 at. water	$H_2\ O_2$		2 at. ammonia	$N_2\ H_6$
	$C_{10}\ N_4\ H_{20}\ O_{24}$			$C_{10}\ N_4\ H_{20}\ O_{24}$

All these calculations being thus shown to be based on fallacious data, it is needless to add that this—the most attractive and captivating part of the whole work—the portion almost invariably quoted as showing the true application of Chemistry to Physiology—at once falls to the ground, and our readers may attach what value they think fit to Liebig's "analytical proof that the nitrogenized products of the transformation of bile, namely taurin and ammonia, may be formed from all the constituents of the urine, with the exception of urea—that is from hippuric acid, uric acid and allantoin;" and they may form their own conclusion as to whether "every doubt regarding the possibility of these changes is removed."

This same error respecting the true composition of taurin considerably influences the great tea and coffee principle, laid down in pp. 179–182, of the same volume, and quoted in all tea-total addresses, and in the numerous lectures and treatises on the vegetables affording food to man.* In addition to Redtenbacher's discovery, we may notice Pettenkofer's test for bilin, and Platner's researches, as important additions to this subject, which have appeared since the publication of the first volume of Simon: the former may, however, be found at page 190, and the latter at page 20 of the second volume, or more fully in Ranking, Vol. 1, p. 341, and Vol. 3, p. 313.

The remainder of the Introduction calls for no special remark; it is brought down completely to the date of its publication, and contains a sufficiently copious description of the constituents of the body to facilitate the perusal of the remainder of the work, without wearying the reader with comparatively useless details. Especial stress is in every case laid on the means of diagnosing the substance under consideration—a point of the greatest importance in the examination of the animal fluids.

In addition to the Introduction, the first volume contains two chapters, the first on *the Proximate Analysis of compound Animal Substances*, the 2d on *the Circulating Fluids*. In Chap. I, we meet with the following observation which we extract, because it affords an useful hint to many young chemists:—"A single isolated analysis is of very little intrinsic value in substances of so varying a nature as the blood and urine. The only method by which we can hope to throw any light upon the leading alterations that occur in these fluids is by the results obtained from a series of analyses: and if we were desirous of merely ascertaining so simple a fact as the determination of the pathological states in which either an excess or deficiency of fibrin and blood-corpuscles occurs in the blood, and the relation that exists between such pathological states and such modifications of the vital fluid, science would be more benefited by the investigation than by the performance of a few very perfect analyses, which did not tend to elucidate any particular point." Simon then proceeds to give a preliminary sketch of the course that should be adopted if a fluid of whose nature we are ignorant, be placed in our hands for analysis.

"Such a fluid may contain,

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* See "Fruits and Farinacea, the proper Food for Man," by J. Smith, 1845—or Dr. Lankester's excellent little work on "The Vegetables yielding Food to Man," forming one of Knight's weekly volumes.

I. The *protein combinations*, fibrin, albumen, casein, globulin.
II. *Pyin.*
III. *Extractive matters:* water-extract, spirit-extract, alcohol-extract, and their proximate constituents.
IV. *Sugars:* Diabetic sugar, and sugar of milk.
V. *Bilin*, with the products of its metamorphoses.
VI. *Urea.*
VII. The *Fats:* olein, stearin, margarin, butyrin, cholesterin, and serolin.
VIII. *Colouring-matters:* the pigments of the blood and bile.
IX. The *acids* of the animal body:
α. Fatty acids.
β. Other organic acids.
γ. Inorganic acids.
X. The *bases* of the Animal body."—Vol. 1, p. 90.

The ten succeeding pages are devoted to the consideration of this subject, and contain an admirable code of laws for the qualitative determination of all the substances mentioned in the above scheme.

The 2nd Chapter is the longest in the whole work, extending over 259 pages, and embracing the consideration of the blood, lymph, and chyle. Following Simon's arrangement, we shall consider—

1. *The general physical relations of normal blood.*
2. *Its general chemical relations.*
3. *Its chemical physiology.*
4. *Its special chemistry.*
6. *Its relations to physiology.*

And 6. *Diseased blood.*

In the section on the general physical relations of the blood, we meet with all that is known regarding its colour, specific gravity and temperature. Considerable additions, especially from the papers of Gulliver and Owen, might have been made to the microscopic analysis of the blood, and we are inclined to believe from the manner in which almost every other part of the work has been completed by the editor, that he was deterred from interfering with this department, in consequence of the Sydenham Society having entrusted Mr. Gulliver with the editing of Hewson's works —a volume in which microscopic observation on the blood would find a more natural place than in a work on physiological chemistry: or it may be that he differed in opinion from his author in this point, and did not see what bearing the size of the blood corpuscles of apes, cats, mice, ducks, geese, and pigeons, had upon "The Chemistry of Man." For our own part we think Dr. Day has done wisely; we only mention the omission because some, into whose hands the work may fall, might deem that all our knowledge of this subject was embraced in pp. 103–4 of Simon. The following remarks on the peculiar forms occasionally presented by blood-corpuscles will be read with interest:—

"The blood-corpuscles do not always present a regular nummular and flattened appearance; they are sometimes plicated and bent in. The cause of this phenomenon is not known, but it is probably due to a contraction of the capsule at different points. One of the most peculiar of these forms is that in which the edge of the blood-corpuscle appears as if it were studded with minute pearls. In the blood of a patient suffering from Bright's disease, I found that nearly all

the blood-corpuscles had undergone this modification. On the addition of a solution of muriate of ammonia, the appearance it presented under the microscope was very striking. I immediately made a counter-experiment with my own blood, but it did not exhibit the phenomenon in question. Ascherson has offered the following explanation of this peculiarity in the form of the corpuscles, viz. that it is due to the exudation of fat which exists in a fluid state in the blood-corpuscle. In opposition to this view, it may be urged, that if each individual corpuscle contained a separable portion of fat (however minute it might be), we should obtain in our analyses a much larger quantity of fat than in reality we do. It is true that the dried clot yields a larger proportion of fat than an equal weight of serum, but the difference is by no means so striking as it would have been if Ascherson's theory were correct. Hünefeld has observed a similar appearance on treating the blood-corpuscles of the frog with putrid serum, in which granules were present. The granules seemed to form a sort of girdle round the corpuscle, and he conceives that they penetrated into minute depressions upon the surface of the capsule. If this statement be correct, it is strongly opposed to the observations of Ascherson and Wagner respecting the lubricity and evenness of the blood-corpuscles. On mixing the blood of a carp with a solution of sugar, and on the cautious addition of water, I observed that the blood-corpuscles assumed a stellar appearance. On treating frog's blood with bilin, an agent which usually dissolves the corpuscles, I observed that some of them resisted this action for a considerable period, and ultimately assumed a pyriform appearance, while others became narrowed at the centre, and extended at both extremities. Others, again, seemed to undergo an internal change, and appeared as if their inner surface were studded with minute vesicles. Hünefeld made a similar observation on treating frog's blood with carbonate of ammonia. The same chemist observed a remarkable peculiarity in the corpuscles of human blood, on the addition of sulphate of quinine. In the course of a few minutes they assumed an irregular, angular form, and appeared as if their sides were drawn together. Schultz has made the following important microscopic observation. On examining the blood-corpuscles of a salamander which had been suffocated in carbonic acid gas, they were found to be of a darker colour than usual; the darkness was particularly marked on some spots, so that they exhibited a sort of chequered appearance. On shaking the blood with oxygen gas, the corpuscles became brighter and more transparent."—Vol. 1, p. 105–6.

We now arrive at the second section—*on the general chemical relations of the blood*. The chemical relations of the corpuscles are first considered. The principal labourers in this department are Müller, Schultz, Hünefeld, and Simon, who have observed under the microscope the apparent effects produced on them by the addition of numerous chemical and medicinal agents. There can be no doubt that enquiries of this nature, properly conducted, must ultimately lead to important practical results. With regard to the colouring matter, Hünefeld believes that it exists in an insoluble form, attached to the inner surface of the capsule. Simon opposes this view; he observed that it "is contained in all probability in a state of solution in the corpuscles, an opinion which is also supported by Müller, Schultz, and Reichert. If, on the examination of frog's blood, one corpuscle be observed to move over another, the lower can be distinctly perceived through the upper one. Moreover, the instantaneous solution of the corpuscles by means of bilin, supports this view, for, if their contents were gelatinous or solid, the act of solution would be observed to progress from the circumference to the centre, and would admit of being observed by the microscope." The general chemical relations of the nuclei, and of

the *liquor sanguinis*, call for no special remark, neither do the observations on the retardation and acceleration of the coagulum.

The *chemical physiology of the blood* forms a highly important section, and we shall devote some space to its consideration.

The formation of the blood and the *forces that circulate it*, belonging strictly to physiology, we proceed to *the process of respiration*. Dr. Day has made extensive additions to this department, incorporating the recent investigations of Andral and Gavarret on the quantity of carbonic acid exhaled *per horam*, those of Scharling on the same subject, and those of Brunner and Valentin on the composition of expired air. From the experiments described in the text it follows "that the quantity of carbonic acid expired is very variable, and that it may be altered by many circumstances. Hunger and rest diminish, satiety and labour increase it. It is greater during the day than during the night, in the proportion of 1.237 to one. If the expired carbonic acid be estimated in relation to the weight of the body, it is found that children give off a proportionally greater amount of this gas than adults. In some forms of disease, the amount of expired carbonic acid falls below the standard; it seems in a state of health, to vary directly with the activity of the circulation." In connexion with this subject the reader would do well to consult Hannover's Memoir on the Amount of Carbonic Acid exhaled in Disease, or the notice of it in Day's Report on Chemistry, in the last volume of Ranking's Abstract. A considerable space is devoted to *animal heat, and the changes undergone by the blood;* and although we are not prepared to express our conviction of the entire accuracy of the more minute details, we cannot refrain from recommending this section to the attentive perusal of our readers. We regard it as the most succeessful attempt yet made to apply chemistry to the elucidation of animal physiology. Simon evidently attached much importance to this portion of his work, as he especially alludes to it in his preface, where we find his views on this subject summed up in the following terms:—

"The blood is subjected to a continuous metamorphosis, which may be regarded as the expression of its vitality. The nutrition of the peripheral system is effected by the liquor sanguinis, not by the blood-corpuscles. The liquor sanguinis affords nutriment to the cells and organs, which possess an inherent power of selecting proper material, or of forming it from non-homologous matter, at the same time secreting the products of decomposition. The principal nutritive matters in the liquor sanguinis are albumen, fibrin, and fat. The chief products of this metamorphosis are the extractive matters and lactic acid, which occur in the excretions, especially in the urine. Urea, bilin, and carbonic acid are either not products of the metamorphosis of the blood during the act of nutrition in the peripheral system, or at most they are only in part formed by it. They must be regarded as products of the vital energy of the blood-corpuscles, which doubtless possess the same power of attracting nutriment, and of throwing off decomposed products as other living cells. The proper nutriment of these corpuscles is oxygen, albumen, and probably also fat, which are furnished them by the liquor sanguinis. The most important products of their metamorphosis are carbonic acid, urea, fibrin, extractive matters, and very probably some of the constituents of the bile. The leading and most important object of this vital energy of the blood corpuscles is the production of animal heat, without which every function of the organism, nay, even life itself, would be instantaneously annihilated. The production of animal heat is due to the combination of oxygen

with the carbon of the globulin; the principal products of this reaction are carbonic acid and urea, or uric acid, (which is excreted as a substitute for urea in most of those classess of animals in which elliptic blood-corpuscles occur.) The urea excreted may thus be regarded as a measure or equivalent of the animal heat developed. The production of blood-corpuscles and the formation of blood are intimately connected with nutrition; when the food is too scanty and insufficient, the amount of blood, and especially of blood-corpuscles, is diminished; when the nutriment is proper and abundant, the reverse takes place. In the former case, therefore, the vital energy is depressed, the secretions and excretions are diminished, and the animal heat sinks; while in the latter case, exactly the reverse is observed. In the normal state there is an equilibrium preserved between the production and consumption of blood-corpuscles. The food is prepared, and to a certain extent assimilated, before it enters the blood. The vital energy of the blood-corpuscles continues even during a perfect abstinence from food, and carbonic acid and urea continue to be formed, although their amount gradually diminishes in a direct ratio with diminution of the blood-corpuscles. Moreover, the amount of carbonic acid and the formation of urea are lessened by a torpid, and increased by an excited circulation; and in proportion to the amount of corpuscles and to the rapidity of the circulation, so much the higher is the animal temperature. Thus in birds we observe a high temperature, and the reverse in the amphibia. In chlorotic, and also in very aged persons, we find a low temperature, and a diminished excretion of urea; while in inflammatory diseases, and after prolonged corporeal exertion, the temperature rises, and there is either a relative or an absolute increase of urea, in the former case, even in the absence of all nitrogenous food. The capillary and cutaneous systems tend to regulate an excessive rapidity of the circulation, and to prevent the animal heat from exceeding a certain limit. If we only knew whether, and in what manner, the pulmonary exhalation is changed in various diseases, (especially in relation to the amount of carbonic acid contained in it,)—whether the carbonic acid always increases relatively with the urea or in certain cases with the uric acid—and if further we possessed experiments illustrative of the effects of diseases, and of varied diet on the bile, we should then have a more solid basis than we now occupy, on which to found our chemical enquiries, while the acquisition to the science of medicine would be positive and incalculable. The questions here involved must however, unfortunately, at the present time, be regarded as unanswerable. We cannot doubt that the pulmonary exhalation does vary, under different circumstances, in the amount of carbonic acid; for instance, more carbonic acid is exhaled during prolonged corporeal exertion than when the body is in a state of repose, although, as far as I am aware, no experiments on this subject have yet been instituted. We have, however, conclusive evidence that the amount of urea is increased under these circumstances."—Vol. 1, pp. xi—xiv.

We now arrive at *the special chemistry of the blood.* The various methods of analysing this fluid are given at considerable length; we have the methods of Berzelius, Lecun, Denis, and Simon; and to these Dr. Day has added the method of Figuier, which we regard from its simplicity, as the best in all ordinary cases when we wish to determine only the most important constituents. The method adopted by Andral and Gavarret for analysing coagulated blood, is given further on, in the section on diseased blood: we cannot see why it should be separated from Simon's method given in p. 190. The method of Figuier is based on the fact that, after the addition of a neutral salt to defibrinated blood, the globules do not (as before) pass through filtering paper. On the addition of two parts of a solution of sulphate of soda of spec. grav. 1.130 to one of blood, Figuier found that the whole of the corpuscles remained on the surface of the

filter. The following are the steps of his analysis. The fibrin is removed by whipping, dried, and weighed; the weight of the corpuscles is ascertained by the method indicated; and that of the albumen by coagulating, by means of heat, the filtered solution. The proportion of water is known by evaporating a small portion of blood whose weight has been previously determined.

We proceed to "*The healthy blood in relation to physiology.*" The distinctions between arterial and venous blood are thus summed up:— "Arterial blood contains less solid residue generally than venous blood: it contain less fat, less albumen, less hæmatin, less extractive matters and salts than venous blood. The blood-corpuscles of arterial blood contain less colouring matter than those of venous blood." Dr. Day adopts Mulder's theory to account for the difference of colour between arterial and venous blood. The oxides of protein formed in the act of respiration soldify round each corpuscle, making the capsule thicker and better qualified to reflect light. Each corpuscle of arterialized blood is thus invested with a complete envelope of buffy coat, which gradually contracts and speedily forms cupped or bi-concave surfaces favourable to the reflection of light. On reaching the capillaries the coating of the oxides is removed for purposes connected with the nutrition of the organism, and the corpuscles losing their opaque investment and their cupped form, can no longer reflect light, and the blood assumes a venous tint.

The peculiar characters of the blood of the portal, hepatic, and renal veins are then noticed, and we regret that want of space precludes us from quoting the important physiological results deduced from the analysis of these varieties of the circulating fluid.

Simon has made two analyses of healthy venous blood, yielding the following results:—

In 1000 parts there were contained:—

	A Male aged 17 years.	A Female aged 28 years.
Water	791·900	798·656
Solid residua	208·100	201·344
Fibrin	2·011	2·208
Fat	1·978	2·713
Albumen	75·590	77·610
Globulin	105·165	100·890
Hæmatin	7·181	5·237
Extractive matters and salts	14·174	9·950
Colouring matter in 100 parts of blood-corpuscles	6·3	5·2

From these analyses it appears that healthy blood "contains about 30$\frac{0}{0}$ of solid constituents; not much more than 0·2$\frac{0}{0}$ of fibrin, and about an equal quantity of fat; the blood-corpuscles considerably exceed the albumen in quantity, and contain about 5·7$\frac{0}{0}$ of colouring matter." The analyses of Lecanu and Denie are then quoted, and the editor has added others by Nasse, Becquerel and Rodier, Marchand and Enderlin. Passing over a few pages devoted to the consideration of the effects of sex, temperament, and age on the blood, we arrive at its *pathological conditions*. The extent to which the different constituents may vary is forcibly shown in the following table, drawn up solely from the original analyses of Simon.

The water may vary from . . .	880·0 to	750·0
Fibrin	9·1	a trace.
Fat	4·3	0·7
Albumen	131·0	50·1
Globulin	106·6	30·8
Hæmatin	8·7	1·4
Extractive Matters and Salts . .	16·5	7·6

Moreover these must not be regarded as the extreme limits. Thus Andral and Gavarret found the fibrin amount to 10·5; Rindstropf to 12·7 (Simon p. 262), and Popp in a case of articular rheumatism found it as high as 13·3 (Day's Report in Vol. 3. p. 307 of Ranking's Half-yearly Abstract.) Diseased blood is arranged in four divisions:—

1. *Hyperinosis* (υπερ and ις, ινος fibre) in which there is an excess of fibrin and a corresponding diminution of the corpuscles.

2. *Hypinosis*, in which the quantity of fibrin is less than in healthy blood, or if it amounts to the normal quantity, its proportion to the blood-corpuscles is less than in a state of health.

3. *Spanæmia* (from σπανος poor, and αιμα blood), in which the blood contains an excess of water, no constituent exceeding the normal average.

4. *Hetero-chymeusis* (from ἑτερος and χύμευσις chylification) which includes those states of the blood in which a substance is present that does not exist in the normal fluid—when, for, instance, the blood contains urea (in appreciable quantity), sugar, biliphæin, fat, or pus. The last division is altogether artificial and merely adopted for convenience, since the only common property possessed by this class is, that the composition of the blood is here qualitatively changed, whilst in the three others it was merely altered quantitatively.

This is undoubtedly the most suggestive portion of the work. It is an enormous collection of isolated facts from which many valuable deductions of a practical character may be drawn. A reference to the description of the blood in chlorosis will sufficiently illustrate our meaning. A supplement embraces the following subjects:—blood during pregnancy, menstrual blood, the lochial discharge, and the blood of animals. It extends over fifteen pages, and is almost entirely from the pen of the English editor. The chapter, and with it the volume, terminates with the consideration of the lymph and chyle. It contains, we believe, all that is known on the subject, with the exception of Gulliver's Microscopic Investigations, but still leaves the subject in a very unsatisfactory and imperfect state.

The Third Chapter—the commencement of the 2nd volume—is devoted to the consideration of the *secretions of the chylopoietic viscera, and the theory of digestion*. There is nothing in the observations on the saliva, or pancreatic fluid requiring comment. The bile is described at considerable length, and Dr. Day has added the following remarks on its uses.

"That the bile is not merely an excrementitious fluid, intended to remove effete matters from the blood, but that it is a secretion essential to the animal economy, was rendered almost certain by the experiments of Berzelius, Theyer and Schlosser; which showed that the human fæces contained much too small a quantity of a substance resembling bile to justify the idea, that it is evacuated in this manner. A further proof that the bile is absorbed, and not excreted, is afforded by an examination made by Enderlin, of the ash yielded by the contents of the different

portions of the intestinal canal of a hare. He found that the ash from the contents of the duodenum *alone* effervesced on the addition of an acid, thus showing that the choleate of soda (which yields the carbonate on incineration) is absorbed before reaching the jejunum. Schwann has recently established this opinion beyond a doubt, by a series of well-devised experiments on dogs. He tied the ductus communis choledochus, and at the same time formed a fistulus opening in the gall-bladder, by which the bile escaped externally. His most important conclusions are —1st, That when the bile does not get into the bowel, its absence is generally perceptible in dogs about the third day by a marked diminution in weight; and 2ndly, That unless the channel for the conveyance of bile to the duodenum is re-established, symptoms of deficient nutrition, wasting, debility, &c. ensue, and death is the ultimate consequence."

Few fluids have recently attracted more attention at the hands of chemists than the gastric juice, and very few have given rise to more discordant results. Prout showed that it contained free muriatic acid. Tiedmann and Gmelin found both acetic and muriatic acids, and there is no doubt, adds Simon, that lactic acid likewise occurs in it. M. Blondlot has recently published a treatise on Digestion detailing numerous experiments made on dogs, in whom fistulous openings into the stomach were maintained for upwards of two years. His conclusions, deduced from a large number of experiments, is that the acid secretion of healthy gastric juice is owing to the presence of superphosphate or biphosphate of lime, and that the fluid contains no free acetic, hydrochloric, or lactic acid. M. Blondlot's views were speedily attacked by MM. Bernard and Barreswil who "conclude that the acid reaction of the gastric juice is not owing to biphosphate of lime, but arises from a free acid, which is not hydrochloric or acetic acid." They have always found lactic acid with a minute proportion of phosphoric acid; the latter being a product of the reactions of the lactic acid on the phosphates present. M. Melsens has also examined the gastric juice, and denies the accuracy of Blondlot's conclusions. The latest experiments on the subject are those of Dr. R. D. Thompson. In order to prevent complication of the phenomena, the animals (pigs) were fed on vegetable food alone. His experiments tend to show that no free hydrochloric acid is present in the stomach of animals living on vegetable food, but that the free acid is the lactic. A little acetic acid was also generally present.—*Day's Report on the Progress of Chemistry*, vol 2, p. 347-51.

It appears, from an observation in a recent number of Heller's Archiv., that Dr. Mack of Vienna is at present engaged in the analysis of the gastric juice and the chemistry of digestion, a patient with gastric fistula affording him ample opportunity for the investigation. It is most sincerely to be wished that his labours may settle the point beyond all further dispute.

The next chapter treats of the Milk, and we believe we may add contains everything known upon the subject. Simon observes that a metastatic or vicarious secretion of this fluid is by no means rare. Dr. Day quotes two very singular illustrative cases—the most convincing we ever met with, since the fluid contained sugar of milk, butter, and casein. In one, the liquid was contained within the coats of the testicle (Vol. 1, p. 65); in the other, it flowed from an abscess in the thigh of a woman who was suckling (vol. 2, p. 521); he has likewise inserted an analysis of the mammary secretion of a *he-goat*, (vol. 2. p. 65.)

The following is Simon's description of the colostrum—the milk secreted immediately after delivery.

"On examining it with the microscope a very large number of fat globules are seen (some of which are larger than those that occur in ordinary milk) and these are frequently observed clinging to one another; besides these there are granulated, yellow, roundish corpuscles, larger than the milk-corpuscles, which appear to be composed of very minute fat vesicles; they seem to be peculiar to the colostrum, and were first observed by Donné, who states that they occur in woman's milk till the twenty-eighth day, when the milk loses all the characters of colostrum; I have never succeeded in detecting them after the eighth or tenth day."

From comparative analyses of the colostrum and healthy milk of the same woman, it appears that the former is much the richer of the two in solid constituents, especially in butter and sugar of milk.

The absolute quantity of the casein is also greater, but the ratio of the casein to the solid constituents is less than in ordinary milk. The salts are also increased. The aperient property of the colostrum is probably dependent on the increased quantity of salts and sugar of milk.

The characters of ordinary human milk, the effect of temperament, the changes dependent on nutrition, the changes corresponding with the age of the infant, the modifications produced by disease, and the effects of medicines on the milk, are then considered; and the chapter terminates with the "milk of animals."

In the Appendix (vol. 2, p. 521) we find a notice of some very important analyses recently instituted by Dumas. He shows that, after dogs have been restricted for some time to a purely animal diet, all traces of sugar disappear from the milk.

These experiments bear very forcibly on the much disputed question of the existence or non-existence of lactic acid in the animal fluids.

Chapters V. and VI. embrace the consideration of the secretion of the mucous membrane and the external skin; namely, mucus, pus, and sweat.

The following analysis of arthritic pus is interesting, as showing the general predominance of uric acid in the body in cases of gouty diathesis.

"I have examined the dried residue of the liquor puris of an arthritic person; it was of a grayish yellow colour, containing no membranous shreds, could be easily pulverised, and exhibited no appearance of crystals when examined under the microscope. On heating it with nitric acid, I obtained, after the evaporation of the acid, and more strikingly on the addition of ammonia, a brilliant purple colour, indicating the presence of uric acid beyond a doubt. On triturating this substance with water I obtained a pulpy mass, which, when examined under the microscope, was found to contain numerous epithelium-cells and pus-corpuscles, but no crystals of uric acid. Alcohol extracted $5{\cdot}4\frac{0}{0}$ of fat, consisting chiefly of margaric and oleic acids with a little cholesterin; boiling water took up $52{\cdot}6\frac{0}{0}$, of which a little fat and extractive matters, with hydrochlorate of ammonia and lactate of soda, were soluble in anhydrous alcohol; and chloride of sodium, extractive matter, and albuminate of soda, in spirit. The remainder was washed with cold water (which extracted very little) and was then dissolved in a faintly alkaline solution. On the addition of hydrochloric acid to this alkaline solution, crystals of uric acid were deposited, and some albumen thrown down from the albuminate of soda: the acid solution then contained hydrochlorate of ammonia and chloride of sodium. The portion insoluble in water yielded on incineration

5⅝ of ash, consisting of earthy phosphates, with a little peroxide of iron and carbonate of soda; the dried residue of the liquor puris yielded, however, 10⅝ of ash, composed of carbonate of soda, a little phosphate of soda, carbonate and phosphate of lime, a little chloride of sodium, and traces of peroxide of iron."—Vol. 2, p. 95.

It is not a great many years ago since *pus-tests* were in great fashion; and, when the occurrence of pus in the expectoration was regarded as diagnostic of phthisis, extreme importance was attached to the difference between it and mucus. These tests were mostly based on the chemical relations of the corpuscles towards various re-agents; thus, Grasmeyer's and Donné's tests were founded on the action of the alkalies and their carbonates on pus. Güterbock's test was based on the circumstance that pus, being fatty, burned with a clearer flame than mucus, and there were other tests by Gruithuisen, Young, and others. None of these tests are to be altogether trusted, and the microscope renders them superfluous. If this instrument reveals their apparent presence, and on the addition of acetic acid the corpuscles disappear, leaving only cup-formed nuclei, we may be sure we are examining pus.

The Seventh Chapter extends over 240 pages, containing by far the most perfect history of the urine in health and disease, that has yet appeared in any language. It would lead us far beyond our proper limits to attempt a condensation of this part of the work. It commences with the physical characters and the qualitative and quantitative analysis of the healthy urine. In addition to the analysis of Berzelius, we find five by Simon, three by Lehmann, and others by Becquerel, Marchand, and Day. The physiological relations of the urine are then considered; these are chiefly based on the researches of Liebig on the influence of the salts contained in the food upon the composition of the urine, on those of Lehmann on the influence of diet in this secretion, and on those of Lecanu on the influence of age and sex. The greater portion of this matter has been introduced by the English editor.

We now arrive at the pathological relations of the urine. The chemical changes may be reduced to one of the following forms:—

1. One or more of the normal constituents existing in larger quantity than in healthy urine.
2. One or more of the normal constituents existing in less quantity than in healthy urine.
3. A normal constituent absent.
4. The presence of substances that do not exist in normal urine.

The substances noticed by Simon under the fourth head, are albumen, blood-corpuscles, fibrin, fat, chyle, casein, biliphæin, bilin and bilifellinic acid, sugar, carbonate of ammonia, oxalate of lime, carbonate of lime, cystin and pus.

The constitution of the urine in different diseases is now considered, no less than 117 pages being devoted to this subject. We select the following observations regarding the determination of the sugar and urea in diabetic urine.

"On treating diabetic urine evaporated to the thickness of a syrup with warm spirit, the mucus, uric acid or urates, and earthy phosphates are precipitated. On evaporating the filtered spirituous solution to the consistence of a thin syrup, and adding anhydrous alcohol, an insoluble semi-fluid mass separates, which, when repeatedly treated with anhydrous alcohol, becomes finally thick and tough.

On dissolving this saccharine mass in warm spirit, and again precipitating it by anhydrous alcohol, it will still be found to contain a certain amount of urea; in fact, I have detected urea after the operation has thrice been effected, and I find that sugar can only be obtained from urea by allowing it to crystallize spontaneously from its spirituous solution. In consequence of the difficulty of separating these substances, I proceed in the following manner: the solid residue of the urine is first accurately determined; a weighed portion of urine is then evaporated, mixed with spirit, and the solution filtered. The filtered solution is evaporated to the consistence of a syrup, and, when cold, mixed with a sufficient quantity of concentrated nitric acid to allow of a few drops remaining on the surface of the crystalline mass. It must be submitted to a low temperature, and the crystals placed on blotting paper and compressed till they cease to communicate moisture. The fixed salts must be determined from a separate portion of urine. If we deduct from the known quantity of solid residue the portion insoluble in spirit (from which the uric acid is determined), the urea, and the fixed salts we obtain, as the difference, sugar and alcohol-extract which appears to decrease in diabetic urine in proportion as the sugar increases."—Vol. 2, pp. 297-8.

Urines containing fat, milk, an excess of hippuric acid (of which Dr. Day has collected three singular cases), urostealith, and semen are then considered, and these are succeeded by observations on "urine of peculiar colours," "urine during pregnancy," and on "the passage of medicinal and other substances into the urine." The chapter terminates with "the urine of animals."

There is nothing in Chapters VIII. and IX. to call for especial remark. The former is devoted to the consideration of the secretion of the lachrymal meibomean and ceruminous glands; the latter, to the secretions and fluids of the generative organs, including the seminal and prostatic fluids, the amniotic and allantoic fluids, and the vernix caseosa.

The tenth Chapter treats of the intestinal excretions. We have full accounts of the meconium and the fæces of infants and of adults, in health and in disease. The diseases especially noticed as influencing the fæces are diabetes, dysentery, enteritis, abdominal typhus, diarrhœa, cholera, enteropthisis and jaundice. The effects of calomel on the evacuations are then considered; and the chapter terminates with the chemistry of vomited matters.

The component parts of the animal body form the subject of the Eleventh Chapter. The article on bone is re-written by the Editor, the matter being chiefly taken from the recent work of Von Bibra on the subject. Teeth, cartilage, cellular tissue, tendon, ligament, skin and hair are then considered. From the observations of Van Laer it appears that the hair consists essentially of:—

"(1). A connecting medium consisting of a tissue yielding gelatin, and represented by the formula $C_{13}\,H_{10}\,N_3\,O_5$; and (2), Of bisulphuret of protein, $C_{40}\,H_{31}\,N_5\,O_{12}\,S_2$.

"The large amount of sulphur in hair (averaging $5\frac{1}{2}$) is the cause of its colour being affected by various metallic salts. As there is no constant difference in the results obtained by the analysis of hair of various tints, it is to be presumed that the colour is dependent on peculiar arrangements of the ultimate particles."—Vol. 2, p. 419.

In the crystalline lens, Simon finds, in addition to albumen, a peculiar substance closely resembling casein, to which he applies the term crys-

tallin. • When the lens becomes opaque (in cases of cataract), it is found to contain an excess of phosphate of lime.

The muscles next claim our attention. Simon observes that, "on making incisions in the warm flesh of an animal just killed, we obtain by pressure an acid fluid, which rapidly coagulates in consequence of the presence of a little fibrin: if the flesh has been kept for some time, the fluid obtained by pressure no longer coagulates, although it exhibits an acid re-action." We quote these lines to draw the attention of our chemical readers to the nature of this acid. Hoffman (op. cit., p. 13, note) observes, that from this acid fluid he obtained a zinc-salt, having the crystalline form of the lactate of zinc. The obscurity which rests over the question regarding the existence or non-existence of lactic acid in the animal body, is disgraceful to the science of chemistry. The brain, spinal cord and glands occupy the remainder of the chapter.

The two remaining chapters are devoted to solid and fluid morbid products. The former contains full instruction for the qualitative and quantitative analysis of the various concretions occurring in the animal body, together with a good account of their physical and chemical characters; it likewise embraces the chemistry of tubercle, scrofula and cancer, in so far as it is yet known. The latter contains analyses of the fluids in various cysts, the fluids of pemphigus, hygroma, hydrocephalus, ascites, hydrocele, thoracic effusions, the subcutaneous serum in Bright's disease, and the fluid effusions found in the body after death.

There are two Appendices, the former containing the ultimate composition of the proximate constituents of the human organism, the latter embracing the most important additions to animal chemistry made during the course of publication.

The value of the work is much increased by three accurate and well-engraved illustrative plates, and by a very copious and most useful index extending over eighteen pages. The length to which our review has extended, and the numerous extracts we have made, afford sufficient evidence of our opinion respecting its great merits. It is in the highest degree creditable to the Author, the Editor, and the Sydenham Society. We believe that the publication of this work, and of Hasse's Pathological Anatomy (noticed in our last number), will tend—provided always the re-printing of some of the standard old medical works be not forgotten—to instil into the minds of their medical brethren a confidence in the proceedings of the Council, which some of their acts have gone far to undermine; and we most sincerely trust that this may be the case. We cannot, however, help remarking upon the singularity of the fact, that for both these works we have been indebted to the suggestions of their respective editors, and not to any intimate knowledge of their contents by the twenty-four learned gentlemen forming the aforesaid body.

Of Dr. Day's Reports on the Progress of Chemistry in relation to Physiology and Pathology, we need merely add that they are written in the same spirit of research and impartiality which characterises his extensive additions to Simon. Every medical man is expected to be conversant with the recent advances of physiological and pathological chemistry, and these reports afford the only means in the English language of meeting these expectations. We regard them as one of the most important features in Ranking's Half-yearly Abstract.

Hoffman's Elements are clearly written, but present no claim to originality. His principal fault lies in the undue importance he attaches to subjects of little intrinsic value, or, at all events, hardly bearing, even in a remote degree, on Animal Chemistry; thus, nearly six pages are devoted to the glands—for the most part to their development and uses; while the space occupied by the blood—the most important subject in the whole work—is considerably less than ten pages.

Of the manual of Dr. Griffith, his own title-page (a work we opine of no small labour) gives a full, and we may add, a true and correct account. It is the only portion of the book that would bear much abbreviation; indeed, in many points, we think that the system of condensation has been carried a little too far. The work commences with the protein and gelatinous compounds, and the extractive and fatty matters. We have already (see page 482) had occasion to notice his introduction of a *new* formula for protein; we must now take him to task for the opposite fault—for retaining *obsolete* chemical opinions. In page 15 we find that "gelatine occurs in the bone, skin, serous membranes, cellular tissue, tendons, ligaments, and ossified cartilages." Gelatine no more *occurs* in these structures than does tritoxide of protein in albumen. *It is obtained* from them by ebullition.

Organic acids next claim our attention—lactic, acetic, hydrocyanic, formic, benzoic, oxalic, oxaluric and tartaric acids. This list is rather more extensive than need be; the development of hydrocyanic acid in the animal body is more than doubtful; formic and oxaluric acids have never been discovered in any of the secretions, and benzoic acid, when present in the urine, is a result of decomposition; and regarding tartaric acid, which we are told, "has been found in diabetic urine in combination with lime," we would suggest that the diabetes had nothing to do with the question, further than that the patient, being thirsty, probably drank *imperial*—a solution of bitartrate of potash—which would fully account for the phenomenon in question.

The rules for the detection and separation of the *inorganic matters* are concise, and at the same time clear; we would, however, observe, that Dr. Day has given a more delicate test for the presence of carbonic acid (Simon, vol. ii. p. 120) than Dr. Griffith. It would perhaps have been better not to have alluded to the supposed discovery of titanic acid in the renal capsules. There can be little doubt that there was some fallacy in the experiment, and even if it really were there (and from the frequent association of titanium with iron, the thing is not altogether impossible), it must have been altogether an accidental occurrence, and would have afforded no just grounds for the statement that the renal capsules contain titanic acid more than any other part of the body. Dr. Griffith's manual shows that he is well acquainted with the processes he describes; and if, in several points, he has not carried his subject to quite the latest date, his readers may at least be assured that he will never lead them into serious error. Considering the limited space he had at his command, most of the subjects he treats of are handled in a very satisfactory manner. We have, therefore, pleasure in recommending his work as a useful introduction to the more elaborate writings of Simon and Day.

I. THE WHY AND THE WHEREFORE, OR THE PHILOSOPHY OF LIFE, HEALTH, AND DISEASE. By *Charles Searle*, M.D. M.R.C.S.E. 8vo. pp. 266. Churchill: London, 1846.

II. LIEBIG'S PHYSIOLOGY APPLIED IN THE TREATMENT OF FUNCTIONAL DERANGEMENT AND ORGANIC DISEASE. Part I. By *John Leeson*. 8vo. pp. 216. Renshaw: London, 1846.

III. HOMŒOPATHY, ALLOPATHY AND YOUNG PHYSIC. By *John Forbes*, M.D. F.R.S. 12mo. pp. 121. Philadelphia Edit. 1846.

WE place the titles of these publications together at the head of the present article, not because they treat of any subject in common, or resemble each other in either object or execution; but inasmuch as each of them, although in very different degrees, furnishes us with the opportunity of animadverting upon the tendency, by no means rare at the present time among medical writers, to pay undue homage to the advantages derivable from various forms of quackery, and to most unfairly depreciate those which the legitimate practice of physic has at its command.

Charlatanism always has existed, and probably always will prevail in the world; and certainly, in spite of our advance in a *quasi* education, which furnishes the learner with the means of perusing, without the power of judging of the value of the lucubrations of pretenders, at no period of time has it ever been, professional and non-professional, more rife than at the present. One remarkable feature in the history of its modern progress as distinguished from that of former times deserves our especial notice. Heretofore, quackery received no favours, or even quarter, at the hands of the regular members of the profession. In fact these, in the shape of the various authorities in the different parts of Europe, as Faculties, Universities, Colleges, &c., prosecuted an open war against quacks with unrelenting vigour and energy. We do not say that such proceedings did not occasionally lack discretion; for, by their indiscriminate, and therefore sometimes unjust and vindictive character, they often had the mischievous effect of arousing public sympathy in favour of the proscribed, furnishing a notoriety which was the one thing desired. The very fact of the existence of such errors, however, proves the utter absence of connivance at, or encouragement of, these irregular proceedings on the parts of those who had been enrolled members of an important and highly responsible profession. How stands this matter at the present time? Our incorporated medical bodies, hypothetically supposed to represent the interests, the honour, and the dignity of our profession, neither possess, nor have shown themselves desirous of possessing, the requisite power for the suppression of quacks and impostors of any kind, however flagrantly these may invade the rights of educated practitioners, or, however widely they may spread abroad their desolating mischief amid the community. More than this: they do not even reprove, discountenance, or expel any of their own members who encourage, or even take part in, any of the impostures of the age.

But, besides this apathy and indirect encouragement thus offered to impostors by the corporate bodies, various individual members of the

profession have, from time to time, by act, word, or book, added not a little to such encouragement, and to the perpetuation of the delusions prevailing in the public mind upon the subject. There is, perhaps, scarcely a quack medicine in existence which has not at one time or another been prescribed by members of the faculty, and *proh pudor!* sold by others of them; while each of the arch-quackeries of the present day, whether it be mesmerism, homœopathy, or hydropathy, has received open encouragement from members of the profession, whose acquirements and prior position taught us to expect better things at their hands. And, whether it arises from the contagious influence of a few great names, the involvement of the medical mind in, or its submission to, the fashionable delusions of the day, or its easy gullibility by specious appearances and unauthenticated facts, we know not; but certain it is that it is as common as possible to find medical writers furnishing their more or less complete approval of certain forms of quackery, and deprecating the treatment which is pursued by the *non-illuminati*. For, let it be observed that, approval of the new schools and abuse of the old one, proceed *pari passu*, and that eloquent tirades upon the evils inflicted by the demon of polypharmacy, and cutting sarcasms upon the presumptuous and dangerous individuals who would fain endeavour, by means of some poisonous, horrid, filthy drug, given too in imaginable quantities, to cut short the course of a disease which should have been left to Nature's all-restorative power, are standing topics of discourse with this class of writers. The works we have in hand contain specimens of this injudicious procedure, one or two of which we proceed to exhibit. Dr. Searle, after stating what he considers to be the two principles of the operation of *hydropathy*, [viz. 1, the purification of the blood, and the reduction of præternatural excitement—and 2, the exciting and giving tone to the skin, and thus invigorating the entire system] goes on to say that, he considers it a most useful practice in gout, rheumatism, scrofula, various cutaneous diseases, dyspepsia, general debility, &c.; and adds,

"Although open to some objections, it assuredly embodies principles which, if judiciously carried out, are of most useful application in the treatment, not only of the large class of affections previously adverted to, but many others also; inasmuch as it, in an eminent degree, developes and augments all the natural powers of the system—and these are the only curative means in any case. The hydropathic practice, it will be well to observe, embodies in it the system of the trainer. * * * * * * *

The system of Preissnitz, I am however bound to say, does more than this, as it includes, in addition to active exercise in the open air, the mind's repose by abstaining from all active mental employ; with abstinence also from all warm fluids and exciting beverages, a plain, wholesome diet of meat, bread, vegetables, fruit, and cold water, being only allowed, with early rising, and early return to bed; and last, though not least in amount of beneficial consequences, preserving the blood from contamination and further deterioration, by excluding physic altogether! or when the blood is so contaminated, or by other causes deteriorated, as we have previously spoken of as being the case in gout and many other affections, purifying and diverting it thereof, by washing out the impurity with cold water, and expelling it from the system through the agency of the skin and kidneys.

"This system, I must avow in conclusion, embodying as it assuredly does, if not all the requirements of a complete system of treating disease, yet including so many pre-eminently useful principles, deserves most undoubtedly great con-

mendation; and it is not only puerile, but discreditable, to say in disparagement of its author, although but a peasant, that he has taught nothing that was not known before: inasmuch as it is not what may be learnt, but what is practised by man, that constitutes desert.—P. 264.

Mr. Leeson thus favours us with his opinion upon the abuse of drugs in this country, and the advantages of *homœopathy*.

"The indiscriminate employment of drugs of the most offensive and drastic kind, of mineral compounds of the most poisonous description, and of the metallic salts, irritating and violent in their action, by the old and by many of our modern practitioners in medicine and surgery, often led, as they do now, to the most disastrous results; while those actions upon the body, the result of surrounding influences, and the principles of restoration by nutrition, and through the science of the chemical forces, were entirely unknown to them, or not respected sufficiently to be subjects for their consideration. These systems of treatment in themselves were sufficient to derange the healthy structure, or to convert simple cases into dangerous maladies.

"Wounds, bruises, ulcers, glandular irritation, inflamed textures, amputated surfaces, with all other such cases, were greatly aggravated, if not brought on to fatal results, by such treatment. The homœopathic or do-nothing system, or the plan of letting diseases take care of themselves, would gain for the charlatan or impostor the highest reputation; whilst the character of the legitimate practitioner would sink in public estimation, were he to pursue the plans which are often taught him under the direction of his Halls and Colleges.

"There are about 410 preparations in the Pharmacopœia of the Royal College of Physicians, which no doubt are considered by that learned body useful for medicinal purposes, or else they would not have been retained there. It is from this collection of preparations that the medical youths of this country are instructed to cull their remedies and apply them in the treatment of every form of disease. Now, any practical man, of 10 or 20 years' standing, must have found that 400 of these preparations are of little or no value whatever in the treatment of any form of disease, and that about the remaining 10 might have assisted him in reducing, at one time or other, cases occurring in every department of his practice. * * * * * * Hahnemann might have been considered a great modern improver in the art of constitutional treatment, had he not surrounded his do-nothing system with the most extravagant absurdities—had he not left acute inflammatory disease to the risk of homœopathic treatment: in other respects, his system often obtained great success in cases which received no advantages when under the medical treatment of some of our most *eminent and experienced practitioners*. Homœopathy has therefore its merits and demerits: and if the shrewd German had not something in his invention, he would not have received the sanction of one of our most distinguished modern philosophers."—P. 9-12.

These, however, are mere incidental observations, the one appearing in a work containing several good things, and the other prefacing a worthless book of no weight or authority whatever. Dr. Forbes' publication is a laboured production, written with the distant view and object of indicating the necessity and approach of a "Reformation" in the practice of physic—Hahnemann acting the Luther for the occasion. From the position of its author as a medical reviewer and court physician, and his reputation as an exposer of mesmerism, apparently so anxiously sought for, the tone he adopts, the admissions he makes, the suggestions he offers, and the blunders he commits, may do great harm, if not protested against, as not exhibiting the creed or the feelings of the profession in general.

Our limited space forbids our following his arguments and illustrations *seriatim*, and will allow of our only adverting to some of the most important conclusions he arrives at. It results from his examination of the claims of the Homœopathists, as far as the limited and imperfect data published by Fleischmann and Henderson permit it to be made, that he allows about an equal degree of curability of disease may be achieved by the homœopathic as by the allopathic mode of treatment; and as the whole course of his prior reasoning goes to show that what the homœopathists attribute to the operation of their infinitismal doses of medicine, is really brought about by the agency of nature, it follows, that if diseases were left to their natural cures these would be about as numerous as they are at present, under the operation of the ordinary mode of managing them. It will be observed that this important conclusion, and the reformation to be based upon it, depend entirely upon the accuracy of the data furnished by the homœopathists. Now, in the first place, these are admitted by the author as being frequently so imperfect in their particularization, as to render them worthless or dangerous for statistical purposes; and in the next, however illiberal it may seem, we are disposed to place much less faith in the veracity of facts which have been compiled and published for the specific purpose of propagating a new system, than if no such object had been held in view during their accumulation. Observation of human nature shows us that, when a new doctrine has to be promulgated, facts or pretended facts, are but too easy an acquisition; and certainly we are not prepared to believe that the *morale* of those who have deserted their profession of faith, and are anxious to find any excuse for such desertion, is in so satisfactory a condition as to warrant us in supposing that falsification of statements, by the concealment of unsuccessful cases and an unfaithful relation of such as recover, is an impossibility or even improbability. Where experiments have been conducted by the homœopathists themselves in the hospitals of France and Germany, but under the supervision of men accustomed to observe with exactness and record with faithfulness, the results have been found to be entirely adverse to their cause—and that in countries accustomed to the observation of the effects of the treatment of disease by expectation. Experiments of this kind have not, as far as we are aware, yet been made in our own hospitals, and it is to be hoped that such will never be permitted: for we believe the poor creatures who, with confidence or through necessity, are entrusted to our care, are not those upon whom rash statements or blameable curiosity should allow us to put such trials into force. There is an abundance of voluntary dupes who deserve neither commiseration or mercy; and we suppose the practice of most of our readers must have acquainted them with instances in which the folly of such has met with its due reward.

Dr. Forbes thus states the inferences which may be drawn from the consideration of the homœopathists' alleged facts.

"1. That in a large proportion of the cases treated by allopathic physicians, the disease is cured by nature, and not by them. 2. That in a lesser, but still not a small proportion, the disease is cured by nature, in spite of them; in other words, their interference opposing, instead of assisting, the cure. 3. That, consequently, in a considerable proportion of disease, it would fare as well, or better, with patients, in the actual condition of the medical art, as more generally practised,

if all remedies, at least all active remedies, especially drugs, were abandoned: We repeat our readiness to admit these inferences as just, and to abide by the consequences of their adoption. We believe they are true. We grieve sincerely to believe them to be so; but so believing, their rejection is no longer in our power; we must receive them as facts, until they are proved not to be so."

In illustration of Nature's agency in accomplishing what homœopathist and allopathist claim as due to their own special instrumentality, Dr. Forbes enumerates several circumstances, such as the results derived from expectant medicine, from quack medicines, from mesmerism, from influencing the imagination, from diet, hydropathy, &c. With the exception of quack medicines, we admit that all these are citable in proof of many diseases being curable without the aid of *drugs;* but it is exhibiting but a poor degree of knowledge of the actual condition of the practice of medicine among the mass of practitioners of this country, to endeavour to make it be believed that they place the whole, or in many cases, their principal, reliance upon drugs. To them, the value of diet, change of air, the various hygienic appliances, the necessity of, in many cases, abstaining from medicine, and in many others of not relying upon it as their principal means, are as well known as to their critic. The fact is there are more ways than one of curing the same disorder. The patient who has plenty of time, whose confidence is not easily shaken, and whose purse will admit of the delay, may frequently be enabled to exchange the disagreeabilities of drug-taking for the quietude of expectancy, or the pleasures of change of air, scene, mode of life, &c. But for the man whose very *status* for years to come may depend upon whether his illness is one of days or weeks, whose *morale* is depressed and despondent by its prolongation, it becomes a mercy and a benevolence to quicken the actions of his system, and eliminate the cause of its disorder, by a more rapid process than nature usually employs, be this by a drug ever so nauseous. And that this frequently can be done, who does not know? The slow-coach business is well enough for the wealthy, but is destructive to the poor. How the fact of quack medicines having occasionally worked cures can be drawn into a proof of the general inefficiency of drugs, we cannot see. It is well known that many of them consist of anything but inert substances, so that alarming consequences, and even death itself, have not unfrequently resulted from their employment. Taken as they are by such multitudes of persons, it of course must happen that occasionally they effect a cure, and more frequently give relief, or what is mistaken for such. The ground of opposition to their employment is the absence of adaptation to the special exigencies of the case, not their supposed inertness. That would only be an affair of the pocket. When Dr. Forbes, too, cites the effects of the Imagination, Mesmerism, and the like, as proofs of the efficacy of the do-nothing policy, we also feel obliged to dissent from his views. The influencing the imagination has always been, and always will be, a recognized, and sometimes a powerful, influence in the hands of the legitimate practitioner. Who is not aware that one of the most essential steps for the acquisition of dominion over the disease is the securing the confidence of the patient as to his attendant's power of treating it? May not a disease be treated by precisely the same means, but with quite different results, by two different practitioners? What man of sense ever relied upon drugs

alone for the management of a long and tedious disease, to the neglect of the agency of that balm of life, hope? We will even give the homœopathists, mesmerists and *hoc genus omne*, the credit of producing their cures, when they do produce them, through the agency which the means they employ exerts upon the minds of the patients, and may contrast this advantageously with the system of expectation in which Nature is left to fight her battle, deprived of one of her most puissant allies. That such means will thus effect a cure in certain classes of disease we have no intention of denying, although we may be asked, why then do we object to their admission amid our other ordinary therapeutical resources? We reply, that their elevation into a *general system* for the treatment of diseases is what we chiefly object to, as, while we acknowledge that they may be occasionally serviceable in one class of diseases, we are far more certain of their hurtfulness in another; and we cannot consent, for the sake of the advantage which might in some few cases be derived from their employment, but which might often as well be obtained from other measures unassociated with the evils which attach to them, to give that sanction to a system of quackery which would inevitably be afforded even by their partial employment by the profession. It may be said that the means which, in the hands of the present possessors is neither more nor less, from the general and indiscriminate nature of its pretensions, than gross quackery, might become, through its intelligent employment by professional men, at least a valuable adjuvant in appropriate cases; and why, therefore, discourage the partial approvals which are ever and anon appearing in medical works, or at least that class of medical works which are intended as much (or more) for the public as for the profession. We reply that it is derogatory to the profession, and injurious to the public for regularly educated medical men to enter into the same career, however modified, with quacks and impostors. When once a medical man begins to dabble in hydropathy, mesmerism, homœopathy, &c., he runs the risk of becoming confounded with the herd of adventurers who are only eager to take advantage of the popular delusion of the day, and must be content to lose caste in the eyes of his professional brethren accordingly. We do not say that, on the failure of the means which the legitimate practice of his profession places at his disposal, he would not be justified in employing such as accident, or even the proceedings of quacks or impostors, may have acquainted him with. But these are rare cases, and not to be confounded with that morbid desire which seems to seize some professional persons, of patronizing every novelty, however absurd its pretensions, and questionable its promoters. Were things ordered as they should be, no man who had not received a medical education would be allowed to experiment upon the lives, health, and safety of the public, as he may at present, provided only he starts some new and preposterous humbug; and then each proposal for the relief of human ills would be submitted to the ordeal most competent to judge of its merit, and if any good were proved to reside therein, it would be appropriated to the special cases calling for its employment. At present, while an unbridled charlatanism envelopes everything, the legitimate practitioner may flatter himself he rigorously limits his approbation of these various procedures to the cases for which they are adapted; but, in point of fact his approval is heralded abroad, his limitations disregarded, and the

public delusions only confirmed by one who should have done his part in their dissipation. Thus we always have, and always shall, set our faces against professional works addressed to an ignorant public in praise, however qualified, of measures which, whatever good they may contain, are indisputably in the hands of quacks and adventurers of all kinds. Writers are seldom sufficiently alive to the responsiblities they incur by the publication even of what they may consider the truth!

Upon this last point Dr. Forbes more than once emphatically states his opinion. Thus, he says in one place—

"But many of our readers, we expect, will be of opinion that, in admitting what we have done, we are betraying the cause of legitimate medicine, and lending our aid to extending the heresy of homœopathy. If such should be the results of our admissions we cannot help it. We have said only what we believe to be true; and if what we believe is in reality the truth, the promulgation of it cannot lead to evil. Truth is good. If the art of medicine, as we profess and practise it, cannot bear investigation, and shrinks before the light of truth, from whatsoever quarter it may come, it is high time that it should cease to be sanctioned and upheld by philosophers and honest men. If, on the contrary, it be true and good—even if it be only but partially true and moderately good—the stirring touch of enquiry and the stimulus of opposition cannot fail to benefit it in the end."

No one will dispute the abstract proposition that "Truth is good;" but then comes the question, supposing that Dr. Forbes has stated but the truth, whether he has chosen an appropriate time for its promulgation. We have already stated that he has based his whole argument upon an insufficient foundation, and has very much exaggerated the case as against the allopathic mode of treating disease; and we think he is no less unfortunate in the reason he assigns as the immediate cause of publication. It seems that the subject has long occupied his thoughts, but that these have been at last forced from him prematurely by the fact of the conversion of Professor Henderson by the homœopathists. Without attaching so much importance to the secession of a very second-rate man, as Dr. Forbes seems to have done, we look upon it, as far as it goes, in the light of an evil, and as such it ought to have impressed additional cautiousness upon those who had the opportunity of expressing an opinion regarding it, and certainly not have been seized upon as an occasion for bearing testimony in favor of the practical advantages derivable from homœopathy, however erroneous its doctrines might be. Any opinions of this kind, although appearing at first in a professional organ, were certain eventually of reaching a general circulation, and should therefore have been considered with as great carefulness, as if popular and unintelligent perusal were the necessary consequence of their delivery. We look upon the present period as a singularly inappropriate one for exposing the condition of our profession to profane and mocking eyes, and exaggerating its errors, its shortcomings, and its excesses, for two reasons in particular. In the first place, perhaps the public mind was never so much occupied with various pseudo-professional quackeries as at present, and never so disposed to interpret such a confession literally and mischievously; and in the second, at no former period has the medical profession been practised with such enlightened views as at the present, and at none shown itself so disposed to avail

itself of every means of augmenting its sphere of usefulness. The increase in the value of human life is progressive; the spirit of investigation is abroad and diffusing; our means of diagnosis are improved and improving; our treatment is more simple, more judicious, and more successful; the excessive use of drugs is, and has long been, much on the decline; while the temptation to its continuance, by making it the means of remuneration, is discountenanced by a very large portion of the profession—and yet this is the period chosen to proclaim aloud the utter rottenness of our system, its want of superiority over a no-system at all, and to declare that, in necessitating its speedy reform, *the doctrine of Hahnemann will have conferred an inestimable benefit on the healing art!*" Dr. Forbes, it is true, informs us that, with all its faults, the allopathic system of treatment is the best, and that to it he adheres. It is not our business to reconcile or expose these strange inconsistencies on his part, but simply to consider what effect his exaggerated statements must tend to produce upon the profession and upon the public—the interests of both being identical; and we regard this as discouraging and mischievous.

Alarming as his patient's condition may be, Dr. Forbes does not consider it quite desperate, and before taking his leave of him, he furnishes a prescription, consisting of several ingredients, through the instrumentality of which, in the due course of time and nature, relief may yet be hoped for. To our thinking, it is a somewhat expectant affair, and in so serious a case we would fain prefer a more heroic procedure. We are told that we should institute more accurate enquiries into the natural cause and event of diseases, when uninterfered with by art—*upon patients, we hope, who are fully apprised of our intentions:* submit our therapeutical agents to re-examination; endeavour more accurately to ascertain the proportion of curable diseases, and the degree of benefit derivable from medical treatment; and carefully eschew the *post hoc propter hoc* mode of reasoning. For these and other purposes a general adoption of the *Numerical Method* as the means of recording experience is insisted upon. The old maxim, *melius anceps remedium quam nullum*, is to be overturned in favour of Nature's own unmolested operations. Hygiene is pointed out as a fertile field for the medical labourer, and polypharmacy exhibited in colours fitted to make a salutary impression. The Expectant system is elevated at the expense of the Heroic, and active medicines and doses generally discountenanced, especially in exanthemata, fevers, &c., and when given as *placebos*. A greater simplicity in the construction of medicinal formulæ is recommended, and mere routine prescribing held up to animadversion. A more comprehensive and philosophical system of nosology is stated as a desideratum; while professors should show more anxiety to impart a knowlege of the elements of medical science, than to communicate by rote the names of the medicines which have been found efficacious in various diseases. Endeavours should also be made for enlightening the public mind as to the true powers of medicinal substances, and reconcile them to simpler proceedings—inculcating upon them also the necessity of the early commencement of the treatment of their diseases. "Lastly, and above all, to bring up the medical mind to the standard necessary for studying, comprehending, appreciating, and exercising the most complex and difficult of the arts that are based upon a scientific foundation—the art of Practical

Medicine. And this can only be done by elevating, in a tenfold degree, the preliminary and fundamental education of the Medical Practitioner."

We were desirous of presenting these various suggestions (somewhat pedantically styled "Cogitanda—Excogitanda—Agenda") *in extenso*, but by want of space have been constrained to limit ourselves to an analysis of the most important. Among them we find propositions of undoubted, and others of questionable utility, and very few that can lay any claim to novelty: while, looking at them *en masse*, we cannot but consider that their realization would furnish a very inadequate security for that onward progress which we hope and believe practical medicine is destined to achieve. The assimilation of our practice with that of the Expectant system of medicine, here so strongly counselled, may certainly maintain us upon that footing of equality with our homœopathic rivals, which the author allows to be our true position, but we doubt whether it will procure for us a continuance of that superiority which we maintain we really possess. Dr. Forbes observes that, since our increased intercourse with the Continent has taught us a greater reliance upon the powers of Nature, we have become more cautious and more patient in the treatment of disease. It is but fair to notice, however, that the influence here spoken of seems to have been reciprocal, for most certainly disease is now treated in France and Germany more actively, and we may add more efficiently, than it was wont to be; and, among the best practitioners of those countries and of our own, the management of the most important affections is now extremely similar. Indeed, we may observe that while, on the one hand, many of the ablest French practitioners resort to doses of powerful medicines of a magnitude unknown to us; on the other hand, those of them who, like M. Magendie, adhere to the strictly expectant treatment, have little enough to boast of as regards the proportion their tedious and imperfect cures bear to the amount of mortality!

We deny that these suggestions can be regarded as the manifesto of "Young Physic." They are rather the offspring of the exaggerated fears and timid previsions of "Old Physic" in his dotage. "Young Physic," not contemning the part of an accurate observer of Nature's operations, sees also a wide sphere of active usefulness, elements of future progress, and good cause for encouragement. He stands forth with his microscope, test-tube, stethoscope, and speculum, intent upon the investigation of the intimate nature and the signs of disease, well knowing that the first step to its more successful treatment is to be found in its more accurate diagnosis. He is aware that an ardent, an enthusiastic spirit of investigation is abroad, which, in the field of Animal Chemistry, has reaped laurels and will gather more; and if the labourers of our own country are fewer than for its honour could be desired, he looks to remedy this in some provision and encouragement for those who may be willing to devote their lives to pursuits of the last importance to humanity, and yet which can never prove remunerative to themselves. Even with the drawback the absence of such must entail, he points with pride to discoveries in anatomy and physiology which have graced our æra, and which have not been barren in practical results. He acknowledges also the extended acquisitions of the present race of medical men as compared even with their immediate predecessors; but, while appreciating the benefit which has and must ac-

crue to the public from the consequent more enlightened practice these enforce, he is not blind to existing defects, and to the necessity of yet far more extensive improvements. For the attainment of these, and various other advantages, he will seek to urge, by every means in his power, the necessity of an IMPROVED MEDICAL EDUCATION. To this end he will demand that the present system be entirely reformed, both as regards pupil and teacher. Professional education requires prolongation as well as readjustment. The previous years now wasted behind a counter should be employed in storing the mind with facts, disciplining it by science, and enlarging its future sphere of operation by the acquisition of languages. Pursuing his special medical studies, the pupil should be placed under a more active supervision, and submitted to so severe a test by repeated examination, that the trade of the *grinder* might become annihilated. Medical teaching should not be in the hands of those whose length of purse or family influence has raised them to an undeserved pre-eminence; its grave responsibilities being entrusted to those whose talents have been proven by competent judges. It should assume a more practical and less didactical character. Lectures should be essentially demonstrative and clinical; and the student not required to pass hour after hour listening to descriptions of disease which he might more advantageously peruse in his closet, neglecting the while the invaluable opportunities, never again to recur, which surround him, amid the living or the dead, on every side. The crowded condition of the lecture-rooms at some of our medical schools, as contrasted with the deserted state of the hospital-wards, dissecting-rooms, and dead-house, furnish a melancholy subject of contemplation to those who are aware of how little is gained and how much is lost.

These are a few of the objects which may worthily occupy the attention and excite the energies of "Young Physic." If pursued with the earnestness we think there is good prospect of their being, little cause will there be to fear the rivalry of homœopathy, or any other description of quackery, as little to believe in the advent of an impending "Reformation," and still less to doubt of the progressive advancement of legitimate medicine.

Reverting to the two other works upon our list, we have to observe that that by Mr. Leeson is one of abundant pretension and small performance. Professing to exhibit a view of the application of Liebig's Physiology in the Treatment of Disease, he furnishes only indications which were well known, and in appropriate cases acted upon, long before that philosopher was born or thought of. Indeed, although he prefaces the practical portion of his work by an imperfect analysis of Liebig's views, with which we should have thought all the world was by this time well acquainted, it has passed our power to discover even an attempt at shewing how these bear upon the management of disease. We can in fact find nothing whatever new in the treatment here recommended, although this is detailed with great self-laudation. The author, it is true, refers to several cases which, condemned by other medical men either as incurable or as requiring operation, were by the persevering use of tonics and abundant food, &c., recovered; but we suppose most of our readers could also refer to such without thinking that they had worked a radical change in medical practice, or endeavouring to elevate such exceptions into a general rule.

Although, perhaps, we are not justified in detaining the reader with a work which, written for a special object, and that a very transparent one, in no-wise deserves his attention, a specimen or two of the author's opinions may at least amuse, if they fail to instruct, him. Speaking of bloodletting, he says :—

"Thousands of cases might have been saved had not bloodletting and other parts of the antiphlogistic system been employed in the treatment of consumptive cases: on the contrary, had the various outlets through which the system had been allowed to waste been closed, and nutrition carefully introduced, instead of thousands of deaths, we should have thousands of recoveries, and medical science in this department of practice would have been a blessing to mankind, instead of being, as it now is, a horror and a misery. * * * * * While the lungs are undergoing all the changes which constitute the various stages of pulmonary consumption, the stomach frequently remains little impaired; indeed, I might say that I have seen instances where the appetite in many of the advanced stages was almost voracious. It is clear then that, with such advantages in favor of the patient, recovery ought to he looked upon with great hope, if not with certainty, even though the morbid changes in the pulmonary structures had made considerable progress. Almost all the cases which I have had under my care, and where the stethoscope with other means revealed the existence of cavities, and where the integrity of the appetite was yet preserved, got well by putting them under a judicious system of nutrition, combined with such medicines as tend to give vigour to the system and soothe constitutional irritation, and with such pure air as is known to be fit for admission into the respiratory organs."—P. 177-180.

The author's experience has been equally successful in other serious maladies, *e. g.*

Spinal Disease.—"In every case which came before me, I succeeded in turning the threatened mischief by restoring the constitution to a healthy condition, by means of a well-applied system of nutrition and tonic medicine, together with pure air and moderate exercise."—P. 133.

Mesenteric Disease.—"I have had an immense number of such cases with children, and when it was my fortune to see a case in time, I made a correction in the dietetic system; and by attending frequently to the secreting and excreting functions, I was enabled to succeed in every such instance."—P. 116.

Mr. Leeson is a gentleman of susceptible feelings, as witness.

"*Scrofula.*—I do not like the word scrofula, for it literally means a littte sow or pig, and implies a disease to which these animals are subject. Individuals of the highest and most distinguished families in this country have had this disease attributed to them, and often as one of the most delicate, and at the same time one of the most malignant slanders which could be whispered against the interest of honourable and respectable families."—P. 115.

They even occasionally betray him into a *little* exaggeration.

"Here we have two cases of disease of the knee-joint condemned to amputation *by pure surgery*, both of which recovered by constitutional treatment alone. Such operations are far from being uncommon in diseases of the knee-joint, and if we are to judge from the two cases which did so well without, how many amputations must have been cruelly and unnecessarily performed by many of the older surgeons. The punishments under surgery, up to a modern date have been truly awful, and such as our criminal code, in the worst of times, could not have contemplated as punishments for the vilest offences against law and morals."—P. 131.

We will conclude our extracts with a curious specimen of the author's pathology.

"Atrophy, or wasting of the lungs, often arises from the absorption of the permanent air in the cell by surrounding inflammation, upon which the mucous surfaces adhere so closely that no re-admission of air within them can be effected."—P. 178.

It is refreshing to turn from a book of this description to the one written by Dr. Searle; for although, as regards the latter, we find ourselves unable to agree with either the premises or conclusions of its author, we can testify to the philosophic spirit which has presided over their elaboration, and the valuable character of many of the observations adduced for their elucidation. Would that the principles and practice of physic admitted of the simplification here attempted! Alas! they do not! One point (in addition to another adverted to at the commencement of this article) we decidedly object to; and that is, the author's addressing himself "to the public rather than to the profession," under the delusive hope that, by exhibiting the principles upon which the preservation of health and the removal of disease depend, he is giving a death-blow to the impostures of quacks and deceivers. That the endeavour to inform the public on all those points of hygiene which contribute to the prevention of disease, is a most meritorious (although apparently a very unsuccessful) undertaking, we have repeatedly stated; but we must maintain that, when we come to discuss with it the description and management of disease, we discourse of things it never can and never will comprehend, and by appealing to its judgment and criticism upon subjects so beyond its ken, and which the best-educated even of its members have shown themselves the most liable to delusion upon, we are sacrificing our own honour, dignity, and means of usefulness, without conferring upon it any equivalent benefit whatever.

Dr. Searle regards electricity evolved from the blood as the actuating principle of life. By its agency the action of the capillary vessels is excited, and the nutrition of the system thus effected. Life is first manifested in these capillaries, and upon the proper maintenance of their due action, its continuance and the preservation of health depend. Therefore "all disease, or derangements of health consist intrinsically and virtually in the disorder or derangement of this, the primary, organic action." The derangement of these vessels may manifest themselves in the form of *congestion* or passive venous plethora of the part affected—*inflammation* or active arterial excitement, and *irritation*, which is not an affection of the nervous system, as usually believed, but consists in a state intermediate between the active condition of the arteries in inflammation, and the passive state of the veins in congestion—and, when general, constituting fever. One of these three states of the capillaries may be assigned as the essential condition of every disease, wherever this may be situated. All disease depending therefore essentially upon the condition of the vascular system, although it may become "modified in character by the nature, structure, and function of the part in which it is centralized, and combinations founded upon the derangements which successively ensue," its treatment becomes much simplified, and is chiefly accomplished through the agency of bloodletting and calomel, the two most powerful instru-

ments we possess for influencing the condition of the capillaries. But blood is not to be taken away in large quantities without discrimination; for, however proper this may be in the case of inflammatory action involving important organs, in the case of congestion, dependent upon debilitating causes, small quantities, slowly abstracted at frequent intervals, are only indicated. Upon the same principle that we relieve the suffering part by reducing the amount of blood circulating in it, we may occasionally effect the same object by exalting the action of the general system by stimuli. In this way do *mercurials* act, independently of their operation on the secretions—of these calomel is the best. Entering into the current of the circulation, it exerts a specific power upon the capillaries—exciting them to increased action. As a general rule, its use in inflammation is limited to the atonic stage, and that of oppression—it being only guardedly given, and in conjunction with other evacuants, in the more active stages.

"The fruits of my experience justify me in declaring, that if there is any single remedy in the cure of disease, meriting the name of *universal*, that remedy is calomel. The explanation I have given of its operation, and the universality of its influence on the system, in exciting the functions of all the organs, and increasing all the secretions, renders it evident, I conceive, that it fulfils indications of one kind or other in the treatment, with few exceptions, of every disease —which are all, it may be truly said, with very few exceptions indeed, based upon the depression of the active energies of life;—health, as I have before said, consisting in the due action and efficient performance of the various functions of the system. Judiciously employed, (Dr. Searle is an advocate for small doses,) I can say with confidence, in opposition to much prejudice on the subject, founded on the circumstances which first introduced it into practice in this country, and its too commonly improper mode of administration, (the principles by which its employment should be regulated not being understood,) that calomel is as harmless as iron, or any other of the articles of daily remedial administration. This conviction, be it remembered, is the fruit of thirty year's experience, twenty of which were spent in India, where this is the chief remedy employed in the cure of disease, and one of universal use both by native and European practitioners."—P. 91.

Following out these views, Dr. Searle passes successively to their application in the various diseases to which the frame is liable. Many of his observations are acute, and some of his practical remarks useful, but we entirely dissent from his view, that a changed condition of the capillary circulation is the sole primary cause of disease. We believe that essential diseases of the nervous system exist and play an important part in disordering the frame; and that remedies widely different from those he has adverted to, are best fitted for the removal of such.

I. ON THE NERVES OF THE UTERUS. By *Thomas Snow Beck*, Esq. Surgeon. Communicated by *Sir Benjamin C. Brodie*, Bart. F.R.S. Philosophical Transactions, 1846. Part 2.

II. SUPPLEMENT TO A PAPER "ON THE NERVOUS GANGLIA OF THE UTERUS." By *Robert Lee*, M.D. Fellow of the Royal College of Physicians, London, Philosophical Transactions : 1846. Part 2.

MANY of our readers are aware that a controversy has arisen respecting the award of one of the medals of the Royal Society to Mr. Beck, for his investigation of the nerves of the Uterus. Although our principal object in the present article, will be to consider the subject matter of the papers, the titles of which are given above, it would be a dereliction of our duty as journalists, were we to pass by unnoticed, the complaints of injustice which have been brought forward against the Council and Physiological Committee of the Royal Society in reference to this transaction. The facts of the case, so far as an impartial consideration of the involved assertions and contradictions of the numerous parties implicated will enable us to form a judgment, are essentially as follows: a paper "On the Nerves of the Uterus," by Mr. Beck, was presented, but not read, the title alone having been announced, to the Royal Society at their meeting on June 19, 1845. On the same evening a paper from Dr. R. Lee, entitled "Supplement to a Paper on the Nervous Ganglia of the Uterus," was presented and read. Mr. Beck's paper was referred by the Committee of Physiology to Drs. Todd and Sharpey, for the purpose of deciding the question of its being printed. On the 28th of October last, the Committee met to receive the report of the two referees, when, at the commencement of the meeting, Dr. Todd only was present and prepared with his report: Dr. Sharpey subsequently arrived, but unprepared with any report. At this meeting Mr. Lawrence presided, and, after the routine business had been transacted, Dr. Roget, one of the Secretaries, having by mistake stated that the Royal Medal for the year was not to be awarded in the physiological section, Mr. Lawrence vacated the chair and left the room. Subsequently to the departure of the chairman, it was discovered by Dr. Roget that he had committed an error; that a medal was to be awarded for the best paper on physiology during the preceding triennial cycle; and that it was requisite the selection and recommendation should be made on that day. On this, the Committee was re-formed, Dr. Todd being now in the chair; a list of all the physiological papers that had been read during the last three years, fifteen in number, was made by Mr. Bell, the Secretary of the Physiological Committee; and, ultimately, Mr. Beck's paper was recommended to the Council for the award of the Royal Medal.

Such is a very brief, but we believe correct, statement of a proceeding which has excited a large amount of animadversion and comment; and we do not for a moment hesitate in expressing our unbiassed opinion, that there is much ground for rebuke in the whole transaction. In the first place, we entirely agree with our contemporary, the Editor of the Lancet,

that the award was irregular and of questionable legality, inasmuch as it was made in direct contravention of the following regulation, approved by the Queen and affixed yearly to the Philosophical Transactions: "that the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions." The paper of Mr. Beck, which was never read before the Society, has only now been published nine months after the award. In the second place, it appears to us that the Physiological Committee acted, to say the least of it, with impropriety, by adopting any resolution in the absence of a document deemed by themselves requisite to guide their judgment, the written report, namely, of Dr. Sharpey. Further, the proceedings of the Committee were allowed by the responsible officers of the Society to be conducted in a manner so slovenly and hasty, that the members, when assembled to perform the most important of all their functions, were in total ignorance of the business which was to come before them. So long as irregularities like these are permitted, there can be no security against partiality and injustice; and though in the present case we are convinced, knowing the upright and honourable character of those present, that no intentional unfairness influenced the decision, still it cannot be admitted that, in such a hurried scrutiny, fair and even-handed justice swayed the award. Time may show, as we think it will, that although Mr. Beck's paper does not present that amount of original observation or discovery to which alone, as a rule, these Royal Medals should be awarded; yet, that it contains so many important facts, and is calculated to obviate so much uncertainty respecting the pelvic nerves, as to justify a deviation from what ought to be a fixed rule of procedure. But being an exception, and bearing upon a question which is still sub judice, the arbitrators must be content to bear whatever blame attaches to hasty and ill-considered measures, in a matter above all others demanding delicacy and deliberation.

It is well known that Dr. Robert Lee, in two papers published in the Philosophical Transactions for 1841 and 1842, described and depicted the nerves, and especially the ganglia situated on the neck of the uterus, as being remarkably enlarged during pregnancy. The account thus given rested on some most careful dissections of the gravid uterus made by Dr. Lee; and which, as that gentleman states in his appeal to the President and Council of the Royal Society against the award made to Mr. Beck, have been examined by more than fifty distinguished English and foreign anatomists, who have borne testimony in writing to the truth of the published statements.* In the volume of the Philosophical Transactions just published, Dr. Lee has given the results of another dissection, which, he affirms, corroborates his former account.

"In a gravid uterus at the full period I have recently, and with still more care, traced the great sympathetic and spinal nerves into the two hypogastric ganglia, and from thence over both sides of the uterus to the fundus. A lens which magnified six diameters was employed in this dissection, which enabled me with unerring certainty to distinguish and to separate the nervous filaments from the fine cellular membrane by which they are so closely surrounded, and from all the other

* See Letter from Dr. Lee to the President and Members of the Council of the Royal Society of London.—*Lancet*, May 9, 1846, p. 527.

contiguous structures. In this minute dissection, many of the details of the nervous system of the uterus are more perfectly shown than in any previous dissection made by me, and they confirm, in the most complete manner, the accuracy of all that is contained in any previous communications on this subject to the Royal Society. To this preparation, I can now appeal, as affording a perfect demonstration of the truth of all my statements respecting the ganglia and other nervous structures of the uterus."—*Phil. Trans.* 1846, Part 2, p. 211.

In the Abstract of the "Proceedings of the Royal Society," of April 2, 1846, an outline of "Further Researches on the Nervous System of the Uterus" is printed; but the only additional fact here brought forward by Dr. Lee is that Mr. Dalrymple, whose accuracy as an observer is well known, has made a "microscopic examination of the uterine nerves in preparations furnished by the author, which tends to corroborate his views."—*L. c.* p. 610.

It thus appears that Dr. Lee abides by his opinion, that, during pregnancy, the nervous system of the uterus is enlarged, and to such an extent that one of the many ganglionic masses, the utero-cervical ganglion, exceeds in size the semi-lunar ganglia of the great sympathetic. The researches of Mr. Beck, now for the first time published, have led that gentleman to conclusions of an exactly opposite character; for he contends that "the nerves undergo no change of size during pregnancy, nor of position, except such as results from the enlargement of the organ over which they are disturbed.—(*Phil. Trans.* 1846, Part. 2, p. 214.) The dissections supporting this assertion were made on one uterus only, being that of a woman, aged thirty-five years, who had died from hæmorrhage immediately after delivery. The original object of Mr. Beck in making the examination, and the circumstances attending it are thus stated:—"With the view of confirming the researches of Dr. Lee, I commenced the dissection of the nerves of the gravid uterus, but found so many points at variance with his published statements, and the nerves so small, considering the size of the organ, that it appeared very doubtful whether or not they had increased in size during pregnancy; but in order to arrive at a correct conclusion on this point, a comparison of the nerve of the unimpregnated with those of the gravid uterus was indispensable; and in the following year I commenced a dissection of the nerves of the unimpregnated uterus and of the neigbouring organs."—*L. c.* p. 213.

Although the investigation was directed more especially to the uterine nerves, the author was led, in the elaborate dissection which he prosecuted with a rare perseverance for a period of two years, to several other points of great interest relating to the anatomical connexions and structure of the great sympathetic nerve. It is to these last-named parts that we would, in the first instance, direct the attention of our readers.

It has always been deemed a point of physiological importance, to determine the exact nature of the communications existing between the spinal nerves and the great sympathetic; and for this obvious reason, that it is evidently through their medium that all the impressions passing reciprocally between the cerebro-spinal system and the ganglionic system, must traverse. Physiological experiment, equally with pathological observation, demonstrates that such modes of transmission do exist: for instance, in a decapitated kitten, irritation of the spinal cord causes an acceleration of the

heart's action; and again, showing a passage in the opposite direction, irritation of the mucous surface of the intestine, often determines convulsion in the voluntary muscles. Mr. Beck, carrying his investigations further than those of other observers, has proved, by noting the microscopic characters of the fibres, that, in the branches of communication between the sympathetic and spinal nerves, there are contained both gray and white filaments, and he has likewise traced these to their ultimate distribution.

The following is the author's account of this intricate piece of anatomy: "The branches of communication between the spinal nerves and the thoracic ganglia consist of two distinct portions, marked by difference in colour, consistency and distribution. One portion, the smaller of the two, is soft, semitransparent, and of a light brown colour. It leaves the posterior border of the thoracic ganglion, and joins the spinal nerve about one-eighth of an inch anterior to the giving off of the white portion; a gray communicating cord also passes between the thoracic ganglia. This ganglionic mass, when first exposed in the recent dissection, was of a deep red-brown color, which quickly faded to a light brown by the action of the water in which it was examined. The other portion, about one-half larger than the former, is firm, of an opake, pearly-white colour; it passes to the anterior surface of the ganglion, and divides into two portions. One of these portions joins with a similar part, sent down from the ganglion next above, and then goes out in a curving direction on the side of the dorsal vertebræ, to contribute in the formation of the splanchnic nerve. The other half of this branch of the intercostal nerve turns down, constituting a part of the intercommunicating cord between the ganglia, to the ganglion next below, and there joining with a similar portion from the intercostal nerve on a level with this ganglion, passes forwards to join the other branches which form the splanchnic nerve. Each branch sent to the splanchnic nerve contains branches from at least two intercostal nerves, viz. from the nerve on the level with its apparent origin, and from the nerve next above it. This arrangement of the nervous fibres is very apparent in the recent dissection, from the marked distinction which exists in colour and consistency between the gray and white portions of the so-called trunk of the sympathetic."—(*L. c.* p. 214). It is, however, important to state, that the branches forming the splanchnic nerve are not exclusively formed of the white fibres of the spinal nerves, but "are partly composed of gelatinous nervous fibres derived from the thoracic sympathetic." Thus the splanchnic nerve is composed of cerebro-spinal (tubular) fibres and ganglionic (gelatinous) fibres; and "these two kinds of fibres, although associated together, are yet as distinct as the motor and sensitive tubules of the cerebro-spinal nerves, after they are mixed together."

The relations of the gray or gelatinous fibres, passing in the communicating branches, are so interesting that it is desirable to give the author's account of them. These "gray roots of the sympathetic," as they are usually called by anatomists, are given off from the thoracic ganglia; some of these gelatinous fibres "join the intercostal nerve, and, becoming associated with other gelatinous fibres which arise from the ganglion on the sensitive root of the spinal nerve, go with the intercostal nerve in its distribution. The ganglia on the sensitive root of the spinal nerves be-

come thus associated with the ganglia of the sympathetic, whose office, anatomically considered, is to give origin to the gelatinous fibre. A few gelatinous fibres are found in both the motor and sensitive roots of the spinal nerves, but the quantity is so small when compared with the quantity found in other parts, as to preclude the idea that the sympathetic arises from them. Most probably these fibres are distributed to the vessels of the cord; as gelatinous fibres, which come from the gray branch of the thoracic ganglion, pass along the motor root in the direction of the spinal cord, and gelatinous fibres from the same source, also enter the ganglion on the sensitive root; both these sets of fibres evidently pass from the thoracic ganglion towards the spinal cord."—(*L. c.* p. 215.) Mr. Beck sums up this division of his enquiry by observing, "we have thus two distinct and separate systems of nerves, one composed of gelatinous nervous fibres which have their origin at the different ganglia; the other composed of tubular nervous fibres which arise at the brain and spinal cord."

He further has confirmed the opinion generally entertained that the so-called gelatinous fibres are in reality true nervous fibres, by ascertaining the facts that many small nerves are entirely composed of them, and that the ultimate distribution of many of them is to the arteries. The sole anatomical use of the ganglia is conceived to be "to give origin to the gelatinous fibres, to which end they are distributed irregularly over the body, in those situations where a supply of the gelatinous fibre is required. The true sympathetic system appears to be a system of gelatinous nervous fibres, which are distributed everywhere over the body, and which preside over the organic functions. It appears to exist independently of the brain and spinal cord, although the tubular fibres which come from the brain and spinal cord are associated wilh the gelatinous fibres in the larger branches of the sympathetic."—(*L. c.* p. 219.) All these are important details, confirming in many essential points the views arrived at by other modes of research; they also afford another illustration of Bell's one fundamental principle—the individuality of the primitive nervous fibres; and, by showing that the great sympathetic, with all its intricate plexuses obeys the same disposition as that established in the cerebro-spinal axis, prove that, throughout the entire nervous system, there is but one law of conduction.

We now come to that part of these researches which constitutes the main object of the whole enquiry, the anatomy of the nerves of the uterus. The clue to the cause of the discrepancy between these dissections of Mr. Beck, and those of Dr. Lee, is indicated by the following note, appended by the former gentleman to his paper, and dated May, 1846. "In the progress of my dissections, I found that the structure considered by Dr. Lee as 'the hypogastric or utero-cervical ganglion' was a mass of dense fibro-cellular tissue enveloping several small ganglia and a nervous plexus, formed at the junction of the lateral hypogastric plexus with branches from the sacral nerves; that the 'vesical ganglia' were also a mass of fibro-cellular tissue, in about the centre of which were situated small ganglia; and that the remaining structures described as ganglia and plexuses were not nervous structures."—(*L. c.* p. 213.) What these non-nervous structures really consist of, according to Mr. Beck's views, may be understood by one of the references to the plates: "R. R. Portions of

the superficial layer of muscular fibres (of the uterus) which have been dissected out by Dr. Robert Lee, and described by him as nervous ganglia and plexuses. These muscular fibres adhere to the under surface of the peritoneum."—*L. c.* p. 234.

Our limits will not permit us to follow the intricate plexuses of nerves situated in the pelvis, so minutely followed and traced out by Mr. Beck; it would, however, be unjust towards that gentleman, if we did not state that his account differs in some respects from that of former writers, and that he has more successfully determined the source and disposition of those nerves than has hitherto been accomplished. One or two points are all that we can notice. The author has ascertained that there is an important difference in the constitution of the nerves supplying the bladder and vagina, organs endowed with a high degree of sensation, especially the vagina, and also with voluntary muscular power, when contrasted with those proceeding to the uterus, rectum and intestines; in the former case, the nerves contain a much larger amount of tubular (cerebro-spinal) fibres than in the latter. The precise arrangement, extent, and relations of the lateral hypogastric plexuses, springing from the inferior aortic plexus, of the sympathetic, of the branches of the sacral plexus, and of the pelvic plexus formed by the union of the two former, are most satisfactorily demonstrated. Among other points that have been ascertained, it appears that no branches of the sacral nerves are sent to the neck of the uterus, as some have imagined, upon physiological grounds to be the case.

The author thus speaks of the much controverted question, respecting the size of the nerves in the unimpregnated and impregnated uterus:—

"THE SIZE OF THE NERVES.—The size of the nerves in both these dissections (that is of the unimpregnated and impregnated uterus) is essentially the same, and when the nerves are carefully compared, no doubt is left that the nerves of the gravid uterus have undergone no change in size; nor any change in position, except that consequent upon the development of the organ. Yet at first sight the nerves of the unimpregnated uterus appear even larger than those of the gravid uterus; this, however, is due to the manner in which they are arranged. In the unimpregnated state of the uterus, the nerves of the lower portion of the hypogastric and pelvic plexuses, are crowded and packed together so as to occupy a much smaller space than they do in the gravid state of the uterus. The consequence of this is, that the nerves of the former present a wavy arrangement, which gives them the appearance of greater thickness and plumpness."—*L. c.* p. 221.

We may remark that we have observed in the œsophagus of the Boa Constrictor, a part subject to occasional distension, a wavy disposition of the nerves exactly similar to that above described by Mr. Beck, and which would indicate that, in pregnancy, the uterine nerves—we say nothing of the gangliia—are unfolded rather than enlarged.

As to the number of nerves belonging to the uterus, it is, according to the author, remarkably small; a few isolated branches are traced, "which pass on as distinct fine cords, dividing and sub-dividing, but not uniting with each other." The lower part of the organ is supplied by the lateral hypogastric plexus; the middle and upper part has a distinct branch from the inferior aortic plexus; whilst the fundus receives a branch from the nerve, which, coming off from the renal plexus, accompanies the spermatic artery. Mr. Beck also speaks of another set of nerves, minute in size, assuming a plexi-

form arrangement around the vessels, and having the distinctive character of forming here and there minute ganglia.

With respect to the vessels of the uterus, the author is inclined to think that they do not diminish in size after parturition, but are only contracted in their cavity, ready to be again stretched out upon a larger portion of blood being sent to the organ.

In consequence of the great interest that the controversy respecting the merits of Dr. Lee's and Mr. Beck's papers has excited, we have deemed it desirable to furnish our readers with all the details bearing upon the question; and the nature of the enquiry is the best apology for the many anatomical facts contained in the present article. As to the merits of the case, it seems, according to our judgment, that further examinations are required for finally arriving at the truth; and it is especially desirable that the disputed textures—we allude particularly to the large masses which Dr. Lee contends are true ganglia—*should be examined microscopically in a perfectly recent condition;* and this is an examination which that gentleman should have made forthwith. Under such circumstances the existence or non-existence of the nerve-corpuscles, the only certain test of ganglionic substance, would definitely determine the question, and no other kind of proof will be deemed satisfactory by impartial persons. It may not be superfluous to add that, having examined the dissections both of Dr. Lee and Mr. Beck, we are inclined to receive those of the latter gentleman as demonstrating the real structure, although we were among those who formerly admitted the accuracy of Dr. Lee's preparations. Nothing can exceed the beauty of Mr. Beck's specimens; and the plates illustrative of them are equally to be admired.

As we have had occasion, in the first part of this article, to refer in condemnatory language to the proceedings connected with the award of the Royal Medal to Mr. Beck, it is proper to state that our remarks were aimed at what we deem to be the vicious system permitted to exist in a national institution, and were not intended to depreciate the value of these very important researches, which merit, as they will doubtless receive, the highest praise.

THE SANATIVE INFLUENCE OF CLIMATE. By Sir *James Clark*, Bart., M.D. F.R.S. Fourth Edition, royal 12mo. pp. 412. London, 1846.

A WORK of long established reputation like the present seldom calls for more than a passing notice in a quarterly review, unless, indeed, the quantity of additional matter which a new edition contains be such as fairly to demand a more ample examination. That the fourth edition of the "Sanative Influence of Climate" has been carefully revised by its distinguished author, and that its contents have been considerably enlarged, is quite true; but it is not so much for this reason that we are induced to devote a page or two to its consideration, as for the purpose of expressing our earnest caution to young medical men against attaching so great an impor-

tance in the treatment of many maladies to a change of climate from our own shores to distant countries as has been, and we fear still is, too frequently done. That a vast deal of suffering and positive mischief has often been committed by physicians indiscreetly sending patients affected with consumption, to different parts of the continent and other regions, in the vain hope of a southern climate being capable of effecting what no spot in our own country was supposed able to do, cannot be disputed by any one who is at all acquainted with the melancholy history of that disease. Some of the very places, which, a few years ago, were so strongly recommended as most adviseable residences for phthisical individuals, are now admitted to be utterly inappropriate. Nice, for example, once stood very high in estimation; and now what does our experienced author tell us?—"little benefit is to be expected from the climate. . . . Indeed the cases of consumption which ought to be sent to this place are of rare occurrence." And what do we learn from Drs. Renton and Heineken respecting the majority of the poor sufferers that are sent out to Madeira? "Of the thirty-five cases reported by Dr. H., several died before they reached the island, three within a month of their landing, and five or six in about six months. Of forty-seven cases of the same class of invalids in Dr. R.'s report, more than two-thirds died within six months of their arrival in the island." Was it not a cruel and disgraceful thing on the part of medical men to have lent their sanction to, nay, to have suggested the propriety of, a distant change of climate in the greater number of these melancholy cases? Without pursuing this subject further, we have no hesitation in expressing our decided opinion that the favourite practice of sending consumptive patients to Madeira and various places in the south of Europe has, within the last forty years been productive of much greater harm than good. Let it never be forgotten that it is almost solely in the threatened and incipient cases that any decided benefit can be anticipated from a residence in a warm climate. That the young members, more especially the sons, of a consumptive family may often have their health and strength improved by residing for a few years in Madeira or one of the West India Islands, we are ready to admit; but, if the tubercular disease be fairly established, we are only precipitating the mischief by advising such a change.

In the treatment of the different forms of Dyspepsia, Sir James Clark is inclined to lay much more stress upon the particular locality abroad to be recommended than we should deem at all necessary. That change of air, change of scene, change of diet, and change of occupation are better remedies in a host of dyspeptic and hypochondriacal affections than all the pharmaceutical formulæ in the world, who will deny? But surely all these things might be generally had within the limits of our own dear land, if people were but brought to think so.

We fear that Sir James' book has had the effect of making many, especially among the wealthier classes, imagine that they must forthwith go upon the Continent to find relief for certain stomach ailments, which might be just as readily got rid of in their own country, provided some very simple hygienic rules were attended to. The following passage will serve as an example of what we think may sometimes be fairly objected to the present work; viz. in attaching an undue importance to mere peculiarity of climate in the treatment of different morbid affections. After

describing the different forms of dyspepsia, Sir James tells us that each form requires a different climate for its relief:

"The patient with gastritic dyspepsia should not, for example, go to Nice, nor the South-east of France. In such cases, the South-west of France, or Devonshire is preferable, and Rome and Pisa are the best places in Italy. On the other hand, in atonic dyspepsia, in which languor and sluggishness of the system, as well as of the digestive organs prevail, with lowness of spirits and hypochondriasis, Nice is to be preferred to all the other places mentioned; and Naples will generally agree better than Rome or Pisa; while the south-west of France and Devonshire, and all similar climates, will be injurious. In the nervous form of dyspepsia, a climate of a medium character is the best, and the choice should be regulated according as there is a disposition to the gastritic or the atonic form."

Now in truth Dyspepsia, with all its accompanying and induced disorders, is generally much more influenced by the food that is taken into the stomach than by the air that is inhaled into the lungs. Travelling about, too, from one place to another is, on the whole, vastly preferable in all such cases to a stationary residence in any spot, however genial may be its climate. If people *must* go abroad, then it may be perfectly true that "Rome is the best residence in Italy in gastritic dyspepsia, and Nice the best climate in the purer cases of atonic dyspepsia;" but we verily believe that the soft air of Devonshire or the Isle of Wight, and the keener breezes of Wales and Scotland would answer quite as well for the health, and a great deal better for the purse, and often for the morals too.

Within the last five years there has started into notice a new candidate for hygienic distinction as a resort for invalids;—Egypt. Sir James, in the present edition of his work has, for the first time, drawn the attention of his readers to the land of old Nile.

We give the following extract as containing his opinion, formed not from personal examination, be it remembered, but from what has been published by Clot-Bey, Dr. Cumming, and one or two other writers.

"From the description which has been given of the climate, may be inferred the character of the diseases and deranged states of health which will derive benefit from a winter passed in Egypt. Invalids requiring a dry, warm, exhilarating climate will find it here. Certain forms of dyspeptic disorder of long standing, with their consequences, chronic affections of the mucous membranes of the respiratory and other organs of an atonic and congestive kind, and chronic rheumatism may be numbered as among the diseases likely to be benefited; and there is a large class of persons suffering from a state of deranged health which scarcely admits of definition, and yet is well known as one of the many consequences of sedentary habits, prolonged and anxious mental exertions, irregularity of living, &c. who will derive great advantage from a winter spent on the Nile. But the patients, whom I have had chiefly in view in giving this brief notice of the climate of Egypt, are young men whose health has become deranged from the causes already mentioned, or from others of an analogous kind, and to such a degree as to excite apprehension lest the disordered condition of their system might end in consumption. For this class of invalids, suffering from disordered health rather than actual disease, and not so delicate as to be injured by the inconveniences and mode of life to which they would be subjected during a winter in Upper Egypt, the climate seems to me peculiarly well adapted. In the case of invalids also, returning from India for the recovery of their health, and to whom it is important to avoid arriving in England at an unfavourable season, it may be

very advantageous to remain in Egypt till the most favourable period of the year arrives for their return home."

We need scarcely say that there are many regions of the earth where that blessing of blessings, health is much more likely to be found than in Egypt.

If our space had permitted, we should like to have extracted some of Sir James' excellent remarks on the importance of proper ventilation of rooms, in the treatment of many maladies. This is a point that has hitherto been far too little attended to; and it is to be much desired that our author's sound advice will not be lost upon the public.

In conclusion we have again the pleasure of recommending Sir James' work as *the* guide-book to direct medical men in their selection of the proper climate, for those cases where a change of residence may be deemed advisable.

THE MICROSCOPIC ANATOMY OF THE HUMAN BODY, IN HEALTH AND DISEASE. Illustrated with numerous Drawings in colour. By *Arthur Hill Hassall*, Author of the British Freshwater Algæ, &c. London: Samuel Highley, 1846. Parts One and Two, 8vo.

THE author informs us that "this work is to be completed in about twelve monthly parts, each comprising forty-four pages of letter-press and three carefully executed plates in colour;" the price being 2*s.* 6*d.* each part. It is intended to embrace a systematic and copiously illustrated description of the various fluids and solids of the body, no structure or organ being omitted. A work of this nature, successfully completed, would at this particular time confer a real benefit on that large part of the profession, who are either engaged in the prosecution of minute anatomy or are interested in its progress. In the two parts that have already appeared, Mr. Hassall has considered the general characters of the lymph, the chyle, and the blood. As we shall have occasion in our next number to notice the valuable edition of Hewson's works, that has just been issued by the Sydenham Society, and in which the whole subject relating to these fluids has been ably discussed by the editor, Mr. Gulliver; and also the elaborate observations of Mr. Wharton Jones on the Development of the Blood-Corpuscle contained in the Philosophical Transactions for 1846, we can only devote a small portion of our space to that division of the work which is before us.

We are happy to pronounce on the whole a favourable opinion of "the Microscopic Anatomy." It contains a concise but comprehensive account of the subjects on which the author has hitherto treated. The opinions of the most eminent observers, English and Continental, are given with impartiality, and occasional references are made to the writings of the olderm icroscopists, such as Malpighi, Leeuwenhoek, Della Torre, Hewson, and others; the reader is thus put in possession of what is an indispensable

requisite for arriving at the truth, the results, namely, of what has already been ascertained. Mr. Hassall does not, however, confine himself to anatomical details, but enters into such physiological questions as are more immediately connected with the ultimate structures; thus, after describing the blood-corpuscles, their uses are considered, and so with the lymph-globules, &c.

An opinion of the general style of the work may be formed by the author's remarks on the "Uses of the Red Corpuscles," in reference to Respiration.

"Observation has taught us the fact that the colour of the blood changes considerably, according as it is exposed to the influence of oxygen and carbonic acid gases, it becoming bright red under the influence of the former, and dark red, almost black, under that of the latter gas.

"Now the microscope has revealed to us the additional fact that the colouring matter of the blood resides within the red corpuscles; and hence we are led to infer that the changes of colour alluded to are accompanted by alterations in the condition of the colouring matter contained in those corpuscles.

"Further, the alterations of colour which have been mentioned take place not only in blood withdrawn from the system, but also in that which still circulates in the living body, the vital fluid being exposed in the lungs to the influence of the oxygen contained in the atmosphere, and to carbonic acid in the capillary system of vessels.

"But it is not merely a change of colour which the blood undergoes, or rather the coloured blood corpuscles undergo, on exposure to either of the gases particularised, but they also experience at the same time, as might easily be inferred, a positive change of condition, a portion of one or other of the gases to which the blood corpuscles are exposed being imbibed by them.

"That it is really the red corpuscles which absorb the oxygen, or the carbonic acid, as the case may be, admits of demonstration, and is proved by the fact that these gases lose but little volume when placed in contact with the *liquor sanguinis*, or serum of the blood.

"It is clear, then, that the coloured corpuscles are the seat in which these changes occur. Again, from the fact that the blood becomes bright red, or arterial on exposure to oxygen, as in the lungs, and dark red or venous on being submitted to the action of carbonic acid, as in the capillaries, it has been inferred that they are first carriers of oxygen from the lungs to all parts of the system, and second, vehicles for the conveyance of carbon back again to the lungs.

"This inference is correct as far as it goes, but it fails to explain why the imbibition of oxygen or carbonic acid gases should be accompanied by changes in the colour of the blood; and it also fails to show why those gases themselves should be imbibed.—P. 36.

The author, after giving Liebig's theory as to the manner in which oxygen acts upon the protoxide of iron existing in the corpuscles of the venous blood, thus proceeds:

"Venous blood, then, exposed to the air gives out carbonic acid and absorbs oxygen, but arterial blood submitted to the same influence gives out oxygen, and acquires carbonic acid, the seat of these changes being the red corpuscles.

"It will be seen, on reflection, that according to the views just propounded, the surplus amount of oxygen which exists in the peroxide becomes disengaged in the reduction of that oxide to the state of protoxide; during circulation in the capillaries, this surplus is chiefly expended in the elaboration of the different secretions which are continually being formed in the various organs of the body. Such is the *corpuscular theory of respiration*."—P. 37.

The plates representing the various classes of corpuscles found in the blood, are characteristic, though the roughness of lithography is ill adapted for the more minute objects depicted, such as the rouleaux or piles of the red particles of man (Plate 1, fig. 4). The figure of the large discs of the Siren Lacertina, is excellent. The segment to illustrate the small arteries and veins of the frog's foot, with the intervening capillaries, parts it may be remarked en passant, which often puzzle the young microscopist to distinguish, might have been more happily chosen. We must also observe that the account which Mr. Hassall has given of the circulation in the chick is not very clear, the details appearing to us, if we rightly apprehend the description, to correspond rather to the umbilical vesicle and its blood-vessels (omphalo-mesenteric) than to the allantois with the umbilical arteries and veins. Notwithstanding these defects, the work of Mr. Hassall may be regarded as an useful guide to those who wish to become acquainted with the latest views upon microscopic anatomy; and on these grounds we have pleasure in recommending it to the notice of our readers.

CORRESPONDENCE RESPECTING THE QUARANTINE LAWS, &c. Presented to Parliament. Folio, pp. 48. 1846.

WE have alluded in a former page to the negociations that have of late years been carried on in France, as well as in our own country, upon this most important subject. The following are the particulars.

In 1838, a proposal was made by the French to the British government to promote the formation of a congress of delegates from the various European states having ports in the Mediterranean, for the purpose of agreeing upon some uniform system of Quarantine regulations to be adopted by and binding upon all. The British government at once acceded to the proposal. Austria also, which had been applied to by France about the same time, intimated her assent to the general principle and substance, only with some modification in the details. Difficulties, however, were subsequently started by the Austrian government, and the matter dropped entirely until the year 1843; when Lord Aberdeen again took it into consideration, and invited the French and Austrian governments to join with him in carrying out the proposal made by the former in 1838. France expressed her concurrence; but Austria considered that the establishment of any conference or congress, as proposed, would be premature until exact information was procured from competent medical men upon the following three points:

1. The minimum and maximum of the terms of quarantine to be fixed for persons.
2. The terms of quarantine necessary for goods and merchandise.
3. The best measures to be adopted for the disinfection of objects that are susceptible of contagion.

Prince Metternich intimated, at the same time, that a period of six months

would probably be required to obtain this preparatory information, before the proposed congress of delegates could proceed satisfactorily to the determination of the various questions to be submitted to them.

We have already seen what steps the French government took to procure the most accurate instructions on the several points to which the prince referred.

The British government also, anxious that all just or alleged causes of delay might be removed, dispatched about the same time, viz: in October 1844, Sir William Pym, the Superintendent of Quarantine in this country, to visit all the ports in the Mediterranean where quarantine establishments and lazarettos existed, and to draw up a report upon them.

After describing the various places which he visited, and briefly mentioning the quarantine regulations that exist in each, Sir William makes the general remark that "the periods of Quarantine both as to the persons and merchandise may be very considerably reduced, particularly with reference to vessels arriving from places with clean bills of health, and in some instances altogether abolished, such as upon vessels from the Black Sea and those crossing the Atlantic; and that many of the unnecessary, vexatious, and expensive regulations, more particularly in the Italian States, may be discontinued."*

As a matter of course, Sir William takes it for granted that the plague is contagious (communicable by contact, we presume), and that the contagion may be transmitted not only by the sick themselves but by various *fomites;* for we read that "it will be necessary to decide upon a list of articles of merchandise that are supposed to require purification, under the impression that they have been contaminated by persons having the plague, and the period of time required for their purification, together with the best means of doing so. To effect this, it may be necessary to examine practical men from different lazaret establishments, the superintendents of quarantine, captains of lazarets, and the medical men attached to these establishments." Some of these parties here named are not, we fear, the most likely to come to very sober decisons upon the points under consideration; they are contagionists *par metier.*

The date of Sir Wm's letter, from which these extracts are taken, is June 1845. His next letter, dated September of the same year, is occupied with answers to the three questions or points of enquiry which Prince Metternich considered to require solution, before any step could be wisely taken to establish a congress. To the first on the list—as to the minimum and maximum of the terms of quarantine necessary for persons—he gives the following reply:

"It appears to me that this question, and the one of greatest importance in this enquiry (the incubation of plague in the human system), was decided by the almost unanimous opinion of eighteen medical men in the Levant, by their replies to queries as published in the Parliamentary Papers; all of them, however much they differed in opinion upon the subject of contagion, agreed upon the short

* The health authorities of Palermo acknowledged to Sir William that they had run bars of iron through the fire, and had washed sugar-hogsheads with lime, for the purpose of destroying contagion! Very lately, a vessel from England was put under quarantine at Messina, because a report had appeared in the newspapers of a fever having prevailed at Glasgow!!

period of incubation, viz., from three to ten days, with one exception, Dr. Floquin, of Smyrna, who put down fifteen days as the maximum; and the opinion of those medical men is confirmed by the inclosed valuable document. No. 1, being a return of 5240 individuals who had undergone the *spoglio* in the lazaret of Alexandria in the course of four years, out of which number forty-three were attacked with plague, and all of them before the eighth day after the operation of *spoglio*."

The answer to the *second* question contains an admission that is so truly important, in a commercial as well as in a medical point of view, that we request our reader's special attention to it:

"It is difficult to obtain any decided evidence upon this question, as during my Quarantine mission *I could not ascertain that any one case of plague had been produced in any one of the various lazarets that I visited, in consequence of the manipulation of merchandise* (the Italics are ours.—*Rev.*); and as in the principal lazarets in the Mediterranean (Marseilles and Malta) they have gradually abolished the *serenos* (probationary airings,) and reduced the period of depuration of goods with foul bills to twenty-one days, it does not appear that this period could be further reduced with such cargoes as cotton, if exposure to the atmosphere is to be considered necessary; as it will take the full time, according to the present practice of opening first one side of the bale for a certain period, then making up that side and opening the other for the same time, making in fact the quarantine upon cotton only ten days, which last period will be sufficient for small cargoes or parcels of merchandise, which can be at once opened and exposed to the air."

On the *third* point—as to the best means of disinfecting objects susceptible of contagion—Sir W. very naively remarks; "from what I have stated as to the non-appearance of plague in any one instance from the manipulation of merchandise in Lazarets, the present practice of exposing goods to the atmosphere for a certain period, appears to have been attended with success." He seems never to have even so much as dreamed of the possibility that the goods possessed no contagious property whatsoever; and yet, strange to say, he was aware, all the while, that not a single instance could be produced of the plague having been ever communicated by the manipulation of merchandise, in the lazarets which he visited! Such are the blinding effects of old deep-rooted prepossessions on the mental vision even of the most experienced observers.

The following brief notice of the vessels which have arrived at Malta with the plague on board, and have been duly depurated in the lazaretto there, since the island has been in possession of Great Britain (not including the plague of 1813), will be found to contain some interesting particulars, serving to illustrate and confirm several of the most important positions in the French Report. The details will not be deemed tedious or unnecessary by any who wish to understand thoroughly the important subject under consideration.

1819.—A Maltese vessel, laden with oil and soap, arrived on the 27th of March from Susa, from which she had sailed on the 20th with a foul bill, in consequence of the plague prevailing there; from 15 to 18 persons were dying daily. The day before sailing, one of the crew had been taken with symptoms of fever. On the 21st, vomiting with delirium set in, and next day he died. The master stated that there were no external marks of plague during life, but that several petechiæ were observed on the belly and thighs after death. Four of the remaining five persons of the

crew were attacked with the plague, while in the lazaretto; one on the 3rd, and three on the 4th of April. Of these four cases, two proved fatal. *The health guard, and two persons* (all from pratique) *who nursed the sick, were not attacked.*

1821.—A Maltese brig, laden with beans, arrived on the 21st of March from Alexandria, with a foul bill, having 14 of a crew and 8 passengers on board; two of the passengers and one of the crew had died during the voyage. On the 27th February, the day before sailing from Alexandria, ten passengers were received on board. Of these, eight were in good health; but two, a woman and her daughter, were sickly, having been suffering from diarrhœa. Both died, the mother on the 2d March, and the daughter on the 16th; no marks of plague were observed, it was said, on examination of the bodies. Besides these cases, one of the crew, who had suffered much from sea-sickness and had taken no food, became feverish and delirious on the 17th; then profuse dirarrhœa came on, and he died about midnight. Several petechiæ were observed upon the body. While in the lazaret, no fewer than ten of the crew and four of the passengers were taken ill; of these 14 cases, 12 proved fatal.

On the 28th March, a health-guard and four sailors were embarked from pratique on board the infected vessel, to depurate it and land the cargo. One of the sailors was attacked on the 2d of April and recovered; he had had the plague in Malta in 1813. Another sailor was attacked; he also recovered.

No mention is made of any of the official inmates of the lazaretto or attendants upon the sick having suffered.

1828.—On the 13th of June 1828, the Russian frigate "Castor," with 281 of a crew, arrived at Malta from Armiro, from which she had sailed on the 21st of May. She had previously left Malta in pratique on the 30th of April for Navarino and Modone. On the 3d of May, she was in company with three other Russian ships-of-war; they had captured a Turkish corvette, which had sailed the day before from Modone for Alexandria, having on board 600 individuals, including the crew, invalids, sick, and wounded from Ibrahim Pacha's army. (It is not to be wondered at that some malignant fever prevailed amid such an assemblage.) The captured corvette was manned by 15 sailors from the Castor, and men from the other Russian ships; at the same time, 200 out of the 600 of the corvette's company were taken on board the Castor, and, on the 11th of May, were all landed on the coast of Morea: the Castor receiving back the 15 of her crew from the corvette. On the 17th, she arrived at Armiro; and on that day a sailor was seized with headache, vomiting, and delirium. Next day, another was taken ill with similar symptoms. On the 19th the former patient died, the body exhibiting externally nothing indicating plague. On the 20th, the latter was carried off; and still no marks of plague were visible on the body. On the same day, a third sailor was similarly attacked; he died on the 24th, but without having exhibited any of the outward signs of plague. On the 2d of June, while at sea, a fourth sailor was taken ill; but, besides the general symptoms observed in the three preceding cases, this patient had a swelling under the right arm; he died on the 9th. As the surgeon now declared the disease to be a contagious pestilential fever, the frigate was immediately ordered to Malta,

there to undergo quarantine and depuration. *No new cases occurred either on board or at the lazaret during the Castor's stay.*

1835.—A Russian brig, with a cargo of bales of cotton and linen and other susceptible goods, arrived from Alexandria on the 2nd May with a foul bill bound for Leghorn, and having 13 persons (originally 15) on board. On the 11th of April, whilst at Alexandria, one of the sailors fell overboard; on the same day he was taken ill with a pain in the chest. He, however, got better, and continued so until the day after leaving Alexandria (this was on the 18th), when he became worse, and on the following day he died: no marks of plague were observed on the body. On the 27th, whilst off Candia, another sailor was taken ill with pain in the chest, extreme debility and spitting of blood; he continued daily getting worse until the 28th, when he died. Several blue spots were observed on the body. On the 1st of May, being near Girgenti, a third sailor was seized with violent headache and general debility; these symptoms continued until the following day, when he died. On the same day (2nd), a fourth sickened in a similar manner to the others. The rest of the crew remained in health. During the stay in the lazaretto five other cases occurred, and four of them proved fatal. In these four cases there were petechiæ and carbuncles: in the fifth there were not any. *Again, there is no mention of any of the attendants in the lazaretto having been affected.*

1837.—An Ottoman vessel arrived on the 22d February from Tripoli (which she had left on the 15th) with a foul bill of health, having a crew of 6 persons and 52 passengers on board. One of the latter had been taken ill on the day of leaving Tripoli, and continued so all the voyage: he had been kept apart in a small boat on deck. When taken to the lazaretto, two bubos were visible in the groins: the man recovered. *This is all that is stated respecting this vessel: the disease, therefore, did not spread to any one.*

1837.—An Ottoman vessel, "laden with susceptible goods and beans," which had sailed from Tripoli with 11 of a crew and 22 passengers on the 10th of February, arrived at Malta on the 23rd. One of the passengers fell overboard during the passage, and was drowned. Two of the passengers and one of the crew sickened in the lazeretto—one on the 27th, another on the 28th, and the third on the 8th of March; they all died on the second day after the attack. In one case there was a bubo, in another there were petechiæ.

Another vessel arrived about the same time from Tripoli with a foul bill, and two persons sick. On being conveyed to the lazaretto, symptoms of plague appeared in both. One died. "*Two of the crew, who attended and nursed their fellow-sailor in free communication with him, continued in perfect health.*

1840.—H. M. steamer "Acheron" from Alexandria arrived at Malta 27th April 1840, with a foul bill of health in seven days, with eighteen passengers and forty-eight persons in crew, having brought the mails, several parcels, letters. and two horses, all well on board. On the 29th, early in morning, the health-guard, who was put on board on the day of her arrival, reported to the captain of the lazaretto that one of the crew, a boy, during the night, at about 9 P. M., died, and that one of the stewards was seriously ill. The corpse and the sick steward having been landed in

the lazaretto and examined by the physician, evident symptoms of plague were observed upon them. "*The persons who attended and nursed the deceased, continued well.*"

1841.—On the 9th of March, H. M. frigate "Castor" arrived in 15 days from Kaiffa, having had 13 cases (two were doubtful) of plague on board, of which 9 had proved fatal. Four of the men had been taken ill on shore at Kaiffa, before the frigate sailed. On the 23d of February, four other men were sent from shore to be put on the sick list. In one or two of the cases the symptoms were those of gastric fever; but in others the fever was truly malignant, and was accompanied with glandular swellings in the groins and axillæ. In one case the cervical, and in the other the submaxillary, glands were affected: in the latter instance, the patient was to all appearance recovering from the original disease, but was carried off by the diseased state of the tongue and fauces the viscid secretions therefrom having produced sudden suffocation. In one case, the symptoms resembled those of *delirium tremens*, and were treated as such.

It is unnecessary to enter into farther particulars; suffice it to say that there is not a vestige of proof that the disease was communicated, in a single instance, from the sick either to the healthy on board or to any one in the lazaretto.

1841.—An Ottoman brig, in ballast, arrived from Alexandria on the 16th of May, having 15 persons in crew, and 180 passengers (haggis). She had sailed from Alexandria on the 8th. One of the passengers had died during the voyage; and three others had sickened, when in sight of the island. While in the lazaretto, 13 cases of plague took place, and 10 of them proved fatal. Of these, one occurred in a Maltese boatman, who was put in quarantine, from pratique, with one of the patients on the 28th. No other particular is mentioned.

A month afterwards, an Austrian brig, in ballast, arrived from Alexandria, which she had left with a foul bill of health, and having a crew of nine persons, and 105 passengers: she was bound for Tangiers. Eight of the passengers died on the voyage. While in the lazaretto, other three cases took place. Two of these proved fatal: the third, which occurred in a health-guard who had put into quarantine with the sick, did well.

The last instance of imported plague in the lazaretto of Malta was that of an Ottoman brig, which arrived in July of the same year, in 37 days from Alexandria, with a foul bill of health, 87 persons on board, and laden with linen, flax, beans, &c. No casualty occurred during the voyage. While in quarantine, four deaths took place; one from dysentery, one from inflammation of the brain, one from enteritis, and one from (what is called) pestilential bubo.

Now these various facts, drawn from the experience of the quarantine authorities at Malta during the last 25 years, appear to us to afford convincing testimony as to the very little risk of the transmissibility of the plague, away from an epidemic focus of the disease. Here we have an account—unexceptionably authentic, be it remembered—of between 40 and 50 cases of recognised plague having occurred in the lazaretto of Malta, without the disease having been communicated in a single instance to any of the *employés* in that establishment. The only case which occurred among those in pratique were four; and what, pray, were the

circumstances in which these persons were attacked? Two were of a party which had been put on board an infected vessel, to clean her out; and the remaining two were men who, from pratique, were put in quarantine and confined along with the sick—most probably in a small room or chamber.

The critical reader will have remarked how promptly and easily the disease was arrested, and how small the number and mortality of cases were on board the two ships of war, compared with the merchant vessels, crowded as these latter often were with poor filthy passengers, who had come direct from a focus of infection. Most of these vessels moreover, it will be observed, had foul bills of health upon their arrival.

So much for the results of infected vessels which have arrived at Malta within the last 25 years. Let us now see what has occurred at Marseilles during the same period. Our information upon this point is derived from the French Report, which has been already analysed at so great length; but we have deemed it better to introduce the details here, for more convenient comparison with those just given.

1819.—A Swedish vessel arrived at Marseilles on the 1st of May from Susa, which she had left on the 15th of April, and from Tunis, which she left on the 20th: both places were at these dates afflicted with epidemic plague. She had a crew of 12 persons; and, besides them, 17 passengers had been received on board at Tunis. On the 25th, one of the sailors was attacked with plague; he died. On the 26th, another of the sailors fell sick; but he recovered after a very tedious convalescence. Besides these deaths on board, a woman and her infant died; the latter of dentition, the former of what was supposed to be milk-fever. The rest of the crew and passengers remained healthy.

On the 12 of May, one of the health-guards, who had been put on board the vessel, was taken ill: he died on the 16th.

Not a word is said of any other person, either in the vessel or in the lazaretto suffering.

1825.—A vessel, laden with cotton, hides, and wool, arrived on the 30th June from Alexandria, which she had left on the 29th of May. On the 5th of June, one man was taken ill and died on the 9th with symptoms of plague. A boy fell sick on the 16th, and died on the 19th, on which day the captain also was attacked: he died on the sixth day afterwards. Another man was taken ill within a few hours of the arrival of the brig at Marseilles; he was without delay conveyed to the infirmary of the lazaretto. The constitutional symptoms were by no means severe; two bubos had formed in the left groin, but neither of them suppurated. This man recovered. There was another case, in which the symptoms were comparatively very mild.

No mention is made of any of the attendants in the lazaretto being infected.

1837.—The "Leonidas," a post-office steamer, having on board 47 of crew and 18 passengers, arrived at Marseilles on the 9th of July, having left Constantinople on the 27th of June, Smyrna on the 30th, and Syra on the 1st of July. Passengers in quarantine had been embarked and disembarked both at Syra and at Leghorn; and at Malta, too, several passengers had been taken on board, but all in quarantine. Upon her arrival at Marseilles, one of the stokers was affected with low fever; he

died next day. On examining the body on the 11th, it is stated that the brain and intestines were found to exhibit marks of violent inflammation; but that there was no appearance of buboes, carbuncles, or petechiæ. On the same day, another man, who was in the habit of sleeping in the same bed with the last patient, was visited by the lazaretto physician. He had been suffering for two days with severe headache, and within the last 24 hours, a painful swelling had made its appearance on the upper part of the left thigh. The case was regarded as very suspicious. The tumour became considerably larger, and the patient was in a state of great prostration with occasional delirium. On the 15th, petechiæ were observed on various parts of the body. On the 16th, a carbuncle appeared over the left ankle; the bubo had shrunk considerably: diarrhœa, vomiting, and, delirium were the chief constitutional symptoms. The patient died next day. No *post-mortem* examination was made.

On the 20th, the cook of the Leonidas was taken suddenly ill in the lazaretto, with intense headache, vomiting, and great prostration. Besides these symptoms, there was a painful swelling at the lower part of the right groin. Next morning he was bled from the arm. On the 22nd, the headache was very severe, there was frequent vomiting of a greenish matter, the tongue was foul, and the patient was wandering in his ideas, and occasionally delirious. On the 23d, the symptoms were more unfavourable; profuse diarrhœa had set in; the patient was in a state of collapse. Death took place on the 24th. When examined on the following day, the body was spotted with bluish patches in different places; the tumour in the groin had sunk down very much.

No mention is made of any of the lazaretto attendants having suffered; not even Dr. Chevillon, who had most heroically waited upon the two last patients with the greatest assiduity.

We shall make no comments on the numerous facts which have now been adduced;—facts too, be it remembered, drawn from the official records of lazarettos, the very places where we may be sure that the most would have been made of every event which seemed to favour in any measure the doctrine of infection, and the consequent necessity of strict Quarantines.

The amount of *materiel*, which we have submitted to the reader's attention in the present number of this Journal, will enable him—provided always he not only reads, but marks, learns, and inwardly digests its details—to form tolerably accurate conclusions on the important questions which its consideration involves. This is the more necessary at the present time as the subject of quarantine enactments will be brought before Parliament in the course of next Session; and it therefore surely behoves all medical men to be acquainted with whatever has been made known by those who have had competent opportunities of observation. The present Government has pledged itself to the working out of several much-needed social reforms. We sincerely trust that the re-organization of our quarantine establishments may be one which meets with its earliest and most searching investigation.

Periscope;

OR,

CIRCUMSPECTIVE REVIEW.

Selections from the Foreign Periodicals.

NEW RESEARCHES ON THE COMPOSITION OF THE BLOOD IN HEALTH AND DISEASE. By MM. A. BECQUEREL and A. RODIER, Docteurs en Medicine.

THIS paper is the recapitulation of the researches made by these observers during the last year, in the view of confirming, completing, or extending the consequences to which their former labours gave rise. They have availed themselves of every opportunity which offered itself for analysing the blood in the conditions of the system to be hereafter specified; but never has a venesection been performed with the object of affording them such. The details they enter into as to the various precautions to be taken in conducting the analyses, and the tables exhibiting the results of these, are given at too great length to admit of transcription. We must content ourselves with stating some of the general results, referring our readers to the original for more minute particulars.

1. The various descriptions of albuminous matters contained in the blood are endowed with a powerful affinity for water. It results, that when we wish to dry them, these matters yield with great difficulty the last portions of fluid; and, even when they are deprived of these, they instantly commence reabsorbing water from the atmosphere, which is just as difficult of expulsion. The presence of this water, unless the most minute precautions be taken, may interfere in a very material manner with the results of our calculations. Numerous experiments have shown the authors that to desiccate about 100 grammes of blood or serum continuous exposure for 48 hours to a temp. of about 178° F. is required, and that the powder must be weighed while hot, the instant it is removed from the apparatus, for in a few minutes only water will have been absorbed. The vague expression "albuminous matters" has been designedly adopted, for it is not easy to decide whether it is the albumen or the other materials of the blood which manifest such an avidity for water. Some experiments, however, lead to the opinion that this property especially belongs to the alkaline salts, to free soda, and the extractive matters soluble in water.

2. Blood which has been taken and is exposed to the air is submitted to an incessant evaporation, proportionate to the extent of its surface, the temperature, and the humidity of the air. This concentrates the solid parts, and gives rise to very variable results, only to be avoided by keeping the blood in a vessel hermetically sealed. In the hot days of summer, blood taken in large receptacles, sometimes in this way becomes reduced to at least a third of its volume. It should be received into vessels which are deeper than they are broad.

3. The quantity of serum of the blood determined with proper precautions, is in general proportionate to the quantity of solid matter this fluid holds in solution. The equilibrium may, however, be destroyed. Thus the density is greater

when there is little albumen, properly so called, and much extractive matters and free salts. It is less, on the other hand, when there is excess of albumen, and, which is rarer, excess of fatty matters and little extractive matters and free salts.

4. The serum of the blood, whatever its composition, being mixed in different individuals with variable proportions of globules, it follows that, in the complete analyses of the blood, the figures representing the solid matters of the serum have not an absolute value, and we can only compare the relation of the water to these same figures. To obtain an idea of the composition of the serum in health and disease, we must consider it apart, and analyse it separately, after spontaneous coagulation has separated the globules and the fibrine. This important precept was laid down more than 20 years ago by MM. Dumas and Prevost, although other experimenters seem since to have lost sight of it entirely. In all their analyses they have always represented the analysis of the entire blood in one table, and the composition of the serum in another; and it is only in this way we can ascertain the exact modifications of the albumen and other portions in solution.

5. When a bleeding to some extent is practised, and that not too rapidly, the blood has not the same identical composition during its whole progress. The latter portions are more watery and less rich in solid matters. The impoverishment is continuous, although a certain quantity must be lost before it is perceived. The diminution of solid parts takes place especially at the expense of the globules, and, to a slight degree, at that of the solid matters of the serum. The proportions of the serum continue much the same. In what manner is this impoverishment to be explained? In the present state of science we believe the explanation of MM. Prevost and Dumas to be the best. "When a small animal," they say, "is bled to a notable extent, the veins absorb with rapidity, at the expense of the remainder of the system, a proportion of liquid perhaps equivalent to that which has been lost, whence it happens that the mass of particles seems diminished in a given quantity of blood." The impoverishment is therefore only apparent, and it is therefore not a matter of indifference at what period of the bleeding we take the blood, the analysis of which is to represent the condition of the system at the time the abstraction is made. It is the first portion that should be chosen.

6. Anterior bleedings exert a sensible effect upon the composition of the serum. It becomes more watery, less dense, and less rich in solid matters. The quantity abstracted and the number of bleedings will much influence this impoverishment, as will also abstinence and the progress of the disease. It is manifested especially in respect to the albumen properly so called, while the amount of extractive and fatty matters and free salts vary little. Pure albumen is the element of the serum which seems to separate with the greatest difficulty. Thus, when a person who has been bled several times becomes convalescent and eats, and the solid parts of the serum are consequently more and more increased, if a new bleeding is practised, as for a complication, the albumen is found to have increased less than any other element. In estimating the effects of prior bleedings we must, however, confine ourselves to short periods, as from 24 to 48 hours, and to the same disease. If a longer period elapses, or the bleeding is employed for a complication, the serum may have repaired its losses, or have become modified by the new affection.

7. We may offer the following statement of the physiological condition of the blood. 1000 of *serum* contain as a mean 90 solid parts. Of these 90 the *albumen* may be represented by 80. The *extractive* matters and free *salts* by 8, and the *fatty* matters by 2. The limits of the physiological condition are 86 and 95: or, much more frequently, 88 and 92. The mean *density* of the serum is 1027, the physiological limits being 1028 and 1026. The highest figures are found in strong persons, in good health, and living well. The influence of age, sex, and temperament is uncertain. In a former essay the authors had given the following as the weights of the principal elements in 1000 parts of *blood*.

Water, 780; globules, 141; fibrine, 22; solid matters of serum, 80. In women the weight of the globules was stated at but 127, the proportion of water being so much the greater. Subsequent researches have confirmed these, every possible care having been taken to select from those persons who applied voluntarily for and insisted upon being bled, only such as gave every sign of health. "Such cases are of rare occurrence, and exact the most rigorous examination, and we have eliminated from them all such as manifested signs of plethora."

8. *Condition of the Serum in various Diseases.* (A.) *Plethora.*—In our former observations we stated that the composition of the blood in plethora did not differ sensibly from that of health, and that the examination of the symptoms rather led us to admit, with some restrictions, the old opinion, that in this condition there is an augmentation of the mass of the blood. Our later researches have entirely had reference to the composition of the *serum*, and have been conducted upon six men and one woman. These prove its composition to be identical with that of health. Its mean density was 1028. The mean amount of solid parts was 91·7, being a little above the physiological mean: but on looking over the various analyses, they are found to be within the physiological limits, although occupying a high position in the scale of these. It is pure albumen, which furnishes the preponderance, the other solids being as in health.

(B.) *Simple Fever.*—In four of seven individuals examined the serum did not deviate from the normal state; but in four others its density (1026) was diminished, and the mean of the solid portions was as low as 85—such diminution especially manifesting itself as regards the albumen. This, however, the authors believe rather arises from the abstinence the patients were obliged to observe, than from the effects of the disease.

(C.) *Typhoid Fever.*—Analyses have been made of the blood from 25 bleedings performed on 17 persons. In 17 *first bleedings* the density of the serum was low (1026), and it sank in proportion to the gravity of the disease, until it reached 1023. In 7 out of 16 cases the serum was too abundant, in 7 in small quantity, and in 2 normal. In 7 instances it was limpid. The mean of solid matter was only 85·5 to 914·5 water; and the part defective was albumen. In two cases it was normal in amount, and in 1 slightly increased. In 6 *second bleedings* the density was lowered from 1026 to 1024, and the proportion of solid matters to 81. Two patients were bled a *third* time, the diminution still continuing.

(D.) *Phlegmasiæ.*—Modern investigation has shown that, whenever the phlegmasia is sufficiently characterised to induce fever, the proportion of *fibrine* is augmented, and is increased, remains stationary, or is diminished, according to the progress of the inflammation.

The present analyses only relate to the serum. Upon 38 patients suffering from well-marked inflammations, venesection was practised 51 times—24 being men, and 14 women. The *first bleedings* produced a diminished density of the blood, especially in severe cases. The density varied from 1030 to 1023. Its mean 1027·5, being of less value in some affections on account of its oscillations. The serum was usually abundant and clear. The mean of the solid matter was 88·4; *i. e.* at the lower limits of the physiologyical scale; but the amounts varied very considerably. After the *second* bleeding the density was diminished to 1026, and the analyses only gave 81·7 of solid parts. All these effects are increased by repeating the bleeding. In slight inflammations, in those of short duration, and in patients who have taken nourishment up to nearly the time of the attacks, the serum may be scarcely at all different to that of the normal state. In severe cases, those which have severely tried the patient's powers, and have required abstinence, the conditions above named are most obvious. Still there are exceptions, but such patients are usually able to take food.

(E.) *Pulmonary Tubercles.*—Considered in relation to the alterations which

take place in the blood, patients affected with phthisis present different characteristics according to the degree of the development of the organic product. In the earliest stages in some patients the constitution seems not to have in anywise suffered, while in others, an anæmic condition precedes, or is coeval with the tubercles. In the second period, when the tubercles soften, or they are surrounded by inflamed textures, fever is conjoined to the anæmic condition. In the third stage, when hectic, diarrhœa, &c., are present, anæmia if it has not before appeared, manifests itself. The modifications of the blood, that under these circumstances occur, may be reduced to two principal ones, which may manifest themselves in different degrees and combinations. These are the diminution of *globules* when anæmia is present, and the increase of *fibrine* when phlegmasia complicates the original disease. The condition of the *serum* has been studied by the authors in 16 patients who had undergone 24 bleedings, prescribed for the relief of hæmoptysis, violent febrile action, or some intercurrent phlegmasia. Although, owing to the variability of some of the results, no absolute rule can be laid down, yet, as a general remark, it may be stated that, when any complication existed, anæmia become developed or abundant, hæmoptysis had occurred, the solid portions of the serum diminished in quantity, the water increasing, and the density becoming less—the albumen being the portion of the solid matters in which the diminution occurs. The change of the blood in phthisis bears most analogy to that which occurs in phlegmasiæ.

(F.) *Diseases of Spinal Marrow.*—When these are accompanied by paraplegia there is usually a diminution of the quantity of *globules*, and that in proportion to the weakness of the patient and the duration of the disease. In some cases the proportion of globules was less than that observed in some examples of chlorosis, without however any *bruit de souffle* of the carotids being heard. Sometimes the amount of *fibrine* is normal, and at others, if there be an intercurrent inflammation, it may beome increased, which occasionally too it does without the existence of any such cause. The *serum* is usually very dense, containing almost always a large proportion of solid matters, as albumen, extractive, &c.

(G.) *Bright's Disease.*—Six patients only have offered the means of observation in this affection, and the conclusions drawn from them confirm those published by MM. Andral and Gavarret. These are the small amount of globules: the fibrine normal unless a phlegmasia has supervened: the great diminution of the albumen of the serum (the solid portion of serum varying from 73 to 64,) and that in proportion as the disease is chronic in its progress and the bleeding has been repeated. Of course the density of the serum is diminished.

(H.) *Pregnancy and the Puerperal State.*—The former researches of the authors were confirmatory of the account of the blood of *pregnant women* furnished by MM. Andral and Gavarret, viz. that the proportion of *globules* is diminished, while that of the *fibrine* is slightly increased. They added another new characteristic, viz. the diminution of the albumen of the *serum*, and the consequently lesser quantity of this fluid. Additional researches confirm these conclusions. The mean density of the serum examined in 13 females was only 1025·8. The mean of its solid portions was but 85. As to the *puerperal states*, M. Andral suggests, in his "Hæmatologie," that the production of fibrine towards the termination of pregnancy is in some manner connected with the phlegmasiæ which so often arises in the puerperal state. The authors in their former essay had referred to two cases, one of eclampsia, followed by puerperal fever, and the other of puerperal fever, in both of which a considerable diminution of the density and of the solid parts of the serum was observed, and they asked whether a diminution of albumen, equal or greater to that in Bright's disease, might not be an essential feature of *puerperal fever*. Since then a M. Hersent has had abundant opportunities of pursuing the investigation, and he has published the result. He states—"1. That the appreciable changes of the blood in puerperal fever

consist in a large increase of water; an extreme diminution of the amount of globules, and an equal deficiency of albumen, 2. That the extent of these changes and the gravity of the disease are proportionate. 3. Probably the vitiation of the blood pre-existed before the explosion of the disease; but that it cannot be considered its cause, although its existence adds very much to the rarity of the case." The authors now refer to several additional analyses they have made in puerperal fevers of different degrees of severity, and conclude with the following observation. "In proportion then as the pregnancy reaches its termination, independently of the diminution of globules which frequently occurs, and the increase of fibrine which is more rare, the albumen of the serum becomes very notably diminished in amount. May we not enquire whether such diminution, which is often excessive, will not explain certain *dropsies* (independently of those induced by the obstruction of the venous circulation by the developed uterus) which appear towards the end of pregnancy; and if the diminution which occurs in eclampsia and puerperal fever may not exert some influence on the production of these diseases?"

(1.) *Chlorosis and Anæmia.*—The condition of the blood has been studied in nine cases of *chlorosis*, in all of which the serum was found superabundant and limpid. Its density was within the normal limits, as was also the proportion of its solid parts, varying from 85 to 89, and furnishing a mean of 87.9. In this disease the changes in the blood had, therefore, reference to the small proportion of globules. In 26 analyses of blood in *symptomatic anæmia* the *globules* were less than 100. The serum was clear, abundant, and of a density of 1026, its solid parts only amounting to 87·7, the loss being in albumen.

The authors thus sum up the results of their examination of the serum in the above-mentioned diseases.

"The density of the serum, and the proportion of the solid parts which it contains, remain within the physiological limits under the following circumstances: viz. plethora, slight or chronic diseases (causing little disturbance of the general system and in which nourishment is still taken), chlorosis, the commencement of pregnancy, and the onset of some acute diseases, &c. In all these cases the proportion, however, approaches the lower limits of the physiological condition.

"The solid portions of the serum, and especially the soluble albumen, are subjected to a very sensible *diminution* under the influence of certain conditions, which however do not at all act in the same manner or with the same intensity. Thus the impoverishment is inconsiderable under the influence of fasting, anterior depletion, and slight phlegmasiæ. It is more marked in severe disease, especially when this is prolonged, in severe phlegmasiæ, typhoid fever, sympathetic anæmia, the termination of chronic disease, the end of pregnancy, &c. Lastly, it is extremely great in Bright's disease, puerperal fever, and certain diseases of the heart producing dropsy.

"The *increase* of the proportion of solid matters of the serum is a very rare event. It is found in too isolated cases to admit of any general rule being laid down. It is, however, nearly constantly observed in diseases of the spinal-marrow."—*Gazette Medicale*, Nos. 26, 27, 32 and 36.

THE CÆSARIAN OPERATION CONSIDERED IN A THEOLOGICAL POINT OF VIEW.

Some months since a woman died in Brittany, being about six months advanced in pregnancy. The parish priest sent for a medical man to remove the child by the Cæsarian section, in order that it might be baptised, if still alive. Upon his refusing to do so, the priest sent for a *farrier*, who removed a dead fœtus by the Cæsarian section. These facts being severely commented upon in some of

the newspapers, the minister published a reply in justification of his proceedings, stating that they were imperative upon him, in proof of which he quoted Bishop Bouvier's *Sacred Embryology*, which is a text-book of the church. This expressly orders the section to be made as speedily as possible, under all circumstances as regards the age, time, and mode of death, &c., and in respect to the person who is to be called upon, it says—"No endeavour must be neglected to procure the services of a professional man. If this is impossible, a midwife, some other woman, a married man, or in case of urgency any one at hand must be resorted to, but *never a priest*, unless there is absolutely no other person who can be procured." When the medical man refused, the priest read these and other passages of M. Bouvier's work to the farrier, shewing him the absolute necessity of proceeding with the operation.

A medical man, before resorting to the operation has to satisfy himself—1, That the *woman is really dead*; 2, that the *child is alive*, and 3, that it is *viable*. This restricts obviously the operation within very narrow limits. The Church regards not the *viability* of the child, but the chance of its being alive, and therefore fitted for baptism. But the chances of error and danger that this rule gives rise to are obvious. Is the practitioner to submit to the dictates of the priest and open the woman's body the instant she has yielded her last breath. Many there are who would hesitate to do so, and if the professional man delays in order to be certain that his patient is really dead, does it seem reasonable to allow persons utterly destitute of physiological knowledge to take upon themselves not only to judge of the propriety of the operation, but even its execution? This has been so strongly felt by some of the clergy themselves even, that various contrivances have been thought of to dispense with the necessity of the operation, and yet confer the rite of baptism upon the infant. Thus, *vagino-uterine injections* have been advised, and, quite recently, these have given rise to a learned discussion in the Academy of Medicine of Belgium. The validity of such a procedure has been admitted by high authorities of the Church, so that, even according to its own showing, the necessity for the operation may be brought within very narrow limits. When, however, it is determined to practice the operation this should be solely confided to professional men; for an apparent state of death during gestation is of too frequent occurrence, and of too difficult detection, to allow of the interference of unskilful persons. When medical men are regularly consulted in these cases, they should bear in mind the duties and obligations of their profession, and lend their assistance, when the rights of humanity, of which they are the natural guardians, do not oppose, to the accomplishment of the duties of religion, in the same manner as they are accustomed to do with regard to those of justice.—*Gazette Medicale*, No. 30.

[We cannot trust ourselves to remark upon this example of priestly interference; but we ask, is the period of its occurrence the nineteenth century, and the country enlightened France?—*Rev.*]

ON THE TREATMENT OF PHOTOPHOBIA. By DR. DUVAL.

In those ophthalmias which are attended by an exaltation of the vital properties of the organ, we first combine antiphlogistics with the remedies which are calculated to reduce preternatural sensibility. When the inflammatory action has subsided, we continue this latter class of *antineuralgic* remedies alone, varying them from time to time, as by the persistence in the use of the same one, its action becomes enfeebled. Another important indication in their use is always to proportion their dose to the severity of the affection. A too energetic antispasmodic, it is true, throws the part for a time into a state of torpor, but reaction comes, and we only induce a more severe exacerbation.

Opium.—This is one of the most useful remedies in affections of the eye when photophobia is present, the pain coming on in the paroxysmal form. From 3 to 6 grains should be dissolved in water and rubbed into the supra-orbitar region three or four times in the twenty-four hours. Mackenzie especially recommends a combination of calomel and opium, given internally, in cases of photophobia arising from rheumatic and catarrho-rheumatic ophthalmia.

Belladonna.—An infusion of the dried leaves, given internally, is a far more certain and efficacious remedy than the extract, &c., used externally. Ten grains of the infused leaves repeated every three or four hours will often produce great and speedy ease: but the increase of this quantity must only be very carefully decided upon, as it will, if carried too far, produce symptoms of poisoning. The earliest symptoms indicating the necessity of suspending it are a desire to spit, a dryness of the mouth, inspissation of the saliva, and a burning sensation in the throat. After this, the brain becomes confused, giving rise to hallucinations and afterwards to cerebral torpor. Some practitioners give a mixture of *extract of belladonna* (one or two grains per diem) and *sulph. quinine*, either by the mouth or in a lavement. M. Rognetta speaks highly of its combination with *calomel* for the photophobia of scrofulous ophthalmia. *Ext. Bellad.* gr. ½. *Calomel*, gr. xv. Divide into 12 pills, and take one three times for the first day, and two for each dose next day, and so on. In this way the belladona has been sometimes increased to 4 gr. per diem with impunity, although the cause for its use usually disappears after the first or second day. When using the extract belladonna *externally* M. Duval mixes it with a double quantity of *mercurial ointment*, adding, if the pain is very obstinate or acute, from three to ten grains of opium. If the ointment is to be continued, the proportion of belladonna must be augmented. About a scruple should be rubbed into the temples and forehead four times a day. He prefers the watery to the alcoholic extract, the latter being liable to fermentation, during which its anti-neuralgic properties are dissipated; and indeed, united with alcohol, the belladonna seems to acquire even a stimulant property, at all events for a while after its application. The extract of the green leaves is more active than that made from the dry ones: and that procured by displacement is to be preferred.

Hyoscyamus is usually considered succedaneous to belladonna, but its utility is doubtful except in very slight cases, and its dose should be double that of the latter. *Stramonium* is more energetic, and must be given in half this quantity. "To both we prefer Belladonna, which we employ, whatever be the extent and nature of the ocular inflammation, providing photophobia be present."

Camphor is of great service in essential photophobia, the consequence of very prolonged ophthalmias (opthalgia), or coming on after a prolonged sojourn in a dark place. It should be dissolved in alcohol, or mixed with mercurial ointment, and applied over and near the region of the eyebrow and temple. Inspiration of camphor is also highly useful.

Excision of the Conjunctiva.—When the above measures fail in giving relief, and especially when the membrane is vascular or villous, we have recourse to this operation. The relief by this little operation is so great, that we may term it an antiphotobic measure, par excellence, and recommend it, whatever part of the eye may be affected, or whatever the nature or degree of the ophthalmia. It causes little pain, and is easy of execution. We employ Pelier's elevator, Maunoir's forceps, and scissors having a very slight curvature. Seizing a fold of the conjunctiva near the conjunction of the cornea and sclerotica, we make a circular incision of this membrane. This little operation, usually reserved for chemosis only, has always succeeded in our hands in those cases of obstinate photophobia dependent upon chronic oph-

thalmia—but it may be resorted to with equal advantage when the photophobia is excessive in acute ophthalmia.

Puncture of the Cornea.—M. D. has performed this operation seven times, with most remarkable relief to the patient's sufferings. They were all examples of various forms of ophthalmia, and all tormented with the intensest photophobia. After treatment appropriate to their cases had been adopted, they all resumed their usual occupations. He believes this operation has been too much neglected, nor has he found the inconveniences in its performance indicated by Gregory.—*Annales d'Oculistique*, Tom. XI. p. 101–111.

CAUSE OF DEATH IN THE AGED.

According to M. Rostan, the most frequent cause beyond all others is the obstruction of the circulation induced by arterial or valvular ossifications. He has had abundant opportunities for pursuing the investigation at the Salpètriere, and has found that ossification of the sigmoid valves of the aorta, and frequently of the auriculo-ventricular valves, as also of the arterial walls themselves, existed in almost all the subjects of advanced age, whose physiological condition it may in some sense be said to be. Hence the obstruction of the circulation, the consecutive congestion of the viscera, and the necessary death. Boerhave stated that senile deaths occurred through affections of the brain, and Bicat, that they might arise from interruption of the circulation, the respiration, or of the action of the brain. M. Rostan, however, believes the natural, senile, so to speak, physiological, death, always occurs in consequence of the changes in the heart and arteries mentioned. Some authors believe ossification to be the result of inflammation, but that this is not always the case, may be seen from the fact of its frequent occurrence in those aged persons.—*Gazette des Hôpitaux.*

VELPEAU ON FRACTURE OF THE OLECRANON AND PATELLA.

"Some of the observations we find delivered by our classical authors on these accidents are true only in exceptive instances. Thus, they tell us, inevitably a great separation, amounting to an inch or more, takes place, while in fact the interspace does not usually exceed a few lines. The prognosis of these cases is not serious; for even those we meet with which have not been treated at all, do very well. In the case of the olecranon, there is only an impossibility of making complete extension, and of the patella some weakness of the limb—the fibrous matter which is deposited being in fact almost as solid as bone itself. If we had no other means to recommend than that of two months' immoveability of the limb, we had better not attempt treatment at all, for more will be suffered from the consequent stiffness and œdema than from the injury itself. The limb should, however, be placed in moderate extension, the figure of 8 bandage applied, and over it the dextrine bandage, for three weeks or a month.

"To show that I do not exaggerate the harmlessness of these accidents, I may observe, that very considerable movements of the part are retained, even when the separation of the fragments is considerable. Thus, I have seen a Belgian diplomatist who was able to walk about although the patella was drawn up to almost the middle of the thigh. A ship-captain, the fragments of whose patella were separated by four or five fingers' breadth, scarcely limped. Nor are the consequences more serious in fracture of the olecranon."—*Gazette des Hôpitaux.*

ON THE LAW OF THE FORMATION OF PRIMARY ABSCESSES EXTERNAL TO THE BONE (AS DISTINGUISHED FROM CONSECUTIVE SUB-PERIOSTEAL ABSCESSES, DEPENDENT UPON SUPPURATIVE INFLAMMATION OF THE MEDULLARY CANAL) AFTER COMPOUND FRACTURES (BY CONTRECOUP) AND DISLOCATIONS OF THE LONG BONES. By Dr. LAUGIER, Surgeon to the Beaujon Hospital.

Attention has been sufficiently directed to the injury which the muscles, capsule, blood-vessels, &c., undergo in these accidents, while the effects of violence upon the soft parts immediately in contact with the bone, but *opposite* to where its displacement has taken place, have been overlooked; and yet I do not hesitate to say that in the production of the primary or consecutive, and frequently fatal, mischief these lesions play a most important part, although they may exist for some time undetected. The truth of this opinion is proved by the surprising constancy with which abscesses consequent on these injuries are found so situated. The reason of it is that the laceration and separation of the soft parts from the bone are far greater upon the opposite side to that on which the displacement occurs, although the fragment of the bone may penetrate the muscles and skin on this last. If the reduction be not complete, a space between the bone and soft parts persist and becomes the centre of tedious suppuration, the discharge, which had it occurred on the side of the wound would have easily escaped, now burrowing along the course of the bone or bones, and inducing a yet greater separation of the soft parts before any external manifestation of the existence of the abscess is afforded. It is remarkable to what an extent published cases of compound fractures are defective in details concerning the seat of abscess sufficient to test the accuracy of this statement —the observer usually contenting himself with stating the fact of abscesses having existed without indicating their locality. Where such statements have been furnished they do not however impugn its correctness. The same law applies to compound dislocations; for it is at the side opposite to that on which the head of the bone has been displaced that the principal seat of inflammation and suppuration is found.

Several years since I demonstrated that, as regards dislocations of the first metatarsal bone, when abscess follows, it always forms on the outer side of the reduced metatarsal, and that, to prevent it, it is necessary to make a deep incision parallel to the external side of this bone. I soon discovered that this was but an example of a general law equally applicable to other fractures and luxations of the long bones: and the knowledge of such a law has enabled me to preserve many limbs which would otherwise have been amputated, or in which the consecutive lesion would have proved mortal. Guided by it I have made a *preventive incision* at the appropriate spot, or directed the various local applications, as leeches and fomentations, and at a later period, suitable pressure, to be made there.

Some persons have seemed to think that the above views were already comprehended in the rules laid down for making preventive counter-openings at the base of a flap in certain wounds of the head. But my remark does not relate so much to the propriety of making preventive incisions as to the indication of their proper locality; and if the analogy above alluded to really exists, at all events it has never before been perceived; and in fact no author has indicated hitherto the seat of election of deep-seated abscesses after compound fractures and dislocations of the long bones. The principal, and indeed the only, point I have in view to prove, is, that these *primary abscesses are situated on the side opposite to the displacement*. As to consecutive abscesses occupying another seat, they are only found when the recognition of the primary one has been neglected. The pus burrowing around the bone spreads out in whatever direction the aponeuroses allow it a free course. Such an abscess observed at a more or less considerable period after the accident, would there-

fore in no-wise invalidate the law now stated, and so much the less so, inasmuch as these secondary abscesses communicate with the primary one, whence they have derived their existence.

But do *simple* fractures by *contre-coup* and dislocations offer nothing analogous? They do; only from the much less separation and injury of the soft parts in these accidents, the occurrence of abscess is much less common: but when it does happen, like the chief injury to the soft parts (which indeed in simple dislocation is often very considerable, giving rise to relapse, paralysis, &c.) whence it arises, it is also situated at the external part of the limb.

Dr. Laugier briefly relates the particulars of a few cases occurring in his own practice exemplifying the law he is endeavouring to establish; and quotes two or three from the remarkably small number that have been recorded in the periodicals in sufficient detail.—*Archives Generales*, tom. xi. p. 133–145.

[Although we think "preventive incisions" must not be too hastily resorted to for averting evils which may perhaps never occur; yet, if the above-stated circumstance proves upon further examination to be indeed a law, it will induce more careful examination of the suspected locality than is now usually made.—*Rev.*]

MEMOIR ON THE PARALYSIS OF THE THIRD PAIR OF NERVES CONSECUTIVE TO NEURALGIA OF THE FIFTH PAIR. By M. MARCHAL (DE CALVI).

My object is to signalize a relationship which exists between paralysis of the third pair with neuralgia of the fifth that has not been hitherto suspected. Trifacial neuralgia has been little studied as regards the disorders which it produces beyond the nerve it affects, but which form a very interesting and curious part of its history. Is it not in fact remarkable that a lesion, limited to a few filaments of the fifth, can, by a retrograde repetition of morbid actions, propagate itself to the nervous centres and induce the most extensive, multiplied, and serious accidents, such as the loss of speech or power of deglutition, excessive dyspnœa, paraplegia, violent convulsions, emprosthotonos, furious delirium? Nevertheless this is to be found detailed in a case by Ponteau, which I have published with several others in a paper upon *Traumatic Prosopalgia* in the 55th Vol. of the *Memoirs of Military Medicine*. And in these so certainly was it the simple lesion of some of the trifacial filaments that induced so fearful an assemblage of symptoms, that when they were divided, by a section extending to the bone, the accidents, which had so long resisted all medical appliances, disappeared in half an hour never to return. Two phenomena or two orders of phenomena are sometimes so disproportioned that the idea of their connexion never at first presents itself to the mind; for who could have thought such grave disturbances of sensibility and motion were dependent upon an old contusion of a few nervous filaments. Several facts and a careful examination of all their circumstances were required before this connexion could be perceived. These cases of prosopalgia, with *general* lesion of sensibility and motility, led me to recognize the *special* relation which exists between the paralysis of the common oculo-motor nerve and neuralgia of the trifacial, in the following cases.

CASE 1.—A soldier æt. 47, of a very nervous temperament, was the subject of paroxysmal pains of dreadful violence on the left side of the head and face especially in the vicinity of the supra-orbital foramen, mastoid process, and in the teeth of the upper jaw. The left eye became affected also with diplopia, but presented no deviation from its normal direction. The sensibility of the left

cheek was entirely gone, as also of the nostril, although he could still perceive odours. He could open his jaws only to a very slight extent. M. Marchal tried the experiment of compressing the frontal nerve as it passed out of its foramen. This caused great pain, *but immediately, and as long as it was continued, the diplopia ceased.* The experiment was frequently repeated with the same results. The pressure, however, could not be employed as a remedial means in consequence of the great pain it gave rise to: but the patient obtained considerable ease during his paroxysms from inducing compression of the dental nerves by introducing a small piece of wood between two of his teeth. Seven blisters were successively applied over the supra-orbital region in the space of 20 days, purgatives and stimulating pediluvia being simultaneously resorted to. The pain was relieved and the sensibility restored; but the diplopia remained and the globe of the eye became smaller, and drawn inwards, the upper eyelid being also paralysed, so that the eye was kept shut. But now analogous pains and diplopia were observed on the right side—so that this latter could no longer, as heretofore, be obviated by closing one eye. Blisters were applied on this side and the pain relieved: but the diplopia of either eye continued, and the patient's vision became sensibly enfeebled. Time and the use of Meglin's pills, or probably the first alone, gradually restored his vision; and, one evening, after drinking to excess, the diplopia also suddenly left him. The patient, however, eventually became the subject of various other nervous affections, which entirely destroyed his health.

CASE 2.—A young woman, æt. 26, and otherwise in perfect health, had suffered for two years most violent pains in the left side of the head, radiating towards the ear, eye, and cheek. They were accompanied by tinnitus auris and red flashes before the eye. Eight days before visiting her the eyelid could not be raised, and the globe of the eye was simultaneously drawn outwards. The pupil was dilated. A sharp pain was felt opposite the supra-orbitar foramen, and increased when she laid on that side. Following her occupation as a shoe-binder, she had many years since been accustomed to press the left side of her head against an article of furniture. This gave rise to a tumour here which suppurated, and the resulting sore was obstinate in healing. On touching the cicatrix which this left a sudden and violent frontal pain was felt. Blisters were applied over the cicatrix, and galvanism employed in the course of the third pair, but all without success.

CASE 3.—M. Marshall having accidentally observed a pensioned soldier with a blephoroptosis, enquired its history. He learned that the man, after having been exposed to damp, had suffered horrible paroxysmal pains at the root of the nose and near the supra-orbital foramen. After a certain time these ceased, and were followed by the complete descent of the eyelid, the globe of the eye being also drawn outwards and the pupil dilated.

CASE 4.—Louise Heberard, æt. 33, had enjoyed good health until she worked as a dress-maker in a cold, damp, apartment. In June, 1844, she was seized with tooth-ache on the left side, and then with pains along the left eyebrow, and eventually opposite the supra-orbitar foramen. Severe pains were also felt at the root of the nose, and near the angle of the jaw. The left eye became drawn inwards and she saw double. In May, 1845, the upper eyelid fell, and the eye which had been drawn inwards now became drawn outwards. Tactile sensibility of the left side of the face and head was abolished. The sense of smell was gone on the left side, as also that of taste at the anterior part of the tongue. During mastication, the patient often bit the left side of her tongue, and she articulated so imperfectly as to be understood with difficulty. She was much troubled with confusion of the head, and could not guide herself unless the left eye was closed, on account of her double and

confused vision. No means that were tried gave her more than partial relief and she left the hospital.

CASE 5.—A man in M. Gendrin's ward, while employed on a railway, had received a blow upon the forehead, which induced violent pains radiating towards the surrounding parts. Upon his admission long after the accident, pressure upon this point still caused some pain; and, several months after the existence of these neuralgic pains, the upper eyelid of the same side fell, and the eye was drawn outwardly.

"In these cases it cannot be doubted that the neuralgia of the 5th pair, preceded the paralysis of the 3rd. As in the third case, the neuralgia may have ceased for a longer or shorter space of time, and then the paralysis may seem to be dependent of it, until due enquiry is made. I am certain that a great number of cases of paralysis consecutive to neuralgia may, in this way, be detected."

M. Marchal believes the following hypothesis offers the most probable explanation of the occurrence. The trifacial nerve and the common motor oculi meet in the ophthalmic ganglion, the former furnishing it the sensitive root by the nasal branch, the latter the motory root from its inferior branch. "It will be admitted that a reflex morbid action may take place within this ganglion by which the affection which is expressed in the sensitive nerve by pain or anæsthesia, is transmitted to the motor nerve, in which it is expressed by convulsion or paralysis. I say convulsion: for, in the first case, the eye was drawn inwards as it also was at first in the fourth. The symptomatology of the motor, as of the sensitive nerves, is of two opposite kinds; pain and anæsthesia for the latter, convulsion and paralysis for the former; and in this way, prior to the paralysis of the rectus internus, it may have been in a state of excitement, during which the eye would be drawn inwards."

This hypothesis is consistent also with the most plausible theory of the functions of the nervous ganglions—true miniature brains, as they have been called, for the regulation of special actions—receiving impressions by filaments continued from the sensitive roots, and conveying these by the motory filaments—presiding over the nutritive phenomena by their grey fibres, and only advertising the brain proper of what is occurring in their localities, under extraordinary circumstances. In this way, the ophthalmic ganglion, in particular, would be affected in the relations prevailing between the retina and the iris, and certain muscles of the eye. Advertised of the vicissitudes of sensibility of the retina by its connection with the optic nerve, it reacts upon the iris, harmonizing the pupil according to the degree of sensibility of the retina and acts reflectively by its motory root upon the muscles of the eye which are influenced by the third pair.

There is then, besides the perception belonging to the brain, another, viz. a *ganglionic* or *organic perception*.—*Archives Generales*, Tom. xi. pp. 261–273.

We think it may prove advantageous to place in juxta-position with the foregoing interesting paper an extract of the following

CASE OF AMAUROSIS OF THE RIGHT EYE, FROM A SLIGHT WOUND OF THE CORRESPONDING EYE-BROW. Related by Drs. MICHELACCI and FEDI.

The Reporters observe that it is still a matter of controversy whether a simple traumatic lesion of a branch of the fifth pair can induce amaurosis. Muller seems disposed to attribute its production to commotion of the retina or the optic nerve, although there certainly exists examples of amaurosis following severe lesions of the forehead, without any such concussion having taken place. Mal-

gaigne, too, trusting to a false maxim that the lesion of a nerve may paralyse its terminal branches, but cannot operate in a reverse manner towards the trunk, is likewise intent upon proving the ease with which the peculiarity of the structure of the orbit allows of the production of commotion of the optic nerve. Lawrence doubts whether amaurosis ever results from injury of the frontal nerve.

The example we here adduce is not explicable, at all events, upon the above supposition. The patient became amaurotic immediately after receiving a small wound from a shot over the right eyebrow. Three questions were proposed by the legal authorities for the consideration of the Reporters. 1. Whether the blindness really existed? 2. Can it be referred to the infliction of this small wound? 3. What hope is there of a cure? For a reply to the first of these the state of the patient's eyes was diligently examined; and they were found to be quite natural in appearance, as also in the action of their pupils as long as both eyes were kept open; but when the left eye was closed the right pupil was found to be quite unobedient to any stimulus whatever. "The results of the experiments we tried proved, in our opinion, the perfect blindness of the right eye, depending upon a complete paralysis of the sensorial nerve. The movements of the iris of the blind eye, which took place whenever the light was allowed to exercise its influence upon both eyes together, or only on the left one, do not at all depend upon the sensitiveness of the right retina, but are exerted solely by virtue of the nervous action excited by light in the left eye, and by its sensorial nerve reflected through the medium of the brain upon the motor nerves of the right iris. These results agree with those I have observed in other cases of amaurosis confined to a single eye, and find their explanation in the doctrine of the reflex nervous action, as taught by those celebrated men, Marshall Hall and Müller.

A very small cicatrix was observed over the orbital ridge, just at the point where the frontal nerve emerges from its foramen; and the blindness having immediately supervened upon the infliction of the wound which produced this, the second question was answered in the affirmative.

The prognosis was unfavourable; for seeing the rapidity with which the blindness was induced, the completely amaurotic condition of the visual apparatus, and the long period which had elapsed (34 days) without any improvement having resulted, and recollecting Scarpa's opinion upon the rarity of cure in these cases, it is to be feared the loss of sight will prove permanent.—*Annali Universali*, Vol. 116, pp. 21–32.

[Agreeing with the Reporters that cases enough are on record to allow of the admission of the production of amaurosis by injury to the frontal nerve without concomitant *concussion of the retina*, we cannot allow that their own case, one of gun-shot wound of the forehead, although a slight one, can be considered as an unexceptionable example of this.—*Rev.*]

OBSERVATIONS UPON ELECTRICAL OR ACUTE CHOREA. By DR. DUBINI.

During the last ten years Dr. Dubini has observed about thirty cases of this dangerous form of chorea, and his colleagues have met with a great number also. He seems to have imposed the name "electrical" upon it in consequence of the resemblance which the muscular movements bear to those produced by the electrical shock. One finger, one extremity, or one side of the face (generally the right) only is first affected, but in the course of a few days the whole of the *same* side of the body becomes implicated. The movements are almost constant, and, besides those of a more moderate character, there occur, two, three, or more times in the twenty-four hours, violent convulsive paroxysms, affecting precisely the

same muscles, accompanied by a rapid pulse, and followed by profuse sweating. After these have subsided, the parts affected by them fall into a more or less complete state of temporary paralysis.

Fear seems in the majority of cases to have induced the disease, although, in some, the patients who are usually young (from 7 to 21)and robust are unable to trace it to any such cause. In some instances, pain in the head or in the course of the spine have preceded the attack. The convulsive action always attacks during the course of the disease precisely the same identical muscles, producing oftentimes great distortion of the eye, mouth, head, &c. Occasionally certain fingers, or one hand only may be affected without the disease extending to the rest of the body for 30 or 40 days, and in one case the only symptom consisted in an affection of the muscles of the tongue for nine days; but the slightness of the attack, and the otherwise apparent health of the patient, must not put us off our guard, as the case is just as dangerous as the more severe forms of the disease. During the convulsive paroxysms the patient articulates with great difficulty, and if he has not arrived at the stage of stupor, he is usually employed in steadying the one arm, which is agitated just as if by electricity, with the other. The limbs are usually tumid but not œdematous; and the surface is morbidly sensitive, so that the least touch of the arm will sometimes excite the most violent clonic spasms of the whole corresponding side. As the case gets worse the convulsions become more unintermitting and sometimes implicate the other half of the body. They, however, get feebler and at last cease. The patient now becomes comatose; sweats pour from the body, the eyes sink in the head, the respiration is stertorous, the pupils are fixed, and the cornea becomes softened and sometimes ulcerated from the pressure at the angle of the eye. The pulse, which during the existence of the convulsions had been strong and vibrating, is now feeble or imperceptible. This condition may precede death for a day or two. The intellectual faculties are preserved, as indicated by the movements of the patient's head, when the convulsions render speech impossible, until the apoplectic stage of the disease; and even in young children a remarkable presentiment of the fatal issue of their disease is often observed. The appetite is at first good, but becomes gradually less. The bowels get confined, and almost all the patients void *lumbrici*. Those of them, who do not pass the worms, however, have not the disease milder than the rest—nor does the expulsion of these by anthelmintics give any relief.

The disease may continue from one to five or more months, if antiphlogistic treatment, suggested by the early deceptive and transitory symptoms, does not hasten the fatal termination.

Diligent examination of the head and spine after death only exhibits slight venous meningeal congestion, the quantity of limpid serum not being anormally large. No lesion of the cerebral substance is observed, save in a few instances some bloody points upon slicing it. A much greater degree of venous congestion is usually found, when bleeding had not been practised, in the chest and abdomen. Where bleeding has been resorted to, the brain has been found in a state of complete anæmia. Dr. Dubini found in three cases (out of 30) softening of the thalami optici.

The author regrets that as regards remedial measures, he is obliged to confine himself to stating what he has found useless—the disease usually proving intractable; and the only two cases which did recover, did so in the employment of measures which in other cases had proved useless. As necroscopies do not, however, exhibit organic changes as an essential part of the disease, he hopes that one day a successful mode of treatment may be devised. *Antiphlogistics* are contra-indicated, and even the depression produced by a purgative or by the menstrual flux has aggravated the symptoms. Anthelmintics, large doses of Calomel, Oxide and Valerianate of Zinc, Opium, Strychnia, Quinine, Iron, and Belladonna have been in turn tried in various cases without satisfactary results. The best palliative of the violence of the convulsions seems to be *Extract of*

Hyoscyamus: and Dr. Rotondi reports three cases to the author in which a definite cure even seems to have resulted from the use of large doses of this substance.

Dr. Dubini enters at great length into the consideration of the differential diagnosis of this disease. He observes that violent muscular contractions characterise many diseases, but he has not been able to find the assemblage of symptoms just described in any work he has consulted. They may be thus recapitulated. Fear the common cause of the disease—the movements like those resulting from electricity—the same identical muscles affected, and all those resulted on the same side of the body becoming gradually implicated—the same muscles after undergoing these violent contractions fall into a state of temporary paralysis—apoplectic stupor supervening upon the frequent repetition of the convulsive paroxysms—the absence of lesions explicable of the symptoms, and the fatal termination. He presents his readers with detailed synoptical tables of the differential diagnosis of this disease and ordinary chorea, epilepsy, eclampsia, ergotism, saturnine convulsions, cerebro-spinal meningitis, and convulsions from organic disease of the brain. We have only space for the transcription of the first of these.

Chorea Gesticulatoria, or St. Vitus' Dance.	*Chorea Electrica.*
1. The disease is characterised by irregular partial or general movements, changing at every instant, and giving rise to involuntary and frequently ridiculous gestures. Of the two sides of the body the left is attacked in preference: so that, of 25 cases observed by Rufz, in only one were the extremities of the right side affected. The irregularity of the muscular contraction is the pathognomic sign.	1. The movements, strikingly resembling those resulting from electrical shocks, are always identical, constantly affecting the same muscles as at the commencement, gradually however implicating others of the same side also. Usually one side only affected, and that the right one. The identity of the muscular contractions is the pathognomic symptom.
2. The patient, at first of an active and correct frame of mind, may become fatuitous, or acquire a marked mobility of character, by reason of which he laughs or cries, &c. without apparent cause, or falls into a state of apathetic indifference, the result of debility of intellect.	2. The patient seems aware of the gravity of the disease, and has for the most part an invincible presentiment of its fatal termination. No change of character is observed.
3. The vague movements of chorea are not necessarily followed by paralysis. If the patient sometimes cannot govern his movements in walking, that is rather due to the irregularity of the muscular movements than to the existence of paralysis.	3. The muscular contractions are followed insensibly, and sometimes suddenly after the first paroxysm, by a paralysis of the affected parts.
4. The disease in the majority of cases terminates in a spontaneous cure, or it may be relieved by various remedies. If death does occur, it is always in consequence of an inter-current disease.	4. Almost all the cases terminate in death, in spite of the most varied means of treatment—so that it may be literally said that death is the rule, recovery the exception. Death, moreover, is due to the progress of the disease, which, by becoming more and more general, induces apoplectic stupor and death.

Annali Universali, Vol. 117, *pp.* 1-31.

TREATMENT OF FRACTURE OF THE CLAVICLE.

Two difficulties are met with in the treatment of this accident; the maintenance of the fragments of the bone immoveable, and the procuring a regular callus. Desault's bandage does not entirely fulfil these conditions, but M. Blandin obtains them by rendering this apparatus immoveable by means of dextrine. Six cases have just been treated by him at the Hôtel Dieu, and in one only, this patient being delirious from a wound of his head, was the success incomplete. In the others, when the bandage was removed, it was difficult to point out the side upon which the fracture had occurred, no angle or projection whatever being visible. It might be supposed that an immoveable bandage of this kind would prove very irksome to the patient. It does not: for, owing to the fragments being maintained *in situ*, he suffers less; and there is no necessity to renew or tighten the apparatus every few days, as would otherwise be the case. Care is of course taken to guard the armpits, and other parts which the stiff bandage would irritate, by means of compresses. In private practice, M. Blandin causes the patient to wear a somewhat tight flannel-waistcoat, over which he applies the bandages; and with the skin thus protected, the most delicate females experience no inconvenience from it. —*Gazette Medicale*, No. 33.

NITRATE OF SILVER IN TRAUMATIC ERYSIPELAS, ARTHRITIS, &c.

Erysipelas frequently shows itself in the St. Louis Hospital, and it is always treated locally (employing purgatives, &c. according to the indication) by M. Jobert with the *nitrate of silver ointment*, of varying strength, (viz. 4, 8, or 12 parts of the nitrate to 32 of the lard,) according to the degree of action desired to be obtained. He also uses this very advantageously in acute and chronic inflammations of the joints, bones, periosteum, and cellular tissue; violent arthrites, white-swellings, &c. The inunction quickly dissipates the inflammatory symptoms, and especially the pain, so as to be regarded by M. Jobert as one of the most powerful antiphlogistics. The application of the ointment blackens the skin. This becomes less stretched, wrinkles, and undergoes a kind of desquamation. The blackened epidermis is gradually detached, and the skin resumes its normal colour. When the inflammatory symptoms have disappeared, the disappearance of the blackening may be hastened by washing the skin with a solution of ioduret of potassium. In persons possessing a very delicate skin, slight pustulation, or a pungent pain of short duration, may be produced.—*Gazette des Hôpitaux*, No. 96.

In connexion with the above subject, we may mention that M. Robert employs *sulphuric acid* with great advantage in *chronic arthritis*. He dips a small pencil in the acid, and makes four or five linear superficial cauterizations. Such marked relief from pain results, that the patients themselves call for a repetition.

M. Hilsenburg has likewise employed the same means after local depletion with equal advantage. He however uses the following formula: *Acid. Sulph. Concent.* 8 *parts*, *Syrup* 15 *parts*. To be rubbed twice a day.—*Abeilie Medicale.*

M. VELPEAU ON FLEXIONS AND ENGORGEMENT OF THE UTERUS.

"A proof of how often the term *engorgement* has expressed an error of diagnosis is found in the fact that of late years, and in proportion to the progress of science, engorgements of the uterus become more and more rare, while the number of vicious flexions is augmented. I do not fear to state, that of 50 women reputed

to have uterine engorgement, 45 will be found upon examination to be suffering from some deviation from the normal position of the organ. How can we explain this error of diagnosis being committed by well-educated practitioners? The reason is simple. The woman is examined in the recumbent posture, and the finger meets, in a certain direction of the neck, forwards if there be anteflexion and backward if there be retroflexion, with a tumour of considerable size and sensible, which is declared to result from an engorgement. But the tumour is simply the body of the organ bent at a more or less obtuse angle, and sometimes at a right angle. We can easily assure ourselves of this, especially when, as is the case with most women who have borne children, the walls of the abdomen are neither tense nor thick. By gently depressing the hypogastrium, we may grasp the body of the womb between the two hands, and appreciate its volume as accurately as if we could see it. Engorgement is one of the least usual conditions of the organ that we meet with, and on the contrary the body of the organ is often found somewhat atrophied.

"Accurate diagnosis, in consequence of the treatment it designates, is here of great importance; for when we have to do with a simple deviation we dispense with the use of means proper for resolving a tumefaction which does not exist, of debilitating remedies each more mischievous than the other, and with confining the patient for months in the recumbent posture. We order for her, moderate exercise, a substantial and tonic regimen, antispasmodic, ferruginous drinks, saline baths, astringent vaginal injections, and lastly an abdominal bandage which may support the viscera and prevent their weighing down the deviated organ."—*Gazette des Hôpitaux*, No. 89.

ON PARALYSIS OF THE INSANE. BY M. BAILLARGER.

Incomplete general paralysis is an apyretic disease, generally of long duration, and principally characterised by embarrassment of speech, the *progressive* paralysis of the limbs, and dementia. Its exact frequency cannot be determined, as many patients who are the subjects of it are retained at home by their families; while it is also often overlooked by medical men, or confounded with diseases of the brain. About one-sixth of the patients admitted into the large asylums of France are affected with this paralysis. More than one-half the cases observed by Bayle were descendants of insane or highly nervous persons, and M. Calmeil believes that *hereditariness* exists in about a third. The influence of *sex* is great, for in the French asylums 1 man in 4 admissions is paralytic, while only 1 woman in 12 is so. The sanguine and plethoric *temperament* is most liable. Of 600 men, the medium *age* was 42, and of 400 women, 41; the disease very rarely occurring before 30. It is most frequently found in those classes of society which indulge in excess of drink, &c. Although we have found that insanity occurs with a third more frequency in summer than in any other season, the result is different as regards paralysis. Thus, of the 921 paralytic patients received at the Bicetre and Salpétrière during four years, nearly an equal number were admitted in the six cold as in the six hot months. Contrary to what occurs in simple insanity, the occasional causes are more often physical than moral. Of these, cerebral congestions, suppression of menses, or hæmorrhoidal discharge, excess of drink or of work, &c. are the most common. Sometimes the disease arises from organic cerebral affections; and, in exceptional cases, from anæmia and exhaustion.

Symptoms.—These are exhibited in lesions of motion, sensibility, and the understanding, each being exhibited in three different degrees. Of the *Lesions of*

Motion we have (1) *Embarrassments of speech.* These are sometimes scarcely to be observed, being remarkable chiefly by the effort and the suddenness with which the words are articulated. There is a characteristic trembling of the muscles around the mouth, and in a slight degree of the tongue itself. In a second degree there is more hesitation of speech, long intervals separating portions of a sentence, or even of a word, the faulty pronounciation becoming very confused if the patient is excited. In another degree the patient either cannot articulate at all or does so unintelligibly. (2) *The defective movements of the limbs* usually only manifest themselves after the above. The paralysis also observes different degrees, from a hesitation first in walking, and then a want of precision in employing the hand, both hardly to be detected, until the completest loss of power in all the limbs. (3) *Tremors of the limbs*, which occur in a slight degree in almost all these patients, sometimes prevail to a truly convulsive extent. A remarkable grinding of the teeth is sometimes heard, when no movement of the jaw can be detected. The *Lesions of Sensation*, very slight at first, gradually increase; so that eventually the skin entirely loses its sensibility, and the functions of the special senses become completely destroyed. *Lesions of the Intellect* in some patients consist of simple dementia without delirium, while others manifest the latter, which is, however, always, whatever may be its form, accompanied from the commencement with the marks of dementia. The ambitious form of monomania is the most frequent manifestation of delirium in these cases, but it, as well as the other forms, are maintained, owing to the loss of memory, with much less obstinacy and consistency than ordinary monomaniacs exhibit. The affective faculties become effaced, the patient witnessing the joys or sorrows of his friends with like perfect indifference. The *Nutritive functions* are at first satisfactorily executed, the appetite being, indeed, sometimes voracious. Afterwards this is lost and the patient wastes away. The stools are then passed involuntarily; and sometimes, before the sphincters become paralysed, female patients are only observed to befoul themselves at their menstrual periods.

Such is a general picture of the symptoms, and, as regards the order in their advent, I agree with Bayle, that a slight hesitation of speech is the first one, and by observing it I have several times been able to announce the supervention of general paralysis in persons who manifested no other. Next we observe a marked tendency to drowsiness, and then a slight dementia, which is first shown by a weakening of the memory. These persons can now no longer fulfil the duties of their station. They are not competent to even the most simple mental combinations, and incoherence is observed in their discourse. In some, a year or more prior to the accession of the disease, the genital faculties are lost. Among most of those menaced with this paralysis, slight tremors of the limbs, and cerebral congestions, inducing momentary loss of speech, are observed. In almost all cases the paralysis precedes the delirium; but, admitting that occasionally the reverse is the case, must we conclude that *incomplete general paralysis* is always a complication of insanity, which remains the primary and principal disease? We make the following distinction, which we believe an important one. If the lesions of the intellect have only preceded those of motion a short time, and consist only in an ambitious monomania, we must regard the delirium as simply symptomatic of the general paralysis. And we must regard this last as a complication of insanity only in the cases where it follows delirium of several years' duration, and which does not present the special form observed in the paralytic.

As to the *progress* of the disease, it is very irregular. Some patients are detained long at its early periods, and others pass at once to the second stage. Others again, apparently at death's door, recover power of motion surprisingly, only again to relapse. Its *mean duration* seems to be from 18 months to two years, life being more prolonged in females than males. Among the *complications*, epileptic paroxysms are the most frequent, and are often the instrument of the fatal termination. They are much more frequent in men than in women, who seem more liable to a semi-comatose condition. The *prognosis* is of the

gravest character. Among the *Anatomical changes* observed, the firm adhesion of the membranes to the more or less softened cortical layer of the brain is the most frequent, M. Calmeil having found it 28 times in 35 cases. In another group of cases, instead of meningeal alterations, collections of fluids are found in the cavities of the brain, while its substance is atrophied and hardened. However, the lesions observed are often very various, and the dependence of the disease upon any special one very unlikely. "Considering that among these alterations none is constant, and that each may be met with in other diseases, we agree with M. Calmeil that this affection cannot be explained by the meningo-encephalitis or the hydrocephalus, but that it is due to some identical modification, the intimate nature of which is unknown, a modification which must exist in all these cases, independently of the lesions described."

The *Diagnosis* is usually not difficult. The cases which may give rise to most embarrassment are, (1) *Delirium Tremens*, accompanied with ambitious ideas and embarrassed speech; but the cessation of the symptoms after a few days' treatment, will remove all doubt. (2) *Diseases of the superior portion of the spinal marrow.* Here the upper limbs are as soon or sooner affected than the lower. There is no embarrassment of speech or mental affection. (3) *Sudden diseases of the brain*, as hæmorrhages, &c., will be distinguished by an ordinary study of the symptoms. There is a class of diseases, which, in our opinion, are identical with general paralysis, and should not be separated from it, viz., the paralysis of the aged, that resulting from the chronic hydrocephalus of adults, or the hydrocephalus following cerebral alterations.

Treatment is of no avail, except in the earliest period, and rarely then. If the patient be strong, local, or even small general, bleeding may be advantageously resorted to; employing at the same time purgatives, revulsives, and exutories. In the second period, leeches may be sometimes used, but we must especially rely upon setons behind the neck. By these means we may frequently delay the progress of the disease, and can rarely do more.—*Gazette des Hôpitaux*, Nos. 80, 83.

In reference to this interesting disease a short discussion took place not long since at the Society of Practical Medicine of Paris. M. *Brierre de Boismont* observed that, in consequence of its having been stated by some medical men that general paralysis may exist without insanity, he had made researches respecting it in all the large hospitals, and arrived at the following conclusions. 1. General paralysis without mental alienation is excessively rare, since, in examining more than 1500 patients only one distinct instance was discoverable, while such cases constitute about a fourth of the inhabitants of asylums. 2. The paralysis of the dirty patients of the Salpétrière is not identical with this disease. Such patients are all far advanced in age, while general paralysis attacks individuals in the flower of their age, and kills them in a very few years. There is not observed among them, which is an important characteristic so often seen in the insane, those temporary recoveries of power, when even just upon the eve of death. The general paralysis then of the old females of the Salpétrière is only a general enfeeblement of the nervous system, brought on in the progress of age. M. *Colineau* observed that he had seen all the symptoms of general paralysis without insanity, there being simply a weakness of the intellect. In one case, the individual retained his faculties unimpaired to the last. M. *Baillarger* stated that cases of this paralysis are to be met with in the Salpétrière, accompanied by dementia, but without mental alienation, and that he is occupied in preparing a work distinguishing general paralysis from mental alienation. He thinks *progressive paralysis* would be a better denomination for the disease than paralysis of the insane.—*Gazette Medico-Chirurgicale*, No. 24.

CANCER.

Opium Dressing.—M. Tanchou speaks highly of the relief from pain to be obtained from employing the following dressing in cancerous ulcers. Digest, during 24 hours, at a temperature of about 78°, a certain quantity of rough-powdered or bruised opium in a sufficient quantity of water to form a thick paste. Cover the ulcer with this, a line or two in thickness, once or twice a day according to the severity of the pain, and place over it a piece of thin gummed paper or court-plaister to prevent evaporation.

Relapse of Cancer.—True as it is that relapse is much to be feared after operation for cancer, it is no less so that we may sometimes mistake the effects of inflammation for such. M. *Lisfranc* has made the part which inflammation acts in cancer the subject of very attentive study. A woman had her left breast removed recently. Cicatrization at first took place rapidly; when, all of a sudden, the wound broke out again, the surrounding skin assuming a slate-colour, and lancinating pains reappearing. Here was every appearance of a relapse; but M. Louis ordered a dozen leeches to the margin of the wound, the pains ceased, the slate-colour disappeared, and cicatrization was soon completed.—*Gazette des Hôpitaux.*

ERGOTINE AS A HÆMOSTATIC.

On the occasion of M. *Bonjean's* presenting the *Academie des Sciences* with an account of an additional experiment he has made with *Ergotine*, in which the bleeding from the carotid of a horse, divided through a third of its circumference, was at once arrested by the application of ergotine, M. *Velpeau* delivered the following sensible and pertinent observations:—

"What M. Bonjean says of Ergotine has been said by an infinity of other persons concerning different substances. Hæmostatic means of a *real* efficacy are nevertheless as rare as ever. The error arises from these authors having forgotten two things in their experiments. 1. In animals, the plasticity of the blood is much greater than in man, whence it follows that means which will arrest hæmorrhage in the one, may easily fail to do so in the other. All those who have made experiments on living animals know that, in the horse, the ox, the sheep, for example, the largest wounds of arteries rarely give rise to mortal hæmorrhage. The blood, ceasing to flow almost of its own accord, leads the public and inexperienced authors to believe that it is the *means* or *remedy* employed which has closed the artery. Thus, what powders, waters, liquids, what arcana of every kind have been vaunted at first as infallible; and then, after a searching examination, rejected as useless! 2. In man, many arterial hæmorrhages also cease either spontaneously or under the exertion of mere compression, without our being obliged to have recourse to the ligature; so that it is easy to attribute to a pretended hæmostatic substance a result which takes place quite independently of its employment.

"I have neither cause nor desire to throw any doubt upon the value of M. Bonjean's experiments; but practice has been so often deceived by similar announcements, that it behoves the Academy to accept them with due reserve. I must add, that the practitioners who have tried *ergotine* or the *ergot of rye* have as yet derived nothing conclusive from its use. When, in *uterine hæmorrhage*, the ergot proves useful, it does so by inducing contraction of the uterus, and not by any special action it exerts on the blood or on the arteries. Thus we see the question of surgical hæmostatics is at once a very complex and a delicate one: and we should not receive facts concerning it without a certain degree of distrust,

and only give them a very limited publicity, until they have been tested by a more mature examination."—*Comptes Rendus*, 6th July.

M. RICORD ON THE TREATMENT OF GONORRHŒA.

The Abortive Treatment.—As long as there are no acute symptoms, such as pain in urining or during erection, &c. we have only to do with a mere modification of the surface. The passage of the urine only occasions a slight irritation, and as long as the disease has not proceeded farther than this, whatever be its duration, we may still employ the abortive treatment. This is important, for we find practitioners who state that the abortive treatment has not succeeded in their hands, although resorted to within the first twenty-four hours. Time has nothing to do with it; and you will sometimes meet with pneumonias, which in less than twenty-four hours have arrived at their third stage. The *Nitrate of Silver*, used either in substance or solution, is the most powerful modifier of the mucous surfaces we possess. It is not a panacea; but when we have seen the patient early enough, and have been able to ascertain the exact seat of the urethral inflammation, we have frequently applied the substance by means of Lallemand's *porte-caustique* with great success. We give preference however to strong *injections* of this substance, which employed at an early stage, are admirable means. After making the patient pass water and gently squeezing the last drops from the urethra, we must throw in the injection by means of a glass syringe, our object being to make it quickly traverse the entire length of the urethra. If the patient is allowed to slowly inject it, the mucous membrane puckers up, the canal is narrowed, and the fluid does not pass. We should allow it to remain in the urethra about half-a-minute before it flows out. Severe pain in the part is now felt, as if the canal contained pins and needles. We should advertise our patient (only after we have thrown in the injection though) that there will be a temporary increase of pain and discharge, difficulty of urining, together with a more or less abundant exhalation of blood. Such augmentation of the mucoso-purulent discharge may continue for from six to ten hours, the pain however usually ceasing at the end of two or three.

To the severe pains succeeds a complete collapse. The urine is passed with ease, and the discharge sometimes quite dried up. Occasionally the patient is thus cured at once, but unless he observes for a while the greatest precautions, the discharge in a day or two will re-appear. As long as it does not we must rest content; and if it does we employ a second injection, and sometimes are obliged to have recourse to a third one. But, generally, the discharge has become so slight, thin, and mucous, that mere *astringent* injections suffice. However, my experience proves that caustic injections employed, unaided by internal medicines, do not furnish complete results, although I have never seen the ill consequences described by some as resulting from their employment. Employed alone, then, I reject them: but used in connexion with internal medicines, they constitute an admirable method, and are, without any doubt whatever, the best means we possess for cutting short the progress of this disease.

Hygienic Treatment.—If you wish to derive all the advantages possible from the use of these injections simultaneously with the administration of copaiba or cubebs, the hygiene of the patient must be carefully attended to. His diet should be farinaceous, and his drink spare in quantity, so that the urethra may be irritated as little as possible by frequent urining. Tisans must be forbidden if we wish the abortive treatment to succeed; but, when the patient will not be satisfied without them, linseed-tea forms one of the best. Exercise must be taken as seldom as possible, and all thoughts, books, &c. calculated to excite erections,

avoided. Intellectual pursuits have at such times a useful effect in driving away lascivious thoughts. A substance I can recommend as an *anaphrodisiac*, acting as powerfully on the genital organs as belladonna does upon the iris, is *Camphor*, and this is the manner in which I employ it. R. *Camphor*, *Thrydace* āā 50 *gr. M. and divide in pil.* 20. Five or six of these are to be taken daily, especially in the evening. For those who cannot take pills, I order 12 grains suspended by yelk of egg, as a *lavement*. Some persons, as I did myself some years since, add opium to the camphor, but this only destroys its sedative effects.

Internal, indirect, or revulsive medication.—The most efficacious of medicines are copaiba and cubebs, and after them, but in a far less degree, turpentine and balsams. How do these act? Revulsively sometimes; for the discharge has been evidently arrested after purgation by copaiba or cubebs; but, as Cullerier justly stated, such a cure is not permanent. The specific, not the purgative action, of copaiba is that which we must seek for. The action of such substances really takes place upon the urinary passages, through which they are eliminated from the system. What best proves that it is by the medicinal urine, loaded with the active principles of these medicines, the disease is cured, is, that it is especially in the cases in which the substance imparts its odour to that fluid that cures take place. I had a patient in whose urethra, affected with discharge throughout its length, was an aperture, dividing it into an outward and posterior portion. Copaiba was given him which cured the discharge in that part of the canal only over which the urine was enabled to flow. Yet we cannot obtain a cure by injecting copaiba or cubebs, but may indeed aggravate the case. There is a change operated by the living chemistry of the body which imparts to the aromatic principle diffused in the urine its curative properties.

Copaiba in some patients soon produces nausea and vomiting, and in others purging. In some, again, it induces an erythematous eruption of the skin, usually but of short duration. In a few cases, in which its odour is not perceived in the urine, great disturbance of the nervous centres is observed. *Cubebs* is much more easily tolerated than copaiba, and much more rarely induces ill-consequences similar to those now mentioned. Whichever substance is employed it must be given from the very first in *sufficient doses*, so that the economy may not accustom itself to its presence too easily. For *Copaiba*, the ordinary dose at first, should be 4 drachms per diem, divided into two or three portions. As much as twice or three times this quantity may sometimes be required. Of *Cubebs* the medicinal doses which I prefer, range from 6 to 10 drs. per diem. As long as the dose seems curative we must be content with it, if it do not disagree with the patient, when it should be diminished. It must be continued some time after the discharge has subsided. As to the pharmaceutical forms of these medicines, the less they are modified the more powerful is their action. Cubebs especially should be taken in as nearly its natural state as possible. Few persons, however, can take copaiba in this manner, and the following is the modification of *Chopart's Mixture*, which I have found most beneficial. *Balsam Copaib: Mint Water: Lettuce Water: Orange-flower Water: Syrup of Poppies, of each equal parts. Make into an Emulsion with Tragacanth Powder.* Three spoonfuls are first to be taken daily, and the number increased, as the tolerance becomes established, until 10 to 12 are taken. A little Seltzer water after each dose corrects the tendency of the medicine to rise; but few patients are able to continue it for long. For those who cannot take copaiba by the mouth, it must be given by the rectum; but, so administered, it is a most uncertain remedy, and should never be resorted to, unless no other means present themselves. The following is the formula. *Balsam*, 6 *or* 7 *drachms: Decoction of Poppies*, ℥ iij. *Yelk of Egg* 1. M. The rectum must be previously emptied by an ordinary enema, and then this one administered almost cold. It should be given at bed-time, and the patient must endeavour to retain it all night: to aid him in doing which, a little opium and

camphor may be added to the enema. The introduction of *capsules* is one of the triumphs of pharmacy. I prefer Raquin's gluten capsules to those formed of gelatine, the former containing a little magnesia. I have endeavoured to prepare similar ones less expensively. Thirty parts of Copaiba are solidified with 1½, of magnesia, and then covered with gelatine. With *cubebs* we usually associate a little extract of *rhatany* or frequently powdered *alum* (*i. e.* 2 parts of alum to 30 cubebs). In some lymphatic or chlorotic-looking subjects we combine carbonate of *iron* with the cubebs. The patient's drinks should be in small quantity. Some Seltzer water with a little syrup of oranges, or some infusion of uva-ursi, with a little citrate of iron (6 or 8 parts to 500) and syrup of Tolu, are the best. Cubebs and Copaibá are often given together. I employ them separately: so that if either of these medicines fail me I have recourse to the other; but yet in a few cases where copaiba cannot be supported alone the mixture of the two is tolerated.

The Acute Stage.—Suppose these means have not succeeded in cutting short the disease, and the gonorrhœa is completely established. We must insist upon hygienic precautions, order abundant and diluent drinks, and warm baths (about 92°). These last may be employed for an hour; but for less time, or not at all, in susceptible patients in whom they sometimes induce erections. Hip-baths are to be prohibited entirely, as always hurtful. Saline purgatives and enemata, the use of the suspensory, hard cool beds, rigid diet, and the camphor pills, are to be ordered. These means will usually carry the patient through the acute stage, but sometimes powerful antiphlogistics may be required. When leeches are employed, they must be always applied to the perineum, not to the penis. Some practitioners employ caustic injections and copaiba in this acute stage, and they may have met with some cures. In the great majority of cases however they fail, and thus discredit an useful means when judiciously used. I forbid injections of any kind whatever at this period; so, too, in regard to copaiba, &c., it usually does harm or is useless, only disgusting the patient and preventing his taking it at a more appropriate period. There are however, certain exceptional cases in which the pain in urining will yield to nothing but copaiba, which then becomes a true sedative. When the antiphlogistics have been discontinued, and the discharge still continues, we may then cautiously have recourse to this substance or the cubebs.

Among the *accidents of the acute stage* we may mention *dysuria*, which, as it is only one stage removed from retention of urine, must be carefully watched. We should employ antiphlogistic means still more energetically, and direct the patient to allow his water to pass while the penis is kept under tepid water. In acute *retention* local or general bleeding at once often affords relief. An enema formed of three ounces of *cold* decoction of poppies and a few drops of laudanum may have the same effect. If these means fail, we must place the patient in a bath at very low temperature; but here precautions are required; for if the bath does not cause the patient to urine, it augments the retention. If, however, twenty-four hours have elapsed without relief, we must not longer delay the use of the catheter; for the longer we now wait, the more difficult and painful will its introduction prove. Whether the instrument is to be retained or not, will depend upon the duration of the retention and the difficulty of introducing it. Sometimes the retention is produced by the patient restraining himself too long from urining, in dread of the pain this will cause him. The complication of *bubo* and *urethral abscesses* is a simple inflammatory accident, to be met by antiphlogistic measures. They should be opened at the earliest period that suppuration is manifested. *Chordee* is best treated by large doses of camphor, as are also *urethral hæmorrhages*. These last, in a moderate degree, are useful by the local depletion they induce; and when in excess they are best restrained by the external application of ice and slight compression, or by means of cold urethral injections. *Inflammation of the neck of the bladder* is one of the most tormenting

affections both for patient and practitioner. The antiphlogistic measures must be vigorously pursued, but they often fail to give relief. A small cold enema, containing a little laudanum, given twice a day, will often relieve the pain in a manner that nothing else can.

Chronic Stage.—In the great majority of cases the disease is not cured upon the disappearance of the inflammatory symptoms. As soon as these have lessened we must adopt less relaxing methods. Drinks must be more restricted, and baths left off, as nothing reproduces or prolongs the affection more than an incautious continuance of these last. The camphor must be continued, and copaiba and cubebs resorted to. The use of irritating injections must not be had recourse to, until four or five days after using the balsam; but if the disease then continues stationary, we may have recourse to them. If the inflammatory action, as manifested by pain, has entirely subsided, strong injections of nitrate of silver will suit very well; but this is not usually the case, and the following is that which I then prefer. *Rose Water* 200 *parts*, *Sulphate of Zinc* 1 *part*, *Fluid Subacetate of Lead* 1 *part*. Unchemical as it is, it answers exceedingly well. Three times in the 24 hours is quite frequent enough for its use, the patient urining just before employing it. In this way you will cure 98 out of every 100 gonorrhœas. In the few cases which resist we should study the causes of the persistence of the discharge, and apply appropriate means. Sometimes the following vinous injection succeeds in these cases:—*Bordeaux or Roussilon* 150 *parts*, *Rose Water* 50 *parts*, *Tannin* 1 *or* 2 *parts*. To this a litle *alum* may be sometimes advantageously added. Another excellent injection in quite chronic cases is formed by the *iodide of iron*. *Distilled Water* 200 *parts*, *Iodide of Iron* $\frac{1}{10}$*th* or $\frac{1}{5}$*th part*. I do not approve of the *bichloride of mercury*, for, if irritating injections are required, the *nitrate of silver* is the best. The former indurates the surface, and risks the production of stricture. When the discharge resists the strong nitrate injection, it will sometimes yield to a very weak one ($\frac{1}{10}$ part to 200 parts). In some patients, however weak the injection used, inflammation and increase of discharge result. The patient in such a case should be left alone for a few days, when the discharge will often be found to have ceased.

The treatment of *gleet* or *military gout* is very difficult, as no disease is more obstinate. Its obstinacy generally arises from change of structure, and whenever these patients present themselves, you should *always at once examine the condition of the urethra*, or you may be afterwards accused of producing strictures which already existed when the patient first consulted you. If the canal is free you should try the copaiba and cubebs, and then dried turpentine pills, (from 1 to 2 drachms per diem), giving at the same time decoction of *uva ursi* and syrup of tolu as a drink. Sometimes, astringents and ferruginous preparations are useful; and at other times cauterization of the urethra. Bougies, medicated or otherwise, have often effected a cure. Blistering the perineum or thighs is sometimes of great service. Cold-bathing is another means which may be tried. More frequently than is believed, the persistence of the discharge depends upon prolonged continence; and it is then cured by coition.—*Gazette des Hôpitaux*—No. 82.

M. GINTRAC ON DIAGNOSIS OF CHLOROSIS.

M. Gintrac's object is to recognize a condition of the economy, characterised principally by paleness of skin, feebleness, and palpitations, but yet quite distinct from that of true chlorosis, and intimately connected with irritation of the digestive canal. The pallor of the skin is of a dead-white instead of the yellowish-green of that disease; and the debility and palpitations are less marked. The

bruit de souffle too are less constant. The abdomen is found to be tender on pressure, and the digestive functions are entirely disordered—there being loss of or depravation of appetite, nausea, eructations, constipation or diarrhœa, and sometimes hysterical symptoms. The tongue may be pale, but at other times it is partially red, either at its tip, or in the middle. There is almost always amenorrhœa. M. Gintrac regards these symptoms referrible to the digestive organs as not resulting from pure inflammatory action, but from a complex state of inflammatory irritation and nervous hypersthenia. Preparations of iron and other anti-chlorotic remedies usually aggravate it, while it yields to those of an antiphlogistic and calming character; such as tisanes, milk, infusions of poppies, baths, laxative and emollient enemata, and cupping-glasses to the abdomen. When the symptoms of irritation have ceased, we may resort to tonics and even to iron: but the symptoms then frequently disappear of their own accord.—*Gazette Medicale*, No. 33.

[The distinction here pointed out is an important one: for there can be no doubt that tonics, and especially steel, are frequently hurtful in consequence of sufficient attention not being paid to the removal of a condition of irritation of the digestive organs.—*Rev.*]

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THE HARVEIAN ORATION, delivered before the Royal College of Physicians, London, June 1846. By *John Elliotson*, M.D. F.R.S., &c. With an English Version and Notes. 8vo. pp. 70. London. Baillere.

THE greater part of this oration is occupied with bald and profitless tales about Linacre, Caius, and other old fellows of the College, and with a dreary account of the disputes about the circulation of the blood in Harvey's time. The narrative is, however, enlivened with random remarks, some of which are really very amusing. For example, four of the silly opposers of Harvey's views are thus summarily consigned to rest:—*requiescunt in pace Simon Boullotius cum Hugone Chaslesio; Franciscus quoque Bazin cum Philippo Hardouino suo,*

" —— not a pinch of dust remains of Cheops."

Sanguis autem illum suum "motum circularem" etiam nunc improbus tenet. The classicality of the Latin (*illum suum* is surely Elliotsonian) is strangely set off by the English quotation. By the bye, Sir Astley Cooper is made, *nolens volens*, to speak in Latin—a feat which, we dare say, the worthy Baronet could not well do in life: *solebat in prælectionibus chirurgicis jactantius aliquanto quotannis affirmare, "se, Deo gratias, nihil de medicinâ scire;" quanquam ipse compluria millia sterlingorum singulis annis faceret, quum iis morbis, qui omnino medicorum essent, præscriberet.*

Here is a curious phrenological conjecture: "Harvey's brain must have been of superior composition and organization to those of Linacre and Caius, for he was educated precisely as they had been, was an excellent classic, writing Latin fluently and eloquently, and enjoying the ancient poets to absolute rapture, and well versed in Aristotle and Galen. Yet he found no pleasure in books as books, but only as the depositaries of truth and genius, and therefore never rested without testing the contents of scientific works, however reverenced among the learned, by a reference to nature herself, of whom they profess to be the delineators."

Dr. Elliotson is justly indignant that no public honour was conferred upon Harvey during his life-time; in this respect he was "like the other greatest names in our profession—Glisson, Sydenham, Jenner, John and William Hunter. Indeed the majority of really great British medical men have received no titles; and the majority of those who have received them have been distinguished for nothing but good luck, and often but for little even of that."

The allusion to Mesmerism is made with all the coyness of a shame-faced lover. Dr. E. will not speak out the name of his inamorata; he only leaves us to guess who she is in the following passage, which forms the peroration of his speech.

"A body of facts is presented to us not only wonderful in physiology and pathology, but of the very highest importance in the prevention of suffering under the hands of the surgeon and in the cure of disease. The chief phenomena are indisputable: authors of all periods record them, and we all ourselves witness them, some rarely, some every day. The point to be determined is whether they may be produced artificially and subjected to our control: and it can be determined by experience only. The loss of common feeling,—anæsthesia, is but a form of palsy, and in it wounds give no pain. If this condition can be induced temporarily by art, we of necessity enable persons to undergo surgical operations without suffering. Whether the artificial productions of those phenomena, or the performance of the processes which so often induce them, will mitigate or cure disease, can likewise be determined by experience only. It is the imperative, the solemn, duty of the profession, anxiously and dispassionately to determine these points by experiment, each man for himself. I have done so for ten years, and fearlessly declare that the phenomena the prevention of pain under surgical operations, the production of repose and comfort in disease, and the cure of many diseases, even after the failure of all ordinary means, are true. In the name, therefore, of the love of truth, in the name of the dignity of our profession, in the name of the good of all mankind, I implore you carefully to investigate this important subject."

We regret that Dr. E. has most injudiciously made this Harveian Oration the medium of again proclaiming to the world his belief in the materialism of the soul. Would that he did but follow his own advice and become as a little child in the study of those momentous truths, the right appreciation of which might have kept him from the unfortunate delusions which have beset his path!

MESMERISM IN INDIA, AND ITS PRACTICAL APPLICATION IN SURGERY AND MEDICINE. By *James Esdaile*, M.D., Civil Assistant-Surgeon H.C.S., Bengal. 12mo, pp. 286. Longman, 1846.

WE have in a preceding article (p. 506) stated the reason why any amount of curative power which mesmerism may possess is not available to the profession; and we trust we shall not be deemed inconsistent in stating a case in which it may be resorted to. Most will acknowledge that, in certain individuals, a more or less prolonged insensibility may be induced by mesmeric procedures, and it is obvious, that the fact of this being so complete as to admit of the performance of surgical operations during its existence, and of surgeons availing themselves of such suspension of sensibility, says nothing *pro* or *con* concerning the remedial agency or the ultra pretensions of mesmerism. The same state has been induced by other means, such as injuries to the head, narcotics and intoxication, during which unfelt operations have been completed. The questions then we have to decide are whether mesmerism can induce such sufficient insensibility, and whether the patient's eventually well-doing is at all compromised by the operation being undertaken under such circumstances. Dr. Esdaile adduces 73 instances of painless operations of varying degree of severity, from the introduction of a

seton to the amputation of an arm or the tearing of a toe-nail, performed upon subjects who could have no inducement, and scarcely intelligence enough, to enter into any plan of collusion, in the presence of very numerous witnesses. Without agreeing with the author in the general favorable estimate he gives of Mesmerism, we may state that we believe the cases we have alluded to are entitled to our belief, and that the subject is one of such vast importance as to call for a searching investigation by the authorities at home. This claim of Mesmerism admits of easy testing, and we repeat if its validity is proved, that of the various monstrous pretensions of this art will not be brought in anywise nearer admission, while a vast boon will have been secured to suffering mankind.

I. FEVER PHYSIOLOGICALLY CONSIDERED. By *David M'Connell Reed*, Esq. Octavo, pp. 260. Churchill, 1846.
II. ANNUAL REPORT OF THE CORK FEVER HOSPITAL FOR THE YEAR 1845.
III. MEDICAL REPORT OF THE HOUSE OF RECOVERY AND FEVER HOSPITAL, Cork Street, Dublin. By *George Kennedy*, M.D. M.R.I.A. Dublin, 1846.

DR. REED'S work conveys little useful information, that we can discover, upon the inexhaustible and interesting subject of Fever. He denies the existence of essential or idiopathic fevers, maintaining that all the symptoms are referrible to the deranged condition of certain organs, and according as they emanate from one or other of these we have Pulmonary, Cerebral, Gastric, Pleuritic, Fever, &c. The author, in the course of his work, more than once offers up hearty thanks to God for the completion of various portions of it: and as we had to read page after page from such very accessible authorities as Hooper and Thomas, we felt no less grateful when the perusal was completed.

The other two publications relate to Fever in Ireland; a subject fraught with interest when famine is impending over that unhappy land. We have nothing to do with politics in this Journal, but we should be wanting in proper feeling if we did not offer our humble tribute of approbation to the earnest endeavours the late and present ministry have employed to ward off Fever, that desolating scourge of Ireland, by a due supply of food. It was a noble reply of Sir Robert Peel, when taunted with being too precipitate (alas! who will *now* say so?) in his arrangements for this purpose—"Am I to wait and see how much flux, diarrhœa and fever a people can bear before ministering to their necessities?"

In the Report of the Cork Epidemic we find the Reporter quoting Hippocrates to the Governors, in illustration of *a green appearance of the tongue*, noticed by the Greek Physician, but not by the moderns until Dr. Beamish published an account of it in the *3rd vol. of the Trans. Coll. Phys., Ireland.* Dr. B. has noticed it altogether in 86 cases, 24 of which have occurred during this epidemic. The prognosis in such cases is however favourable, contrary to the statement of Hippocrates, for 83 of the patients recovered.

Dr. Kennedy's Report is a very able one, containing much valuable information on the various forms of Fever, but too condensed to admit of analysis.

ON THE ANTIDOTAL TREATMENT OF THE EPIDEMIC CHOLERA. By *John Parkin*, M.D., &c. Octavo, pp. 60. London, 1846.

OUR only motive for noticing this pamphlet is to protest against the very improper use which the author has made of the Medico-Chirurgical Review to puff off his

strange production. We were not a little surprised to observe that the advertisements in the newspapers, and elsewhere, of this most uncalled-for new edition of the "Antidotal Treatment" were backed with the approval of this Journal; thus: "Carbonic acid is an antidote."—*Med. Chir. Review.*

The notice from which this disconnected scrap is so adroitly taken was written by the late Dr. Johnson, in his usual playful mood, when disposed to ridicule some absurd work that came under his notice. So it was on the present occasion, as the following passage will show:—

"Mr. Parkin is satisfied that carbon or carbonic acid is an antidote for the poison of cholera. He has told the world so several times already; but as it won't believe him, he has written a book. If that fails to convince, he must adopt the expedient of my uncle Toby, and tell this generation to 'go and be d——.'"—*Med. Chir. Review*, No. 50, Oct. 1836.

We trust that Dr. P. will not repeat his scarcely honest procedure.

OBSERVATIONS ON THE EDINBURGH PHARMACOPŒIA, AND ON THE DISPENSATORIES OF DR. CHRISTISON AND DR. A. T. THOMSON; to which are added Illustrations of the Present State of Pharmacy in England. By *Richard Phillips*, F.R.S. L. & E. &c. London: Highley, 1846.

IN a recent number of the British Quarterly, it was well observed, that men of science might be divided into two classes—those who seek after truth, and those who avoid error. So also may literary men be classified, and form two as distinct orders—those who write, and those who comment on what is written; the one being fully as useful as the other. The work now under consideration comes under the latter class, and is well calculated to uphold the value and importance of it.

It is evidently "a work of love," as nothing else could compensate for the great labour and time bestowed on such a careful analysis of the works reviewed. Indeed there are few men so competent as Mr. Phillips for the task he has undertaken, and, notwithstanding he has the reputation of using the knife boldly, still he rarely comes into contact with the sound parts, though at times cutting very close to them. Were we disposed to criticise as minutely as he has done in a few instances, we might perhaps place our finger on a tender place or two.

We have gone through the objections he takes, and several of the calculations he makes in support of them, and have found them to be perfectly correct. The errors which the author has exposed are of various degrees of importance, and some of them exhibit a degree of carelessness which is almost incredible. Had there been space, we should have adduced an example or two.

In the latter part of the Pamphlet there are some very forcible "Illustrations of the present State of Pharmacy in England," which is a matter more directly concerning the public than even the great errors of the works reviewed. We fear much that the points selected for illustration are but very few compared with the mass which would present itself were the enquiry continued. No doubt many err through ignorance, and many through knavery—the former should be removed, and the latter punished. If the Charter of the Pharmaceutical Society is based upon the principle of reform and improvement in its members, they must not tarry on their road; for verily, even by this sketch of their failings, they have some heavy work cut out for them. Our author says he only leaves his post for a season, with the promise to return to it if necessary when sufficient time may be supposed to have elapsed for remedying the existing delinquencies, whether arising from carelessness, ignorance, or fraud.

A MEDICAL TOPOGRAPHY OF TUNBRIDGE WELLS. By *Robert Hutchinson Powel*, M.B., M.D. 12mo, pp. 176. Tunbridge, 1846.

IT is well that medical practitioners should bear in mind that the railway brings within an easy distance a very efficacious chalybeate. There is a large class of cases which this is likely to benefit. Dr. Powel has prepared an interesting little work, describing the locality, detailing opinions upon the curative powers of the waters, and indicating the cases likely to be benefitted by their use. As his work is intended as much for the public as the profession, we think he enters into more medical details than are desirable. It is strange that a spot, possessing so many natural advantages, should have been so often comparatively neglected, and we hope this little *brochure* will have the effect of calling attention to it. We have, however, a question to put to Dr. Powel, namely, how comes it that children, who have been once to Tunbridge, have so frequently a marked objection to return? It is an interest that must be conciliated.

QUAIN ON THE ARTERIES.

OUR readers will observe, among the Advertisements, the announcement of a great reduction in the price of this valuable work. It is an honour to the age and country, and we are much pleased to find it thus brought within the means of private individuals, as well as public institutions.

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